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TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

EDINBURGH.

VOL. XXI.

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THE KEITH, BRISBANE AND NEILL PRIZES.

The above Prizes will be awarded by the Council in the following manner:—

KEITH PRIZE.

The KEITH PRIZE, consisting of a Gold Medal and from £40 to £50 in Money, will be awarded early next Session (1857–8), for "the best communication on a scientific subject, communicated, in the first instance, to the Royal Society during the Sessions 1855–6 and 1856–7." Preference will be given to a paper containing a discovery.

MAKDOUGALL BRISBANE PRIZE.

This Prize, consisting of a Gold Medal and a sum of Money, will be awarded before the close of the Session 1858–9, under the following conditions:—

1. Competing Essays are to be addressed to the Secretary of the Society on or before 1st February 1859.

2. The competition is open to all men of science.

3. The Essays may be either anonymous or otherwise. In the former case, they must be distinguished by mottoes, with corresponding sealed billets superscribed with the same motto, and containing the name of the Author.

4. The subject proposed by the Council for the Prize of 1856–57 is the following:—

A BIOGRAPHICAL NOTICE OF A SCOTCHMAN EMINENT IN SCIENCE; including an estimate of the influence and importance of his writings and discoveries. As instances of such Biographies which still remain to be supplied, the Council would specify the following names: Sir ROBERT SIBBALD, Sir ANDREW BALFOUR, MACLAURIN, BLACK, MONRO *Primus* and *Secundus;* several of the family of GREGORY, Sir JAMES HALL, JAMESON. The earlier volumes of the Transactions

of the Royal Society contain several specimens of able Biographies of the kind here referred to. The Council are anxious to see a continuation of the series.

5. The Council impose no restriction as to the length of the Essays, which may be, at the discretion of the Council, read at the Ordinary Meetings of the Society. They wish also to leave the property and free disposal of the manuscripts to the Authors; a copy, however, being deposited in the archives of the Society, unless the Paper shall be published in the Transactions.

NEILL PRIZE.

The Council of the Royal Society of Edinburgh having received the bequest of the late Dr Patrick Neill of the sum of £500, for the purpose of "the interest thereof being applied in furnishing a Medal or other reward every second or third year to any distinguished Scottish Naturalist, according as such Medal or reward shall be voted by the Council of the said Society," hereby intimate,

1. That the First Neill Prize, consisting of a Gold Medal and a sum of Money, will be awarded before the close of the Session 1858–9.

2. The Prize will be given for a Paper of distinguished merit, on a subject of Natural History, by a Scottish Naturalist, which shall have been presented to the Society during the three years preceding the 1st February 1859,—or failing presentation of a Paper sufficiently meritorious, it will be awarded for a work or publication by some distinguished Scottish Naturalist, on some branch of Natural History, bearing date within five years of the time of award.

AWARDS OF THE KEITH PRIZE SINCE 1845.

(*Continued from Transactions, Vol. XVI., page* iii.)

10TH BIENNIAL PERIOD,		1845–47.	General Sir THOMAS BRISBANE, Bart., for the Makerstoun Observations on Magnetic Phenomena, made at his expense, and published in the Society's Transactions.*
11TH	Do.	1847–49.	Not awarded.
12TH	Do.	1849–51.	Professor KELLAND, for his papers on General Differentiation, including his more recent communication on a process of the Differential Calculus, and its application to the solution of certain Differential Equations.
13TH	Do.	1851–53.	W. J. MACQUORN RANKINE, Esq., for his series of papers on the Mechanical Action of Heat.
14TH	Do.	1853–55.	Dr THOMAS ANDERSON, for his papers on the Crystalline Constituents of Opium, and on the Products of the Destructive Distillation of Animal Substances.

* A Silver Medal, bearing the head of Napier on one side, and a suitable inscription on the other, was at the same time awarded to Mr JOHN ALLAN BROUN, under whose immediate direction these Observations were made.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH.

AS REVISED 5TH JANUARY 1857.

LAWS.

[By the Charter of the Society, (printed in the *Transactions*, Vol. VI., p. 5,) the Laws cannot be altered, except at a Meeting held one month after that at which the Motion for alteration shall have been proposed.]

I.

THE ROYAL SOCIETY OF EDINBURGH shall consist of Ordinary and Honorary Fellows. Title.

II.

Every Ordinary Fellow, within three months after his election, shall pay Two Guineas as the fee of admission, and Three Guineas as his contribution for the Session in which he has been elected; and annually at the commencement of every Session, Three Guineas into the hands of the Treasurer. This annual contribution shall continue for ten years after his admission, and it shall be limited to Two Guineas for fifteen years thereafter.* The Fees of Ordinary Fellows residing in Scotland.

III.

All Fellows who shall have paid Twenty-five years' annual contribution shall be exempt from farther payment. Payment to cease after 25 years.

IV.

The fees of admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual annual contribution of £3, 3s., payable by each Resident Fellow; but after payment of such annual contribution for eight years, he shall be exempt from any farther payment. In the case of any Resident Fellow ceasing to reside in Scot- Fees of Non-Resident Ordinary Fellows. Case of Fellows becoming Non-Resident.

* At the Meeting of the Society, on the 5th January 1857, when the reduction of the Contributions from £3, 3s., to £2, 2s., from the 11th to the 25th year of membership, was adopted, it was resolved that the existing Members shall share in this reduction, so far as regards their future Annual Contributions.

A modification of this rule, in certain cases, was agreed to 3d January 1831.

land, and wishing to continue a Fellow of the Society, it shall be in the power of the Council to determine on what terms, in the circumstances of each case, the privilege of remaining a Fellow of the Society shall be continued to such Fellow while out of Scotland.

V.

Defaulters.

Members failing to pay their contribution for three successive years (due application having been made to them by the Treasurer) shall be reported to the Council, and, if they see fit, shall be declared from that period to be no longer Fellows, and the legal means for recovering such arrears shall be employed.

VI

Privileges of Ordinary Fellows.

None but ordinary Fellows shall bear any office in the Society, or vote in the choice of Fellows or Office-Bearers, or interfere in the patrimonial interests of the Society.

VII.

Numbers Unlimited.

The number of Ordinary Fellows shall be unlimited.

VIII.

Fellows entitled to Transactions.

The Ordinary Fellows, upon producing an order from the TREASURER, shall be entitled to receive from the Publisher, gratis, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

Mode of Recommending Ordinary Fellows.

No person shall be proposed as an Ordinary Fellow without a recommendation subscribed by *One* Ordinary Fellow, to the purport below.* This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three Ordinary Meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

X

Honorary Fellows, British and Foreign.

Honorary Fellows shall not be subject to any contribution. This class shall

* "A. B., a gentleman well skilled in several branches of Science (*or Polite Literature as the case may be*), being to my knowledge desirous of becoming a Fellow of the Royal Society of Edinburgh, I hereby recommend him as deserving of that honour, and as likely to prove a useful and valuable Member."

This recommendation to be accompanied by a request of admission signed by the Candidate.

consist of persons eminently distinguished for science or literature. Its number shall not exceed Fifty-six, of whom Twenty may be British subjects, and Thirty-six may be subjects of foreign states.

XI.

Royal Personages.

Personages of Royal Blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

XII.

Recommendation of Honorary Fellows.

Mode of Election.

Honorary Fellows may be proposed by the Council, or by a recommendation (in the form given below *) subscribed by three Ordinary Fellows; and in case the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting. The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the ballot is to take place.

XIII.

Election of Ordinary Fellows.

The election of Ordinary Fellows shall take place at the Ordinary Meetings of the Society. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided Twenty-four Fellows be present and vote.

XIV.

Ordinary Meetings.

The Ordinary Meetings shall be held on the first and third Mondays of every month, from November to June inclusive. Regular minutes shall be kept of the proceedings, and the Secretaries shall do the duty alternately, or according to such agreement as they may find it convenient to make.

XV.

The Transactions.

The Society shall from time to time publish its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall

* We hereby recommend_____
for the distinction of being made an Honorary Fellow of this Society, declaring that each of us from our own knowledge of his services to (*Literature or Science as the case may be*) believe him to be worthy of that honour.

(To be signed by three Ordinary Fellows.)

_____

_____

_____

To the President and Council of Royal Society of Edinburgh.

deem it expedient to publish in the *Transactions* of the Society, and shall superintend the printing of the same.

XVI.

How Published. The Transactions shall be published in Parts or *Fasciculi* at the close of each session, and the expense shall be defrayed by the Society.

The Council. There shall be elected annually, for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Six Vice-Presidents, two at least of whom shall be resident; Twelve Counsellors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator of the Museum and Library.

XVII.

Retiring Counsellors. Four Counsellors shall go out annually, to be taken according to the order in which they stand on the list of the Council.

XVIII.

Election of Office-Bearers. An Extraordinary Meeting for the Election of Office-Bearers shall be held on the fourth Monday of November annually.

XIX.

Special Meetings: how called. Special Meetings of the Society may be called by the Secretary, by direction of the Council; or on a requisition signed by six or more Ordinary Fellows. Notice of not less than two days must be given of such meetings.

XX.

Treasurer's Duties. The Treasurer shall receive and disburse the money belonging to the Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually; and at the last Ordinary Meeting in January he shall present the accounts for the preceding year, duly audited. At this Meeting, the Treasurer shall also lay before the Council a list of all arrears due above two years, and the Council shall thereupon give such directions as they may deem necessary for recovery thereof.

XXI.

Auditors. At the Extraordinary Meeting in November, a Committee of three Fellows shall be chosen to audit the Treasurer's accounts, and give the necessary discharge of his intromissions.

The report of the examination and discharge shall be laid before the Society at the last Ordinary Meeting in January, and inserted in the records.

XXII.

General Secretary's Duties.

The General Secretary shall keep Minutes of the Extraordinary Meetings of the Society, and of the Meetings of the Council, in two distinct books. He shall, under the direction of the Council, conduct the correspondence of the Society, and superintend its publications. For these purposes, he shall, when necessary, employ a clerk, to be paid by the Society.

Secretaries to Ordinary Meetings.

The Secretaries to the Ordinary Meetings shall keep a regular Minute-book, in which a full account of the proceedings of these Meetings shall be entered; they shall specify all the Donations received, and furnish a list of them, and of the donors' names, to the Curator of the Library and Museum: they shall likewise furnish the Treasurer with notes of all admissions of Ordinary Fellows. They shall assist the General Secretary in superintending the publications, and in his absence shall take his duty.

XXIII.

Curator of Museum and Library.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific Productions, and other articles of a similar description belonging to the Society; he shall take an account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Fellows.

XXIV.

Use of Museum and Library.

All articles of the above description shall be open to the inspection of the Fellows, at the Hall of the Society, at such times, and under such regulations, as the Council from time to time shall appoint.

XXV.

Register Book.

A Register shall be kept, in which the names of the Fellows shall be enrolled at their admission, with the date.

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TRANSACTIONS.

I.—*On the Impregnation of the Ova of the Salmonidæ.* By JOHN DAVY, M.D., F.R.S. Lond. & Edin., Inspector-General of Army Hospitals.

(Read 6th March 1854.)

FROM time to time it has been asserted, that the function of impregnation of the ova of these fish is performed after the manner of that of the cartilaginous, viz., before exclusion. The instances related in proof are commonly of a vague kind, and such that little credit can be attached to them. Recently a more precise example has been adduced,—how the ova of the trout, taken from the abdomen of the parent fish, and placed in a "running stream" apart, included in a perforated box, in due time were hatched, producing young fish. The particulars of the experiment, and the result, were published in the spring of last year, and in more than one of the provincial papers; and Dr ROBERTSON of Dunkeld was named as the institutor and reporter of the trial.

Considering the manner in which this statement was made and received, and the practical conclusion deduced,—that no longer any trouble need be taken in the artificial mode of breeding to obtain the milt to apply to the roe, I have thought it worth while to give the subject some attention, on the supposition that the result, as stated, may have been accurate, being, as it appeared to me to be, within the limits of possibility,—though I cannot say, keeping in mind the structure of the male and female fish, and all the information hitherto collected respecting the manner in which the generative process is carried on by them,—that it is within the limits of probability.

I shall first briefly notice some trials which have been made, and with a view to determine the question.

Mr SHAW, in his valuable paper on the Development and Growth of Salmon Fry, published in 1840 in the Transactions of this Society, describes how, in two instances, he obtained negative results in experiments on mature ova of the sal-

mon which had not been mixed with milt after exclusion, though in all other respects placed and treated like other ova from the same fish,—ova which had been mixed with milt after their exclusion, and were thereby impregnated, and rendered prolific.

Mr Young, in his Natural History of the Salmon, gives an account of some experiments with a similar negative result. In page 17, he states, "We have often experimented on the ova of fishes, merely to arrive at facts. We have impregnated one part of the ova of the fish with milt, and have left part unimpregnated, and then deposited both parts in the same stream, at the same depth, and in a current of exactly the same velocity. But never, in any one instance, did we find one grain of the unimpregnated part productive, while the other portion that was impregnated with the milt never failed to produce fry in due time." He adds, "This has been frequently tried, and has at all times proved the same."

Mr Ashworth, by whom the production of salmon on a large scale has been so successfully carried on in Ireland, informs me, in a letter with which he has favoured me, of a similar negative result,—how Mr Ramsbottom, in his employ, "took a female fish (a salmon) and extracted a quantity of eggs; then placed them in a box alone, without impregnating them with the milt, and none of them came to life;" and how "he took the remainder of the ova from the same fish, and impregnated them with the milt, and these produced young fish."

The trials I have made have afforded similar negative results. I shall mention three in particular.

On the 10th of last November, from a stream in which there were known to be male fish with mature milt, two female trouts were taken with fully formed ova,—ova that were expelled by the application of gentle pressure to the abdomen. These were placed on gravel in a glass vessel with water, which was changed twice daily; they exhibited no marks of development, and one after another became opaque from imbibing water.

On the 25th of the same month, I procured two charr from Windermere,—a male and female fish, taken from a shoal in the lake, a breeding bed. On gentle pressure to the abdomen, ova in large quantity were obtained, and abundance of spermatic fluid; each fish at the time was alive. A portion of the ova was placed in three glass vessels with gravel and water, without having been allowed to come in contact with the milt. Another portion of them was mixed with the milt, and similarly distributed. The vessels were kept in a room of pretty equable temperature, which ranged from about 51° Fahr. to 44°, that is, from the commencement to the present time, and the water—spring water—was changed daily once, and no oftener. Now, January 4th, a large number of the eggs which had been mixed with the milt are well advanced, the fœtal fish being visible in the ova with the naked eye, and this in each of the three vessels; but, on the contrary, in the other three vessels, not one egg bears any marks of vital pro-

gress; many of them have become opaque; the majority of them, and those which remain transparent, are of uniform appearance, whether seen with the naked eye or under the microscope. Under a one-inch object-glass, in all of them, at one spot, a patch, as it were of cellular tissue, is observable, seemingly adhering to the membrane of the egg, with oil globules entangled in and surrounding it.

On the 2d December, I procured some eggs from two charr, taken at the same time as the preceding, and from the same breeding shoal, and kept in company with male fish in a well fed by a small stream. The eggs, obtained by pressure to the abdomen, were the few remaining, the greater portion having been previously shed, as was manifest from the lankness of the fish. From this circumstance, they seemed peculiarly favourable for the trial, on the hypothesis of the possible admission of the spermatic fluid *ab externo*. But the result was equally negative with the foregoing. The ova put into water, the same as that used with the impregnated, fertile ova, and under the same circumstances, all underwent no change, excepting that denoting loss of vitality.

Many other instances of the like kind I could relate, that have been communicated by friends interested in the subject; but I hardly think them necessary, those I have given appearing to me so conclusive, even on the doctrine of chances.

Next, it may be well to advert to the structure of the male and female of the Salmonidæ, to which I have alluded, as seeming to render impregnation from without very improbable.

The female, as it is well known, has no true oviduct, as in the instance of the cartilaginous fish. Her ovaries are not connected with any permanent openings; an aperture for their escape being made only just before the exclusion of the ova,—that is, when the ova are mature and detached from the ovaries, and when, by their volume, they distend and press on every part of the peritonæal sac, but necessarily with most effect where there is least resistance, viz., close to the anus, the very spot where the aperture is to be formed with a suitable structure for their exit. How ill adapted is this for the required effect, according to the supposition of impregnation of the ova before exclusion? Moreover, as regards the male fish, we see the same inaptitude exhibited in the conformation of its generative organs. They are of the simplest kind, the testes terminating in an aperture close to the anal end of the intestine, without even a distinct papilla furnished with erectile tissue, and open only whilst needed for the outpouring of the abundant spermatic fluid, distending the organs in which it is secreted, and by them distending the abdomen.

The inaptitude of the organs in both sexes for the presumed office is the more manifest, as it has seemed to me, the closer the attention is given to the minute structure of the parts concerned. In the instance of the female, the aperture is in a vascular papilla, prominent at the verge of the anus, and internally pro-

vided with folds,—a somewhat valvular structure, that reminds one of the mouth of the common gall-duct in man,—allowing a free passage to a probe downwards, but not in the opposite direction, and being amply provided with mucous follicles, forming a provisional mucous duct, the better adapted to the descent of the ova.* In the male, the testes terminate in a common duct, slightly prominent within the verge of the anus,—the projection so small as hardly to deserve the name even of papilla, very much smaller than that of the female, and neither vascular, so far as I could ascertain, except in the ordinary manner, nor provided with any follicles, such as usually belong to the part destined for the purpose supposed.

Further, if attention be given to the manner in which the male and female fish behave during the spawning time, I think we shall have confirmation that there is no act of intromission,—which indeed, anatomically considered, it may be presumed there cannot be,—but also that there is no attempt made favouring the notion that the spermatic fluid is injected (as would be necessary for the impregnation of the ova) into the cavity of the abdomen of the female. That the fish in the act of spawning sometimes come in contact, pressing against each other, and thereby aiding the expulsion of the ova and milt, cannot, I think, be doubted. By many observant fishermen, poachers addicted to the taking of the fish at the time of their spawning, I have been assured of the fact from their own observations; but this is very different from the act of copulation as performed in other classes of animals in which impregnation is effected before the expulsion of the ova: but though so dissimilar, perfectly suitable to the end required, and quite in accordance, as we have proof in the artificial process, with the necessary requirements.

It is an axiom that nature does nothing in vain; it is not less true that nature is perfect in her works, as regards the adaptation of means to ends. In no part of the animal economy is this more strongly and happily illustrated than in the generative system of organs, diversiform and varied as they are in the several classes of animals. Consistently, then, were the mode of impregnation that which has been asserted, we may be sure that an organization,—an apparatus would have been provided suitable to it. Also, as I think consistently with the hypothesis, we might expect occasionally to find ova in the cavity of the abdomen, bearing marks, if they had been impregnated there, of incipient development, according to the analogy of extra uterine fœtal growth sometimes witnessed in the Mammalia; but none have been described, that I am aware of, as ever observed. In spent fish, that is, those which have spawned, in the instance both

* The closure of this aperture, after the exclusion of the ova, from such observations as I have made, appears to take place slowly, requiring many weeks for its accomplishment, and when effected, by so delicate a medium as to be easily ruptured. To be properly examined, the fish should, after being opened, be placed under water, and the blow-pipe be used before the probe.

of the salmon and trout, I have in spring found mature transparent ova detached from their ovaries, so included, when the aperture for the passage of the ova was closed, or almost so; but they were totally destitute of any appearance of vital development.

In conclusion, granting the observations referred to—of the hatching of the ova of the trout in the manner described, viz., without milt, so far as was known, being brought into contact with the expressed ova—to be accurate in their detail, it may be asked, Does the result, as stated, warrant the inference that impregnation was effected before the expulsion of the ova? The box, we are informed, containing them was placed in a stream. What is more likely than that they might have been impregnated, so included but not insulated, by the spermatic granules, the spermatozoa of milt shed by some fish in the adjoining water? The diffusibility of these living granules—not the least remarkable of their qualities—seems to be favourable to this conclusion.

Lesketh How, Ambleside,
January 4, 1854.

II.—*On the Torbanehill Mineral.* By Thomas Stewart Traill, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh.

(Read 5th December 1853.)

It is well known to the Society, that a mineral, found in the strata of Torbanehill, not far from Bathgate, in the county of Linlithgow, has lately been the subject of a keen lawsuit. Specimens of this mineral were early in the year put into my hands by the lessee, and sometime afterwards by the proprietor of Torbanehill, for the purpose of obtaining a mineralogical opinion on the nature of the mineral. I stated at the time to both parties, that I would carefully examine it before I could presume to offer a decided opinion. In fact, the mineral was not altogether new to me; for, in the two preceding years, I had received specimens of a mineral from Torbanehill, and from Boghead, which I was unable to refer to any described species; and I was therefore determined to give my opinion with due caution, especially as I understood that large interests were involved in the question.

After carefully comparing its characters with a great number of different kinds of coal, of bituminous shale, and asphalt, with all of which it presented affinities, and after a considerable number of experiments on its chemical characters, I came to the conclusion, that it was a mineral distinct from either, and so peculiar as to deserve a particular name. From its qualities, I proposed for it the designation of Bitumenite, as it seemed to consist of much bitumen, mingled with earthy matter.

This opinion I mentioned to one or two friends, and soon after, I received an intimation, that I should be requested to attend as a witness on the part of the proprietor. From his agents, however, supposing that they had enough of scientific evidence on their side, I was not examined on the trial.

I had been requested to examine the mineral in its native bed; and, accordingly, went to Torbanehill, and saw the works. There were four pits or shafts, but No. 1 was no longer wrought; Nos. 2, 3, and 4 were in active operation, and large blocks of the mineral lay around the mouths of these pits. The quality of the mineral extracted from each appeared to the eye nearly similar; the blocks varying in thickness, from 1 foot 4 inches to 1 foot 11 inches.

I descended into No. 2, which was wrought by a small steam-engine. The depth of the shaft was 17 fathoms. From the bottom of the shaft, a drift was carried for 80 yards, in a northerly direction, with a dip of about 1 in 12, almost half-way between No. 1 and No. 3. This *working* was so low, that we could not stand upright; and the most convenient mode of exploring its termination, or the *face*, as it is technically termed, is to lie at full length in a truck, and to be leisurely let down the *incline*. In descending, the succession of the strata are:—

1. A thick roof of sandstone.
2. *Facks*, a crumbling shale=4 inches in thickness. This bed in No. 3 is wanting; but it forms the roof in No. 4.
3. *Cement*, a mixture of shale and a poor ironstone=3 inches.
4. *Bituminite*, which in this pit at the *face*=1 foot 4 inches in thickness.
5. *Fine Ironstone*, from 2 inches to $\frac{1}{2}$ inch.
6. Bituminous Shale, often containing tabular masses of good ironstone= 2 inches.
7. An inferior coal=7 inches. These four last-mentioned beds are all raised with the Bituminite, and together measure 2 feet 3 inches in thickness.
8. Coal, much mixed with shale, here called *foul coal*, about 2 feet 4 inches.
9. Fire-clay.

These notices will sufficiently shew the position of the Bituminite, &c., which has nothing peculiar in its situation in the earth to distinguish it from any other mineral occurring in a coal-field. It seems, however, to be thickest near the top of the field, as in No. 4, and to diminish a little in thickness in the other two pits.

I had the pleasure of visiting Torbanehill with one of the most eminent and experienced coal-surveyors of England, Mr Nicholas Wood, President of the North of England Institute of Mining Engineers, and asked him, "If you had *bored* at Torbanehill before the working of the shafts, would you, from what the instrument brought up, have said that there was here a workable coal?" He answered, "Decidedly not. The 7-inch coal is not workable; and the substance you call *Bituminite* I should have considered as a shale."

I have compared Bituminite with a great number of different coals, as with common English coal, Wigan cannel, and with several varieties of Scottish coals, as—

1. Lord Stair's cannel or parrot coal, from Oxenford.
2. Marquis of Lothian's parrot coal.
3. West Wemyss parrot coal.
4. Arniston parrot coal.
5. Methill coal, which, however, approaches more nearly to bituminous shale than to common coal, having a nearly dull fracture, though with a strongly shining streak unchanged in colour.

6. Three sorts of Capeldrae coal.
7. Two sorts of Lochgelly parrot coal.
8. Duke of Hamilton's Lesmahago cannel or parrot coal.
9. Mr Ferguson's parrot coal.
10. Kingswood parrot coal.
11. Cowdenhillhead parrot coal.
12. West Wemyss *Rums*, which is little better than a bituminous shale.

Mineralogists distinguish mineral species chiefly by what are termed *external characters*, aided by a few simple chemical results.

The Torbanehill mineral differs in *mineralogical characters* from all these; especially in having some degree of *translucency* on the edges, when examined in thin fragments by a strong light; in having a streak not shining, but *dull*; and which is strongly coloured of a pale ochre-yellow hue; whereas every coal has a shining streak, which is not changed in colour. The fracture, too, of every coal affording an abundant gas, as Bitumenite does, is glistening or shining; but the fracture of Bitumenite is perfectly dull. Coal is brittle, or easily broken; but Bitumenite, though it split readily in the direction of the *bed*, is not easily frangible in a cross direction. When so struck with a hammer, it resists, and shews very considerable *elasticity*, causing recoil of the hammer; as was remarked both by Mr Wood and me when procuring specimens of the mineral at the three pits. It has been formed into snuff-boxes in the turning-lathe; but it does not take a polish, and looks dull from this circumstance.

When moderately heated, Bitumenite catches fire almost as readily as asphalt, and burns with a dense white flame, emitting much smoke; but the flame dies away without igniting the mass. In fact, when burnt by itself in a grate, this mineral scarcely produces the glow of ignition, and neither fuses or *cakes* like every bituminous coal, nor forms *cinders*; but it leaves a very white mass of nearly the form of the fragments put into the grate; which mass, however, is very brittle, and easily reduced to a fine powder. It therefore is deficient in one important quality of common coal, that of forming a bright ignited mass, calculated for the roasting of meat, or for completing other culinary operations by radiant heat.

It is distinguished from asphalt by its streak, its tenacity, and by not melting when heated before it inflames.

The Torbanehill mineral is distinguished from bituminous shale by its streak, its tenacity, its cleavage, by its far more ready inflammability, and by the smaller proportion of its earthy residue when burnt, as well as by its far more abundant yield of inflammable gas.

On analyzing *three* different specimens of Bitumenite, I found the proportion of volatile and fixed ingredients somewhat different in each.

a. Yielded 27·5 residue, and 72·4 per cent. was dissipated.
b. „ 21·4 „ „ 78·6 „ „
c. „ 15·9 „ „ 84·1 „ „

One hundred grains of Bitumenite afforded by distillation 60·5 cubic inches of inflammable gas, which is equivalent to rather more than 274 cubic feet per cwt.; and this gas had a far greater illuminating power than the best coal gas I have ever examined. I considered it in this respect to approach nearer to oil gas than to common coal gas. I did not, however, determine the comparative illuminating power by the best test, chlorine, but estimated its superiority by the eye.

Besides this, the distillation afforded a considerable quantity of the chemical compound known by the name of *Paraffine*, a substance now used to grease machinery, and for other economical purposes; a product which I have not obtained from coal when distilled for gas.

I am well aware that by a slow distillation with a moderate heat, *Paraffine* may be procured from certain kinds of coal, from peat, from bitumens, and perhaps from every substance capable of yielding olefiant gas, or what has been termed Bicarburetted Hydrogene; for their atomic constitution seems to be nearly similar $=C4+H4$, or some multiple of these. This substance has been manufactured in large quantities from Bitumenite by Mr Young, as the specimen on the table will shew.

By distillation, too, petroleum may be obtained from it, especially if the mineral be first allowed to imbibe as much water as it can absorb; and this product is more abundant, more than is yielded by any coal. The fixed residue of the combustion of Bitumenite consists chiefly of silex and alumina, with traces of lime and oxide of iron.

Bitumenite occasionally contains casts of vegetable remains, especially of large *Stigmariæ*. A very magnificent specimen of Stigmaria in Bitumenite, as thick as the human body, has been deposited by Dr Christison in the University Museum. In examining the structure of thin slices by the microscope, I perceived where the organisms occurred some traces of vegetable structure; but in other portions of the mineral I was unable to perceive the least trace of organic structure. In several specimens, in the hands of Mr Sanderson the lapidary, I was also unable to see real organic structure; but observed numerous globules of a pale yellowish matter, which I take to be bituminous particles disseminated among the opaque earthy ingredients.

But even if such organic texture could be traced in every part of it, still this would not constitute it a coal; for organic structure is constantly seen, too, in *mineral charcoal*, in *surtarbrandr*, in *peat*, and in petrified wood; yet nobody would denominate these varieties of coal.

One person insisted, that, as the mineral contained carbon, was inflammable, and occurred in a *coal formation*, it must be considered as a *coal*. To this I would reply, Must we consider as coals—Naphtha, Petroleum, Asphalt, Amber, Mellite, or the rarer minerals, Piauzite, Ixolite, Schererite, Hartine, Walchowite, Middletonite, Retinite, Hartite, Ozokerite, &c., all which are inflammables, contain carbon, and occur in the coal formation?

On these grounds I consider the Torbanehill mineral not to be a *coal*, but a mineral not hitherto described in our systems of mineralogy, and for which I have proposed the name of

BITUMENITE;

and which has the following mineralogical characters.

a. Colour passing from blackish-brown (108, *Syme*) to liver-brown (104), and often spotted with hair-brown (105).

b. It occurs massive.

c. It is dull in every direction.

d. Fracture—principal fracture flat, conchoidal, inclining to splintery; cross fracture uneven, slaty.

e. Fragments are indeterminately angular, with sharp edges.

f. Examined by a strong light, the sharp edges are feebly translucent, admitting a dark reddish-brown light.

g. It yields to the hammer in the direction of the bed, but resists blows at right angles to that direction, and is rather difficult to break, exhibiting a considerable degree of *elasticity*, and causing the hammer to rebound smartly.

h. It is, however, soft and sectile.

i. Its streak is quite dull, and has a pale ochre-yellow hue.

k. Its specific gravity = 1·284.

Chemical Characters.

a. It is very inflammable, readily catching fire without melting, burning with a dense white flame, and much smoke. When ignited at a lamp, it continues to burn a considerable time. Some bituminous shales will also thus burn for a shorter time, but they leave their edges more or less white; but with Bitumenite, when the flame expires, the form of the fragment is unchanged, and it is wholly covered with a black carbonaceous matter, derived from the dense smoke of its flame.

b. When exposed to a strong red-heat in a platinum crucible for 2½ hours, it left behind a white matter, retaining the shape of the original fragments, but readily crumbling on pressure into a grayish-white powder; but no part of it was converted into a slag.

c. When distilled in a small iron retort, it afforded an abundant dense inflammable gas, and some *paraffine*, with a few drops of water.

Composition.

A specimen of the darkest colour, from pit No. 3, afforded of—

Volatile matter = 84·1 per cent.
Solid residue = 15·9 „

100 grains afforded 60·5 cubic inches of a dense inflammable gas.

Besides these, the quantity of paraffine appeared considerable.

The solid residue consisted chiefly of silica and alumina, with traces of lime, and oxide of iron.

Geological Character.

It occurs in a bed, varying in thickness from 16 to 24 inches, in the coal formation at Torbanehill and Boghead, in Linlithgowshire, in contact with shales and clay ironstone, with an inclination of about 7°, dipping to the north. It occasionally contains casts and impressions of large Stigmariæ, and other fossil vegetables, which are also found in the accompanying shales.

Note.—Since the above paper was written, I have learnt that an important decision of the question, "*Whether this mineral be, or be not, a coal?*" has occurred in Germany.

About the very time when the case of GILLESPIE *v.* RUSSELL was tried in our Courts, and a Jury decided that the Torbanehill mineral *was a coal*, a directly opposite opinion was pronounced by a more competent tribunal in Prussia.

It seems that in the imperial city of Frankfort there are two rival gas companies, one of which is restricted to the use of oil, or of resinous or bituminous substances, the other to coal alone, in the manufacture of gas for illumination.

The oil gas company imported the Linlithgow mineral, as a bituminous substance for gas-making; but they were opposed by the other company, as infringing their exclusive right. Another question also arose between the first company and the customhouse. By the laws of the German *Zollverein*, coal pays an import duty of from one to two shillings per ton, while oil or resinous substances used for gas-making may be imported duty free.

The city customhouse authorities were unable to decide the question; and, as in all such circumstances of doubt, the case was referred to the determination of the Central Board of Customs, which has its seat at Berlin. That board wisely called in the assistance of the most eminent scientific men of the capital, among whom were some of the distinguished Professors of the Berlin University; and these united bodies have decided that the Linlithgowshire mineral may be im-

ported duty free, "for it is not a *coal*, but must rather be classed with bituminous shales."

This decision of the General Zollverein Board is more worthy of notice, as it is evidently against the interests of their customhouse union, since it allows a foreign article to be imported *duty free*.

I am gratified by finding that the opinion which I gave six months before, *that the Torbanehill mineral is not a coal*, has been confirmed by such authority.

I may be permitted to add, that though I readily allow its greater affinity to bituminous shale than to coal, and had conjectured that it may have been formed by the injection of bitumen into a shale, yet it differs in mineralogical characters from any bituminous shale I have seen, or is described in systems of mineralogy, by being perfectly dull in every direction,—by entirely changing its colour in the streak,—by being translucent on its thin edges,—by its higher inflammability,—and by its lower specific gravity.

On these grounds I considered it an undescribed mineral, and ventured to propose for it the name of BITUMENITE.

III.—*On a New Hygrometer or Dew Point Instrument.* By A. Connell, Esq., F.R.S.E., Professor of Chemistry in the University of St Andrews.

(Read 3d April 1854.)

How convenient soever the wet bulb thermometer and the various organic hygrometers may be for giving indications, by simple inspection, regarding the relative states of dryness and humidity of the atmosphere, it is scarcely possible, in conducting meteorological observations, to dispense altogether with instruments calculated for affording more direct information respecting the amount of aqueous vapour present in the air at any particular time.

The old methods of Le Roi, Saussure, and Dalton, depending on the cooling action of water or saline solutions on glass or metallic surfaces, are always available for that purpose; and some years ago I suggested an arrangement on the same principle, consisting merely of a little bottle of polished brass, and a small thermometer, into the former of which given measures of mixed nitre and sal-ammoniac, and of water, were introduced, and the temperature slowly reduced by simple agitation, so as to admit of an easy mode of noting the dew point by a single operation.*

The elegant hygrometer of the late Professor Daniell is sufficiently well known. It is a happy application of the ingenious Cryophorus of Dr Wollaston.

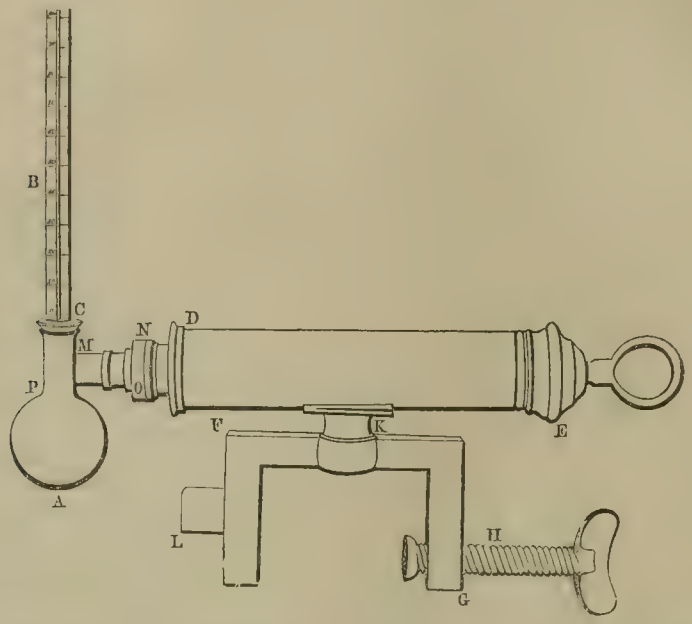
I have now to submit to the notice of the Society an arrangement which has occurred to me for determining the dew point; and I think it will be found that this object may be accomplished by means of it, without much trouble. The method proposed has this in common with Mr Daniell's, that it produces the cooling effect on the observed surface by the volatilization of ether; but it entirely differs from it as regards the manner of removing the obstacles to that volatilization, and of keeping up the process of evaporation.

It is in no respect a cryophorus, but produces and maintains the necessary rarefaction or vacuum, simply by the action of a small exhausting syringe. The accompanying figure will explain the nature of the arrangement.

A is a little round bottle of thin brass, well polished on the outside, and capable of holding, when filled to the bottom of its neck, half an ounce of liquid. Its diameter is about $1\frac{3}{20}$ inch. Its neck is $\frac{3}{4}$ inch high, and about $\frac{3}{10}$ inch wide, and flashed out a little at top. The passage M, which conducts into the neck, has throughout an internal diameter of $\frac{1}{4}$ inch, and it is very essential that it should not be narrower than this.

* See Edinburgh New Philosophical Journal for 1835.

B is a small mercurial thermometer, the bulb of which reaches within $\frac{1}{5}$ of an inch of the bottom of the bottle, and the upper part of the bulb is on a level with



the surface of the liquid contained in the bottle, or a little above that level. The bulb ought not to be entirely immersed in the liquid. Its shape is an elongated cylinder, about $\frac{2}{3}$ inch in length, and about $\frac{1}{8}$ in diameter. The thermometer has a small scale attached to it, graduated according to both FAHRENHEIT's and CELSIUS' scales from 0° F. to 100 F. The stem of the thermometer is cemented at C into a little brass stopper, fitted by grinding into the neck of the bottle, so as to be perfectly air-tight. DE is a small exhausting syringe of brass, the cylinder of which is 5 inches long, by about $\frac{8}{10}$ inch wide. It must effect its purpose of exhausting in as perfect a manner as an instrument of that size can accomplish. FG is a clamp of brass, capable of being attached by the screw horizontally to a window-sill in the position which it occupies in the figure, or vertically to a common table, the holding surfaces which come in contact with the body grasped being well roughened. The syringe screws into this clamp at K by a projecting screw soldered to the former, when the clamp is screwed to a window-sill, as in the case when an observation is made at an open window; or this projecting screw is inserted at L, when the clamp is fastened to a table, as is done when the experiment is made in a room. In both cases the syringe itself occupies a horizontal position, and the bottle and thermometer, of course, a vertical one; the projecting screw K should be so constructed as to cause the syringe to incline with the bottle a little downwards, that the tendency of any ether to pass from the bottle into the

syringe may be counteracted, and any trace of it which should pass or be condensed in the syringe shall run back into the bottle.

Let us suppose the instrument fixed at an open window on the sill. We, of course, note the barometer and the temperature of the external air at the time; and it is convenient, if a wet bulb thermometer happens to be at the window, to observe the amount of the cold of evaporation at the time, because it gives an idea of the point near which the dew point may be expected, although, of course, such an observation is by no means essential.

A half-ounce measure, graduated into drams, is then filled with good commercial sulphuric ether to the extent of three drams of the liquid. This is *slowly* and carefully poured by a proper lip in the measure into the bottle, the other hand being held so as to prevent any interference with this operation from any draft or wind, and the thermometer is immediately inserted in the bottle, and the stopper properly fixed. The process of exhaustion is then begun, at first slowly, by working the piston by one of the fingers of the right hand, so as to produce a gradual cooling agency and equable distribution of the effect; and quickened somewhat as occasion may seem to require. In damp weather, the working of the instrument can scarcely be too slow. One or two fingers of the left hand are held on the upper part of the syringe, both to aid the holding of the clamp, and to be ready to obviate the consequences of any accidental detachment. When the cold of evaporation or other indications prepare us to expect a very dry air, it is best to begin from the first with a pretty quick action of the piston, so as to secure a good reduction, and there will be sufficient time for the spreading of the effect over all the necessary parts. The thermometer will in all cases be observed to begin to fall almost immediately; and generally speaking, according to the observations which I have now been carrying on in this place for three or four months past, the deposition of dew may be first observed on the exterior surface of the little brass bottle in from one minute to one and a half minutes. Sometimes, of course, when the air is drier than usual, longer working will be required, and when it is more moist, less time will be necessary. The screw of the clamp usually requires to be occasionally a little tightened, during the first part of the process of exhaustion.

My first essays, merely in the way of trial, were made with one of the large syringes used in organic analysis, and a very few strokes of the piston were with it found to be sufficient for the purpose. But as it was not only inexpedient that the object should be so quickly accomplished, but so large an instrument would have been deficient in portability, and inconvenient in other respects, I applied to Messrs Kemp of Edinburgh to construct for me a syringe as small as they thought was likely to accomplish the desired purpose. They at first sent one only $3\frac{1}{4}$ inches long. This I found to be quite powerful enough for some cases, but not sufficiently so for many which occur. I therefore had one made of the size described above.

This, during three months' observations, I have found to be sufficient for general use: but an extreme case occurring, when, after reducing the temperature below 20°, no dew occurred, I thought of a method of increasing the effect without having recourse to any augmentation of the size of the syringe. This was to have a collar of ivory introduced between the little bottle and the syringe, so as to cut off entirely metallic communication between them. The passage into the bottle of the heat generated during the motion of the piston, would thus be prevented, and the cold produced by the evaporation of the ether would be enabled to take full effect. This idea was carried into execution, by constructing of ivory the extremity N of the syringe carrying the terminal valve. The aperture in the portion of this ivory piece to which the valve is attached is at least $\frac{1}{15}$ inch in diameter, that of the remainder of it is $\frac{3}{20}$. This ivory piece screws into the brass passage which conducts into the neck of the bottle. Since this change was effected, I have experienced little difficulty in producing the necessary cold, even in a very dry atmosphere, and have obtained it even as low as 8° F., with the external air at 36°. There is one point, however, which requires attention in the use of this ivory valve-piece,—to avoid any considerable force either in screwing or in unscrewing it, as its fracture is risked. Hence we ought to be particularly on our guard against this screw getting *fixed*, either by being overscrewed, or by remaining too long unmoved. Indeed the safer way is, always after using the instrument, to loosen this screw lightly, and leave it so till next observation, and then tighten it again; and if it should happen to get fixed, to wrap it round with cotton, moisten the cotton well with olive-oil, and leave it in this state for a couple of days, when it will admit of being unscrewed without injury. If necessary, a projecting screw of brass might be attached to the lateral passage into the bottle, and fit into the ivory valve-piece.

At a much earlier period of the trial, a difficulty occurred as to the valves of the syringe. These were at first constructed of the usual oiled silk. But it was soon found that the ethereal vapour acted on them, and impaired their energy. Vulcanized Indian-rubber was then tried, but, as might have been foreseen, from the solvent action of ether on caoutchouc, this material, although it stood better than the oiled silk, was also affected. I then thought that it would have been necessary to have had recourse to metallic valves, when Mr Alexander Kemp suggested the employment of goldbeaters' leaf. This suggestion, contrary perhaps to what might at first have been expected, has hitherto proved quite successful; and I have now been daily using, for three months, two syringes, the valves of which are made of this substance, without any material diminution of their action.*

I had at first thought of holding the apparatus simply in the hand during its

* In constructing each valve, four folds of the leaf are employed. Should the valves become impaired by use, they may easily be renewed, and the leather of the piston may also in time require renewal also.

employment, but it was soon found that it could not be efficiently worked in this way; that the same reduction of temperature could not be effected; that the exertion was fatiguing; and that by the motion the thermometer was sometimes broken. I now never think of using it, except when well secured by the clamp to a fixture.

By this arrangement, during the winter, at an open window, I have reduced the temperature of the ether in the little bottle, when I thought proper, from 20° to 30° below the temperature of the air, which, so far as my observation has reached, embraces all ordinary cases of dew point. In a room of the temperature of 57°, I have effected a reduction of 42° below the temperature of the room; and should it ever be thought that the reducing agency is not sufficient, we always have it in our power to augment the energy of the process, by enlarging somewhat the syringe, say to $5\frac{1}{2}$ or 6 inches in length, by $\frac{9}{10}$ inch in diameter. This, I doubt not, would give a considerable augmentation of power, and would not present an inconveniently large instrument; but still I think it much better to continue it at the size I formerly mentioned, unless more extended observation shall shew the propriety of farther enlargement.*

The expenditure of ether during the exhaustion is very small; being on an average about half a dram or less, value from a halfpenny to a farthing. With the dew point only 5° or 6° below the temperature of the air, I have obtained the required reduction with the expenditure of only $\frac{1}{10}$ of a dram. When the observation is completed, the residual ether is immediately poured back into a separate bottle kept for the purpose, and well stopped; and it may be used again repeatedly, making it up each time to 3 drams, by adding fresh ether from another bottle, in so far as necessary. I have used the same ether in this way for a week or two; but, of course, this should not be persevered in too long; and entirely fresh ether should be employed after a certain time, and preserved as before, till no longer proper for use. The inexperienced operator should also be warned, that, in case of any night observations, any light employed should be kept at a distance from the ether, which, from its extreme volatility and inflammability, is very apt to take fire, unless this caution is used. It should farther be borne in mind, that all commercial ether contains alcohol, which, of course, will not be soreadily volatilized as the ether itself, and will accumulate in the residual liquid. After the instrument has been used, and the residual ether poured back, it is expedient to cause the instrument to lean against some support for a few minutes, with the bottle in an inverted position; and then to work the piston a few times backwards and forwards, to expel residual ether or its vapour. The leather of the piston should be rubbed from time to time with olive-oil, and care should be taken that

* Whilst revising the proof sheets of this paper in the month of June in London, I have had opportunities of trying the instrument at higher temperatures at an open window; and with the external thermometer at 68° have effected a reduction of $34\frac{1}{2}$°, the dew point being on one occasion found to be $31\frac{1}{2}$° below the temperature of the air.

the washers of the different screws do not become too dry. This is prevented by the occasional use of olive-oil; and this is one of the first things to be looked to, at any time when the instrument may seem not to work well. It must of course be remembered that the different parts of the instrument must not only be of the best construction at first, but must be *maintained* in a fit condition, by constant attention to the state of the valves, of the connecting screws, and of the piston, &c.

Every one must, of course, be allowed to make his observation as to the true point of deposition, in the manner he thinks best; but I may be permitted to make a few remarks explanatory of my own views upon the subject. It is well known that it has been often objected to Mr DANIELL's instrument, that it gives the dew point too high; and Mr JOHN ADIE, by a comparison of its indications with those obtained by DALTON's method, found the error occasionally to amount to $6\frac{1}{2}^{\circ}$, and on an average of 28 observations, to reach $2\cdot9$. This is explained on the idea that the surface of the ether, which is the seat of greatest cold, communicates the effect to the surrounding zone of the glass bulb of the hygrometer, before the bulb of the thermometer has been cooled to the same extent by the liquid; that portion of the bulb above the surface of the liquid itself, as well as that below it in the liquid, being supposed to be at a higher temperature. It appears to me that the instrument described in this paper will be much less liable to such an objection, because metal being a much better conductor of heat than glass, it is hardly possible that the cooling effect should accumulate in any one zone of the little brass ball, but must be diffused over the whole without delay. Time is thus given for the frigorific influence being communicated to the whole bulb of the thermometer, both by the evaporating surface, and by the body of the liquid itself, which principally yields the latent heat required by the ethereal vapour. I have already stated that the thermometer ought to be so placed as to have the upper part of its bulb in the plane of the surface of the liquid, or a little above that position, and the rest of it immersed in the fluid. These observations being premised, I conceive that the great point, in the first instance, is to endeavour to mark the very first decided deposit of moisture on the outside of the little bottle. I am aware that some regard the point of disappearance of the dew, when the cooling process is stopped, as giving better indication. I confess I do not concur in this idea, because a little time must elapse before the air can take up again what it has already deposited: and if more than the very initial deposition has occurred, this taking up will be still farther retarded, and the apparent point of deposition elevated beyond the reality. This observation, of course, does not apply to DALTON's method by transference, because each observation is isolated and complete in itself. Most experimenters now, I believe, with ordinary dew point hygrometers, take a mean of the two observations. This is perhaps the best mode of any, and at all events is decidedly better than the disappearance alone; and, accordingly, it is the method which I have followed in making the observations with this

instrument, recorded in this paper, and which may be recommended to others. The proper mode will be, first, to note the instant of decided appearance of moisture, and mark the temperature indicated by the thermometer; then, to stop the farther reduction the *instant* this *decided* deposition is observed; next, to mark the instant of disappearance, and the corresponding temperature; and, lastly, to take a mean of the two observed temperatures. In observing the point of *appearance*, I notice the first apparent indication of moisture, and then see whether, *immediately* on farther reduction of temperature, this indication becomes quite distinct. I then take the former indication as marking the temperature of deposition. In this way we can easily distinguish between a true and a false indication. Every one, however, as before observed, must be permitted to make his observation in the manner which he thinks best.

Since this paper was first drawn up, I have made a series of observations, with the view of comparing the indications of the instrument with those afforded both by DANIELL'S hygrometer, and by DALTON'S method of transference, which last is generally admitted to be the most trustworthy, although certainly very far from being the least troublesome.* These observations have supported the view that DANIELL'S hygrometer yields indications somewhat too high, although not to quite so great an extent as follows from Mr ADIE'S observations. They shew that the *tendency* of the instrument described in this paper is rather in the opposite direction, and that its usual indications are a *very little* too low, but that this deviation is less on an average than 1°, and therefore quite within such limits as fully to justify reliance on its results. The following table contains the observations; those by DALTON'S method being stated at the actually observed dew point, and the others in excess or deficiency, with reference to DALTON.

1854. March	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	26.	28.	29.	30.	
DALTON, .	42¾	37½	37½	40¾	40	40	35	33	32	36⅝	34	42	35½	31	35½	39¾	45	43	
DANIELL, .	−1	+1¼	+ ¼	=	+2¼	+ ½	−1	+ ¼	+ ¼	+ ¼	+1⅜	−1¼	+ ⅜	+ ¾	−2	+1½	+1¼	− ¼	Average +0°·29
New Instrument,		−1	+2	− ¾	− ½		−2½	−2¾	−1½	−2	+ ½	−2	−2½	− ½	−2¼	− ½	− ½	+1½	Average −0°·95

We thus have, in a series of observations of dew point temperatures, varying from 32° to 45°, DANIELL'S hygrometer, in eighteen observations, giving twelve of them in excess or above the dew point by DALTON'S method, five of them in deficiency or below the point referred to, and one of them shewing equality. The average of the whole, however, gives only 0°·29 F. in excess. I cannot, however, help saying, that when I got an observation by DANIELL in deficiency, and repeated

* In support of the great trustworthiness of DALTON'S method, I may appeal to the opinion of Professor JAMES FORBES, 2d Report on Meteorology to British Association, 1832; and to Dr THOMSON'S opinion, there referred to. It is unnecessary to say that I had previously compared the thermometers, and that I made allowance in the comparative table drawn up, for a slight difference observed in their indications.

it with every care, I then obtained a result in excess, and therefore am inclined to think, that the tendency of that instrument is so decidedly in excess, although certainly to only a small extent, that I doubt the perfect accuracy of any operation with it in deficiency. I however retain the average, as observation gave it. With regard to the instrument described in this paper, it affords, in sixteen observations, thirteen results in deficiency with reference to the same standard, and three in excess, the average of the whole being $0^{\circ}{\cdot}95$ in deficiency.

These observations, I think, entitle me to ask that any one wishing to make an experimental estimate of the value of the indications of this instrument, shall not assume Daniell's instrument as giving indications which can be rigidly considered as standards of comparison. It is by Dalton's method that I should wish it to be tried; and, I may mention, that in using this method I employed two German becker glasses, $3\frac{1}{2}$ inches deep by $2\frac{1}{4}$ wide, and added a mixture of equal parts of nitre and sal-ammoniac, in powder, to the water, till it shewed dew; and then, by transference of the liquid from one to the other, carefully cleansing and drying the surfaces, noted the point of disappearance of moisture, and took a mean between that and the last observation shewing dew, provided the difference did not exceed 1° or $1\frac{1}{2}^{\circ}$; being satisfied that there were many more chances that the true dew point lay in this interval, than at the first temperature at which no dew was noticed. The difference, however, resulting from this mode of observation in the noted result scarcely in any case exceeds $0^{\circ}{\cdot}5$ F., and is usually much less.*

I think it preferable, on the whole, to leave the little bottle with its proper brass surface duly polished, rather than to have it gilt; at least I think that every one will find this to be the case, who will take care always to have it preserved quite bright and clean, which is easily done by the use, when necessary, of ordinary brass polishing paste.† For those who would wish to have it always in a state fit for use, with little or no trouble, although at the cost of a little polish of surface, it will be best to have the surface gilt. Although not quite so delicate in its indications, it will be sufficiently so for use. This is a matter which may be safely left to the option of individuals. Possibly a bottle of polished silver might, on the whole, be better than either, but I do not think it likely that any advantage in this way would be worth the additional expense. This method is, however, open to any one who wishes it.

* I occasionally noticed that after adding the salts, the *first* deposition of moisture could not be noticed before the surface of the glass had *imperceptibly* become quite moist, and the temperature much below the true dew point; but this, of course, was easily corrected, by transferring the liquid to a dry vessel, and proceeding in the usual way.

† The manner recommended to me is to scrape off with a knife from a piece of good and light rotten-stone, some very fine powder, to place this on a piece of woollen cloth, such as the rind of broad cloth, to mix this with a little olive-oil, and rub the bottle with this mixture, then to rub it with a piece of cotton cloth, on which a little of the fine powdered rotten-stone has been laid, and to finish by rubbing with a piece of soft cloth, without either powder or oil, till a bright surface is obtained. A piece of chamois leather is also kept with which to clean it, when the paste is not used.

In the use of the instrument, a little dexterity of manipulation will of course be necessary, but this will easily be attained by practice, and the necessary care. Success will entirely depend on minute attention being paid to the various particulars which have been mentioned; on the perfection of construction of the syringe and of its valves; on the various connections being quite air-tight; and on the steadiness and security of the clamp employed.* A point apparently unnecessary to be noticed, although really very essential and very apt to be overlooked, is that the internal diameter of the passage M, connecting the syringe with the bottle, shall throughout not be less than $\frac{1}{4}$ inch.

After I had had the apparatus constructed, I made a search through various books, with the view of discovering whether any similar idea had occurred to any one else. The only instances I could find, which would admit any supposition of resemblance, were in the cases of two hygrometers, the one contrived by Döbereiner, and described in Gilbert's Annalen der Physik† for 1822; and the other by Dr Cumming of Chester, of which an account is given in the Second Part of the Article Thermometer and Pyrometer, of the Library of Useful Knowledge. Both these instruments differ, however, essentially from mine, not only in arrangements, but in principle. They agree with Professor Daniell's and with mine, in producing the necessary cold, by the evaporation of ether; but they produce the evaporation in quite a different way and on a different principle from either, viz., by blowing a current of air through the ether, and so producing and maintaining its volatilization. They use a syringe for this purpose, but it is not an exhausting syringe, it is a condensing one. They create no vacuum of air; on the contrary, they increase the amount of it present. In Döbereiner's, also, there is a rather complicated series of valves between the condensing syringe and the metallic tube, which contains the ether. Both instruments may possibly answer their end, but they differ entirely both from Daniell's and from mine. It is only since the paper was given in to the Royal Society, that I noticed in Ganot's Traité de Physique, an account of Regnault's hygrometer, which effects the reduction of temperature on a similar principle to the two just mentioned, by passing through ether a current of air, which is set in motion by means of what chemists call an Aspirator, *i.e.*, a large gas holder containing air from which there is a flow of water.

My experiments with the instrument have as yet all been made in winter, or early spring, but I do not anticipate any additional difficulty in summer. Indeed the higher temperature will be favourable to the exhaustion, and consequent cold, and in such experiments as those made in a warm room, I have found the results quite satisfactory. See also note, p. 9,

* As window sills are often inclined and uneven, one or two little wedges of wood may be employed to produce steadiness of attachment. Pieces of wood may be used to prevent fine tables from receiving injury, in attaching the clamp to them.

† Zehnterband, s. 135.

When the air is very moist, and there is little difference between the temperature of the air and the point of deposition of moisture, care may be taken that the ether shall have a temperature some degrees above that of the air, which will be effected by holding the hand for a little on the brass bottle after the ether has been introduced into it, or by employing any other means likely to accomplish this object.

If an observation should at any time be made entirely out of doors, all that will be necessary will be to have some fixture attached to a tree or gate-post, or inserted in the ground, to which the clamp may be secured.

Messrs Kemp have undertaken to prepare the apparatus with clamp, bottles for ether, &c. &c., packed conveniently in a small portable case; and any one who may procure it from them, or from any other instrument-maker, ought, of course, to take care that all the particulars mentioned in the foregoing observations are rigidly complied with. Insufficient or careless workmanship is quite incompatible with success; and as already stated, it is not only necessary that the instrument shall be of extremely good construction at first, but that it shall be *preserved* in that state, in all points. In its present state it may seem very simple, as I am happy to say it really is, but still the result was not attained without much consideration regarding minute particulars on my own part, and skill and dexterity on the part of the artificers. It is with the view of enabling others to construct the instrument efficiently, as well as to think of remedies for any other difficulties which may occur, that I have allowed myself to indulge in so much narrative and detail of particulars.

I think that it may be found that the instrument will have the advantage of being little liable to accident in travelling, and the thermometer is very securely packed in a proper case.

I should be very sorry to have it thought, that in any observations which I have made regarding Mr Daniell's hygrometer, I should be supposed to speak in the slightest degree in a tone of disparagement of that instrument. On the contrary, I think that I have shewn that, with a slight allowance, it is perfectly trustworthy; and I firmly believe that so beautiful and philosophical an instrument will never be entirely superseded. If that which I have described shall ever be thought to have an advantage over it in any respect, it will, I think, be on the score of little tendency to injury, and probably to greater facility of observation, after a little dexterity of manipulation has been acquired by practice. There is undoubtedly an occasional difficulty in noting the results by Daniell's instrument. I must, however, take the liberty of repeating my protest against a view which I fear there is ground for thinking some English meteorologists have quite made up their minds to adopt, that the indications of Daniell's hygrometer are quite accurate, and ought to be taken as the general standard of comparison, according to which the accuracy of all other dew point instruments, and

of all dew point formulæ and tables of factors is to be determined. The grounds of my dissent from such a view have been already given.

There is a slight modification of the manner of applying the ivory intercepting portion, by placing it as an ivory collar in the neck of the bottle at the point P, instead of at N, as formerly described. It ought to be $\frac{1}{5}$ of an inch broad. In this case the valve piece N is of brass, so that the risk of its accidental fracture, in screwing or unscrewing, is avoided, but in these operations the ivory portion of the neck of the bottle should not be grasped. I am not yet, however, prepared to recommend either of these modifications in preference to the other. Both, generally speaking, answer perfectly well.

St Andrews, 25*th February* 1854.

Postscript.—After the paper was read at the meeting of the Royal Society, I was asked by a gentleman present, whether the instrument would answer for an Indian climate, where extreme cases of great dryness of atmosphere sometimes occur? I, of course, answered that I had had no opportunity of making experiments under such circumstances as an Indian climate might occasionally present; but that if the present size of the syringe was found not to render it sufficiently powerful for such extreme cases, I had little doubt that by some such increase of the size of the syringe as formerly suggested, the necessary augmentation of power might be attained. I may add, that in the event of the occurrence of very extreme cases, where all ordinary hygrometers might fail to give the necessary indication, the object, there is little doubt, might be accomplished, by means of the arrangement which I suggested several years ago, as mentioned in the commencement of this paper, employing instead of a bottle of brass, one of thin glass, or still better, of platinum, somewhat larger than that described in the notice referred to, and introducing into it some mixture capable of producing great cold. One of phosphate of soda, nitrate of ammonia, and diluted nitric acid, is capable of reducing the temperature from 50° to -20°; and from higher temperatures would produce a proportional reduction; and if pounded ice with the proper acid were used, still greater cold would result.

IV.—*On the Action of the Halogen Compounds of Ethyl and Amyl on some Vegetable Alkaloids.* By Mr Henry How, Assistant to Professor Anderson, Glasgow University.

(Read 1st May 1854.)

The memoir which I have now the honour of presenting to the Society contains part of a continued investigation, of which the first results were published in a paper read, last year, before the Chemical Society of London.* The nature of the materials experimented on is such, and the scope of the subject so large, as to afford nothing more to all I have at present brought to light than the name of a bare and partial outline; yet the facts I have now to adduce, of some interest in themselves, will increase that of those already made known, and prove of additional service in aiding to complete that desirable object, a knowledge of the chemical constitution of the natural bases. In the paper alluded to, I shewed that, by the action of iodide of ethyl and of methyl upon morphia and codeine, the two most prominent alkaloids of opium, new salts are produced, in which the basic molecules appear to assimilate by their chemical characters to the hypothetical metal ammonium, or its oxide rather; so that, in the system of Hofmann, these peculiar results of natural, and as yet inimitable agencies, would take rank among the nitryle bases. I say as yet inimitable, because it appeared that although one of the new salts possessed precisely the same centesimal composition as the corresponding salt of codeine, the base contained in the artificial product was widely different in appearance and properties from this alkaloid; a fact which seems to militate against a hope of forming the natural bases by this or similar means, and to show that the building up of these bodies is a process peculiarly the function of the *vis vitæ* of the plants producing them; at least that, easy as the transition of morphia into codeine appears to be on a comparison of their respective rational formulæ, and since the adding the required two equivalents of carbon and hydrogen to the former proves of ready performance, the cause of the failure lies deeper than in this difference merely, and that there are peculiarities in the construction of the primary molecules which we are at present as far as ever from being able to imitate. I further remarked, that so far as the amount of basic hydrogen, or compounds supplying its place, is concerned, the fact that it proved the same in these two alkaloids was possibly to be anticipated from their similar origin; and that I intended pursuing the subject with a view of ascertaining if all the bases of the same plant were so far analogous. It will be seen that they do not appear to be

* Quart. Journ. Chem. Soc, vol. vi.

so in reality, my attempts at forming corresponding compounds with some other alkaline principles of opium having been as yet unsuccessful. Another order of plants, however, has furnished me a base, strychnine, which seems in this respect similarly constituted with morphia and codeine, inasmuch as, under the same circumstances, it yielded a new combination, whose stability in the free state, and in some of its salts, caused it to afford more decided results than I was able to obtain with the analogous products from these alkaloids.

This investigation was pursued in the laboratory of Professor ANDERSON, in the University of Glasgow, to whom my cordial thanks are due for the interest he took in its progress, and, among other things, for the facilities he afforded me in placing at my disposal specimens of subjects for experiment.

In describing the individual re-actions, salts, &c., I have preferred giving them, as nearly as is consistent with clearness, in the order in which they were investigated.

Behaviour of Papaverine with Iodide of Ethyl.

Hydriodate of Papaverine.—Next to morphia and codeine, papaverine is the opium alkaloid possessing the most marked characters, and highly basic properties, and as I was acquainted with the beautiful substitution products it yielded to Professor ANDERSON, of which an account has been given to this Society,* I chose it as the subject for further experiment. Accordingly, some of the base, in small crystals, was placed in a tube, spirit of wine poured on it, and iodide of ethyl added; a great part of the crystals was seen to disappear immediately, and when the sealed tube was placed in boiling water, solution became rapidly complete. The clear fluid was allowed to boil about half an hour, but no solid made its appearance, either during the continuance of the heat, or upon the subsequent cooling and standing of the vessel. The solution was distilled finally in a flask to a very small bulk, and the syrup which remained soon solidified into a crystalline mass; this was found to be perfectly soluble in hot water—papaverine itself being insoluble—and in spirit of wine; but, from peculiarities to be mentioned presently, absolute alcohol was preferred as a solvent. From a concentrated hot solution in this menstruum, the new product was obtained, on cooling, in rhombic crystals, a portion of which, as they proved to be an hydriodate, was submitted to analysis in the ordinary way, with the following result:—

{ 5·558 grains, dried *in vacuo*, gave
{ 2·780 ... iodide of silver,

which gives a percentage of iodine of 27·02, and 27·21 is the theoretical calculation corresponding to the anhydrous hydriodate of papaverine, whose formula is

$$C_{40}\,H_{21}\,NO_8\,III.$$

* Trans. Roy. Soc. Edin., vol. xxi., part i.

I shall shew presently, from an analysis and description of the base obtained from this salt, that there can be no doubt of its real nature; so that, under these circumstances, papaverine behaves in a manner similar to that shewn before* to be the case when hydrated morphia is placed in contact with chloride of amyl, it forms a salt, namely, with the halogen of the ether compound, while no doubt the corresponding alcohol is simultaneously generated by the agency of the elements of water, as in the equation—

$$C_{40}\,H_{21}\,NO_8 + C_4\,H_5\,I + 2\,HO = C_{40}\,H_{21}\,NO_8\,HI + C_4\,H_6\,O_2;$$

this, at least, seems the most obvious explanation of the change when water is present. To determine if the formation of the salt were really due to the agency of water, I repeated the experiment with absolute alcohol in place of rectified spirit, and the papaverine was dried at 212°, to remove accidental moisture, its crystals containing no water of combination. In this case, also, the hydriodate was formed from part of the alkaloid, the rest remaining unaltered. The iodide of ethyl had been distilled from chloride of calcium, and I should think could furnish, at the most, but a minute quantity of water; and it is not easy to see how the reaction is brought about under these circumstances, unless it be assumed that it occurs with the formation of ether. The detection of this substance would not be readily effected upon the small scale upon which I worked, and I therefore only throw out the following equation as a possible explanation of the reaction—

$$C_{40}\,H_{21}\,NO_8 + C_4\,H_5\,I + C_4\,H_6\,O_2 = C_{40}\,H_{21}\,NO_8\,HI + 2\,C_4\,H_5\,O.$$

As no account has yet been given of the hydriodate of papaverine, a short description of it may be subjoined. The salt is extremely soluble in boiling water, and the moment the heat is withdrawn from a strong solution, the fluid assumes a milky appearance, and an oil is soon deposited, which, after some hours, passes into the form of stellate colourless needles; it is also soluble in spirits of wine, but in absolute alcohol it dissolves with far less facility, and it requires protracted boiling, when the salt has once taken on the crystalline condition, to obtain perfect solution with this menstruum. The hot liquid deposits the salt rapidly, as it cools, in colourless rhombic crystals. When exposed to a temperature of 212° Fahrenheit, in a dry state, the hydriodate of papaverine assumes a brown tint, loses slightly in weight, and is found to have undergone some decomposition, as it no longer dissolves completely in hot water, but leaves a brown resinous matter.

By addition of ammonia to the mother liquors of the salt which had been analysed, a white crystalline precipitate was obtained; this was recrystallized from dilute spirit, and burnt with chromate of lead:—

* Quart. Journ. Chem. Soc., vol. vi.

{ 3·552 grains, dried at 212°, gave
9·200 „ carbonic acid, and
2·095 „ water.

	Experiment.	Calculation.		
Carbon, . .	70·63	70·79	C_{40}	240
Hydrogen, . .	6·55	6·20	H_{21}	21
Nitrogen, . .		4·10	N	14
Oxygen, . .		18·91	O_8	64
	100·00	100·00		339

A glance at the result, and the appended calculation of the numbers required by the formula of papaverine, leaves no doubt as to the base of the salt. I was perfectly satisfied as to its nature, from its appearance and some peculiar well marked reactions characteristic of papaverine; thus, with strong sulphuric acid it yielded a splendid purple fluid, at the instant of solution, and when heated with a little nitric acid, became converted into a mass of yellow crystals, which are nitrate of nitropapaverine, described by Dr ANDERSON.*

Behaviour of Narcotine with Iodide of Ethyl.

Hydriodate of Narcotine.—The next alkaloid of opium submitted to examination was narcotine; and the following experiment shews its deportment to be perfectly similar to that of papaverine.

Some narcotine in fine powder was heated in a sealed tube with absolute alcohol and iodide of ethyl; after about ten minutes exposure to a temperature of 212°, the whole of the base disappeared; but to insure complete decomposition, the heat was continued some twenty minutes longer. When the tube had cooled, and stood a short time, groups of four-sided colourless prisms appeared. On opening the vessel, and submitting these to examination, they proved to be unchanged narcotine, while their mother liquor was found to contain an hydriodate. The excess of iodide of ethyl and alcohol was accordingly distilled off, and the remaining syrup tested as to its nature; it was almost entirely soluble in boiling water, leaving only a little narcotine, and the solution contained the hydriodate of that base.

This salt is deposited, from a concentrated fluid, as an oil which does not become crystalline, and when its solution is evaporated, spontaneously or at 212°, an oily mass is also obtained, which does not crystallize from alcohol, or ether, or a mixture of these substances. Owing to its occurring only in this amorphous state, I did not attempt to submit it to analysis; but its solution yielded to ammonia a precipitate easily to be recognised as narcotine, by its characters and chemical reactions; and in order fully to substantiate the nature of the base, the hydriodate was converted into a platinum salt in the following manner:—

* Loc. cit.

An aqueous solution was treated successively with nitrate of silver and hydrochloric acid, and bichloride of platinum was finally added to the clear fluid, care being taken to operate with dilute liquids and in the cold, as the platinum compound of narcotine is very prone to change. An amorphous yellow precipitate resulted, which was quickly collected, washed, and dried; it gave these numbers on ignition,—

{ 8·375 grains, dried at 212°, gave
{ 1·330 „ platinum.

which lead to a percentage of 15·88; and 15·81 is that required by the salt of narcotine, whose formula is

$$C_{46}\,H_{25}\,NO_{14},\,HCl,\,Pt\,Cl_2.$$

To the nature of the reaction in this case, I would apply the remarks made with reference to papaverine under similar circumstances; but, perhaps, should not permit the subject to pass without drawing attention to the relation my experiment may bear to a fact made known some time since by Wertheim.

This chemist announced, in a preliminary notice,* that he had detected in opium two new species of "narcotine," which, from their composition, he named methylo and propylo-narcotine, while he was led to consider the ordinary alkaloid, hitherto known, as ethylonarcotine: the whole being derived from one fundamental substance. I do not think the detailed account of his experiments has yet been published, but it would certainly be interesting to establish the existence of this series in reality, because, in that case, the ordinary narcotine, with which I worked, would seem to have no longer any replaceable hydrogen, and be the first instance of a natural, or indeed of any compound nitrogenous analogue of ammonium or a metal, unless papaverine be similarly constituted. In the absence of positive proofs, resulting from experiment, nothing can at present be said further as to the nature of these known and unknown bases; but the question seems to me very interesting and worthy of being settled decisively, Wertheim's being an unsupported statement, particularly as, chemically, there is nothing to distinguish narcotine and papaverine from other natural alkaloids, as a class, or to assimilate them to ammonium.

Behaviour of Cotarnine with Iodide of Ethyl.

Hydriodate of Cotarnine.—Cotarnine is a base derived from narcotine by oxidation; some of it was treated exactly in the manner just described. It was found to be completely converted into an hydriodate, which remained, on distilling off the excess of alcohol and iodide of ethyl, as a red-brown oily mass, highly soluble in hot, insoluble in cold, water, and not taking on the crystalline state under any circumstances. This salt was the hydriodate of cotarnine; but being of itself no more

* Journal für Praktische Chemie, vol. liii., p. 431; Chem. Gazette, 1852, p. 36.

fit for analysis than that of narcotine, the composition of the base was ascertained by converting it into a platinum compound, which was a very pale yellow amorphous substance, and gave the following result on analysis:—

3·265 grains, dried at 212°, gave	
0·720 „	platinum,

equal to a percentage of 22·38; 22·57 corresponds to the formula of the cotarnine salt,

$$C_{26}\,H_{13}\,NO_6,\,HCl,\,Pt\,Cl_2.$$

This artificial product then comports itself exactly like the alkaloids papaverine and narcotine; and the reaction is no doubt of the same nature.

The formation of these salts of the alkaloids, in cases where water is present, is possibly brought about by a change, observed by FRANKLAND* to take place between iodide of ethyl and water in a sealed tube at 300° Fahr.; the results of this reaction are, according to this chemist, hydriodic acid and ether. It seems not improbable that the presence of a basic substance may determine this decomposition at a much lower temperature.

On Strychnine.

Having examined the action of the alcohol iodides on a number of bases with only one equivalent of nitrogen, I was anxious to ascertain the deportment of a base containing, like strychnine, two atoms of this element, in the hope of throwing some light on the function of the second equivalent. The researches of Hofmann, on a great variety of alkaloids, have shewn us that the volatile bases starting from the original type ammonia, and passing upwards to the most complicated substances, may be viewed as nitrogen attached to basic hydrogen alone, or to it in combination with hydrocarbons performing its functions, or finally to hydrocarbons occupying the entire hydrogen part of the molecule. In the fixed vegetable alkaloids, we see oxygen also included in the system; and if, as I have attempted to prove in the case of some of these, they are comparable with nitryle bases, the hydrogen part of the compounds must contain oxygenized hydrocarbons acting as hydrogen. In the case of a base containing two atoms of nitrogen, it is possible that this element performs as it were two parts; one may be referable to the function of that in any ammonia, amidogen, or nitryle base, while the other may be more analogous to that property in virtue of which, in combination with oxygen, as NO_4, it replaces an atom of hydrogen in the carbohydrogen forming part of the compound molecule. The

* Gerhardt Suite de Berzelius, ii., p. 323.

possibility of the latter being the case, as regards one of the atoms of nitrogen in an alkaloid containing two, was thrown out as a speculative suggestion some few years ago by FRESENIUS.

In the hope of arriving at some conclusions regarding these points, I proceeded to examine the deportment of strychnine with reagents suited to my purpose. The decision of the whole question seems to devolve upon the reactions of two classes of agents; the amount of basic hydrogen would be learned by the quantity of hydrocarbons of alcohol radicals the base is capable of taking up from their halogen compounds, while reducing agents should remove oxygen from any oxidized combination of nitrogen; if it be as NO_4, that the second atom of this element exists, sulphuretted hydrogen should, as in other compounds of this class, remove the four equivalents of oxygen, and permit two additional atoms of hydrogen to enter into the new product.

The first part of the question is gone into in some detail in the present memoir, while, without being in a position to give anything decisive at present with regard to the second, I may mention that strychnine undergoes a curious decomposition in contact with sulphide of ammonium, which results in the formation of hyposulphite of the base itself,—a very beautiful and stable salt,—and the production of another substance I am still engaged in studying. The description of the properties and composition of these products I must reserve till I am able to shew the complete history of the change, and how far the real constitution of strychnine is determined by the experiments; but I may state that, from what I have as yet learned, the decomposition does not appear to be of the nature just spoken of.

Action of Iodide of Ethyl on Strychnine.

Hydriodate of Ethylostrychnine.—Strychnine, in a state of fine powder, is readily attacked by iodide of ethyl; even when these two substances are boiled together in water, a perceptible quantity of iodine is found in the solution, precipitable by silver salts; the insolubility of the alkaloid, however, no doubt interferes with the reaction, for the change is much more easily effected when spirit of wine is used as a medium. The most successful method I found to be that employed before, viz., to operate with alcohol and iodide of ethyl on the powdered base in sealed tubes; at the temperature of boiling water the change is complete in about twenty minutes. A heavy crystalline powder is formed, differing in appearance from strychnine; but solution is not at any time during the experiment complete, no change being perceptible beyond the more definitely crystallized nature of the solid contents of the tube, the complete solubility of which in boiling water announces the completion of the reaction.

A boiling aqueous solution of the new substance having deposited, upon cooling, masses of silky white crystals, which proved to consist of an hydriodate, they

were collected, washed, and dried, and a portion of them was submitted to analysis in the ordinary way.

4·325 grains, dried at 212°, gave
8·925 „ carbonic acid, and
2·180 „ water.
4·565 „ dried at 212°, gave
2·195 „ iodide of silver.

	Experiment.	Calculation.		
Carbon,	56·27	56·31	C_{46}	276
Hydrogen,	5·60	5·50	H_{27}	27
Nitrogen,		5·71	N_2	28
Oxygen,		6·55	O_4	32
Iodine,	25·98	25·93	I	127·1
	100·00	100·00		490·1

A comparison of the above results and appended calculation will shew that a replacement of hydrogen by ethyl in, or an attachment of iodide of ethyl to, the alkaloid has taken place, according to the equation,

$$\underbrace{C_{42}\,H_{22}\,N_2\,O_4}_{\text{Strychnine.}} + C_4\,H_5\,I = \underbrace{C_{42}\left\{\begin{matrix}H_{21}\\C_4\,H_5\end{matrix}\right\}N_2\,O_4,\,HI = C_{46}\,H_{27}\,N_2\,O_4\,I.}_{\text{New Salt.}}$$

The salt crystallizes without water, and whatever constitution subsequent experiment may assign to it, it will be now described as an hydriodate, and its base as ethylostrychnine.

Hydriodate of ethylostrychnine is soluble in about 50 or 60 parts boiling water, and in about 170 of water at 60°; a tolerably dilute fluid deposits the salt in very fine white four-sided prisms of considerable lustre, which they retain in the dry state; it is also soluble in rectified spirit, and comes out of that menstruum in short prismatic crystals. It is unaltered in the air; at 212° it loses no weight, but acquires a slight shade of colour; at a higher temperature it fuses and blackens, affording thick vapours of an alkaline reaction, and disagreeable odour; and a yellowish sublimate, rather oily in appearance, forms on the sides of the vessel; no vapours of iodine are to be observed during the process of heating.

This salt gives no base with potass or ammonia, but is less soluble in these alkaline fluids than in water, and is consequently precipitated, on their addition, in strong solutions; strong potass throws it down at once in the cold, and when ammonia is added to a concentrated boiling aqueous solution, the unchanged salt deposits immediately in fine needles. It is readily decomposed by oxide of silver, and the hydrated base may be obtained in the crystalline state. These reactions assimilate the salt to an iodide rather than to an hydriodate, and the characters of the base, when isolated as far as it can be, being those of an analogue of ammonium oxide, its salts would perhaps be more correctly called

iodide, chloride, &c.; and in the following pages I make use of these terms, without, however, making any alteration in the conventional nomenclature for the base.

The salts of ethylostrychnine, generally, are characterized by their beautifully crystalline nature, and the facility with which they may be obtained pure. I have analysed a few, as affording a favourable opportunity of examining the combinations of one of these derivatives from a natural fixed alkaloid.

Nitrate of Ethylostrychnine.—This compound is readily formed by double decomposition from the preceding, by use of nitrate of silver in warm dilute solutions. It is easily soluble in boiling water, but of extremely sparing solubility in this menstruum when cold; so much so, that I have used this property as a test of the existence of the base on many occasions; a solution containing it, even when tolerably dilute, depositing, on the addition of a little nitric acid, in a short time, very fine four-sided prismatic crystals; in more concentrated fluids their appearance is immediate. The salt, in a pure state, is in colourless prisms of high refractive power, which may be obtained of great beauty from dilute aqueous fluids. Its analysis gave the following results :—

4·555 grains, dried at 212°, gave
10·790 ... carbonic acid, and
2·680 ... water.

	Found.	Calculation.		
Carbon, . . .	64·60	64·94	C_{46}	276
Hydrogen, . .	6·53	6·35	H_{27}	27
Nitrogen, . .		9·88	N_3	42
Oxygen, . . .		18·83	O_{10}	80
	100·00	100·00		425

which agree precisely with the formula—

$$C_{46}\,H_{26}\,N_2\,O_4,\,HO,\,NO_5 = C_{46}\,H_{27}\,N_2\,O_4\,NO_6.$$

Its crystals contain no water.

Chromates of Ethylostrychnine.—Both neutral and acid chromate of potass yield precipitates with salts of the base; the former giving, even in dilute fluids, short prismatic yellow crystals, and the latter tufts of silky needles. As the number of bichromates, of whose analysis we are in possession, is in reality very small, being limited, I believe, to those of potass and ammonia, whose constitution is peculiar, I examined the salt in the present instance, to see if it were quite analogous to these compounds, which it does not appear to be.

Bichromate of ethylostrychnine is deposited from strong mixed solutions of the appropriate salts in very beautiful transparent plates of a golden yellow colour; it falls, as just mentioned, from more dilute fluids, in tufts of needles; it is readily

soluble in boiling water, but when cold, this liquid dissolves but little of the salt. The following is the analysis :—

I. { 5·173 grains, dried at 212°, gave
10·840 ... carbonic acid, and
2·755 ... water.
5·885 ... dried at 212°, gave, on ignition,
0·938 ... sesquioxide of chromium.

II. { 4·480 ... dried at 212°, when ignited, gave
0·715 ... sesquioxide of chromium.

	Experiment.		Calculation.		
	I.	II.			
Carbon, ...	57·15		57·33	C_{46}	276
Hydrogen, . . .	5·91		5·81	H_{28}	28
Nitrogen, ...			5·81	N_2	28
Oxygen, ...			29·99	O_6	48
Chromic acid, . . .	20·86	20·88	21·06	$2(CrO_3)$	101·4
	100·00	100·00	100·00		481·4

which gives results according satisfactorily with a formula which contains an atom of water more than those of the corresponding potass and ammonia salts, and may be thus written :—

$$C_{46}\,H_{26}\,N_2\,O_4\,HO,\,CrO_3,\,HO\,CrO_3 = C_{46}\,H_{27}\,N_2\,O_4\,CrO_4,\,H\,CrO_4.$$

The failure of the analogy here, with the peculiar combinations of potass and ammonia, is worthy of attention; of course, it readily admits of explanation, if the one atom of water be assumed as retained from the water of crystallization of the new salt. The loss sustained by exposing the crystals to heat was as follows :—

I. { 4·660 grains, lost at 212°,
0·160 ... water

II. { 11·510 ... lost at 212°,
0·435 ... water

	I.	II.
Percentage of water,	3·43	3·77

and 3·60 is the calculated percentage corresponding to a loss of two atoms of water by a salt of the formula,—

$$C_{46}\,H_{27}\,N_2\,O_4\,CrO_4,\,H\,CrO_4 + 2\,aq.$$

Platinum Salt of Ethylostrychnine.—By the successive addition of nitrate of silver and hydrochloric acid to a warm solution of the iodide, and that of bichloride of platinum to the clear fluid, this salt fell as a curdy yellow precipitate, which became crystalline after some hours; from more dilute liquids it crystallizes at once in a very beautiful form, namely, in groups of stars, of which the individual

rays resemble, on being magnified, the fronds of some species of ferns. The platinum alone, in this compound, was determined,—

{ 3·390 grains, dried at 212°, gave
{ 0·592 ··· platinum.

The percentage resulting from this experiment is 17·46; and 17·37 is that corresponding with the formula,—

$$C_{46}\ H_{27}\ N_2\ O_4\ Cl,\ Pt\ Cl_2.$$

The salt is anhydrous when crystallized.

The substitution of terchloride of gold for the platinum solution gives, by the same process, a yellowish curdy precipitate, soluble in boiling water; and this fluid deposits colourless brilliant prisms of splendid appearance, of the corresponding gold compound.

The chloride is a very soluble salt, crystallizing in needles; the sulphate is less soluble, an acid fluid furnishing groups of pearly needles and plates, possibly a bisulphate; the oxalate, prepared in a similar manner, has much the same appearance; the acetate remains as a gummy transparent mass when a proper solution is evaporated to dryness at 212°; the chloride gives, with mercuric chloride, a curdy white precipitate, crystallizable from hot water in white needles.

Carbonates of Ethylostrychnine.—Having observed, in the course of experiments to be detailed presently, which were made for the purpose of obtaining the ethylostrychnine in an isolated state for analysis, the tendency of the base, when free and in solution, to absorb carbonic acid, it occurred to me to attempt the production of any carbonates of determinate composition it might be capable of forming. I shall be able to shew that the base enters readily into combination with this acid, forming at least two salts, of which while the one, the monocarbonate, cannot be obtained in the dry state, the other, a bicarbonate,—into which, with the occurrence of some other product of decomposition, the first appears to have a great tendency to pass,—may be readily procured in the solid form, of determinate composition, and even as a very beautiful crystalline substance.

It was found that when moist carbonate of silver is added to iodide of ethylostrychnine, covered with a little water, double decomposition ensues, and is complete in a few minutes, when the vessel containing the mixture is well shaken: iodide of silver, of course, remaining undissolved, while the fluid contains a carbonate of extreme solubility. When, without employing heat, the process is quickly performed, so as to occupy only a few minutes of time, a perfectly colourless liquid is obtained; but with a longer contact of the excess of carbonate of silver, or the application of heat, the fluid assumes a light claret colour, which, by the continued action of either of the above causes, may be deepened almost indefinitely, and seems to be the result of oxidation. This solution shews all the characters of a

carbonate in its reactions, and when saturated with dilute nitric acid, becomes filled, almost before the cessation of effervescence, with the characteristic crystals of the nitrate of ethylostrychnine; when evaporated to dryness, however, either *in vacuo* or at 212 Fahr., it is found to have undergone a singular decomposition. The residue obtained in both cases is crystalline, and in part colourless, with claret-coloured spots pervading its mass; on adding water to this, the great bulk dissolves to a deep claret-coloured fluid, which contains carbonic acid and ethylostrychnine, while an amorphous substance, in whitish flocks, remains in appreciable quantity.

It is obvious that, with the observation of this decomposition, all hope of analysing the expected residue of pure monocarbonate of ethylostrychnine was at an end; nevertheless, as I was curious to know the actual amount of carbonic acid retained, a certain weight of the residue, obtained by evaporation *in vacuo*, was treated with cold water, and to the filtered solution was added a mixture of chloride of barium and caustic ammonia, which had been suffered to remain in contact for some time; the precipitate produced would contain all the carbonic acid, and when collected and washed with the proper precautions, it was ignited; the resulting percentage, calculated on the amount of residue employed, was almost that of the bicarbonate to be described immediately.

The flocky matter remaining undissolved by the water appears to be a new base; it does not effervesce with acids, but dissolves tranquilly and completely, and is again thrown down by ammonia in white flocks; with nitric acid, it yields no crystalline salt, as ethylostrychnine does; it dissolves with the greatest ease in spirits of wine, but is not to be obtained from this menstruum in a crystalline state; the solution, which is, or soon becomes, of a fine pink colour, deposits no crystals on standing for weeks even, but red transparent drops appear at the sides and bottom of the vessel, and an amorphous residue remains on evaporating the fluid to dryness; these characters evidently separate the product widely from strychnine and ethylostrychnine, and must cause it to be considered a peculiar base. It would be an interesting decomposition to follow out, but I had far too little substance at my command to do more than observe the above facts at the time, and the consumption of material necessary for its investigation is an obstacle to the research.

Bicarbonate of Ethylostrychnine.—The preceding experiment having shewn the tendency of the base to accumulate carbonic acid, I proceeded to seek a higher carbonate, by passing a stream of the washed acid gas into the solution, obtained by the double decomposition just mentioned. During this process, a very slight turbidity ensued, and after the gas had passed through the fluid a considerable time, this was filtered and obtained perfectly colourless; it proved to admit of evaporation to dryness, *in vacuo* or at 212°, with similar results as to the properties and composition of the residue. In the former case, a white crystalline mass.

—having, however, here and there, faintly claret-coloured spots—remains, which, by continued drying, becomes a white enamel-like substance; the latter process yields a crystalline residue of a reddish-yellow colour; these products are the bicarbonate. The salt undergoes in a very slight degree the decomposition before spoken of, but this is scarcely perceptible when evaporation is quickly performed; the flocky base produced, at 212° especially, being exceedingly minute in quantity.

The aqueous solution of the bicarbonate. and, though not deliquescent, the salt is extremely soluble in water, has a powerful alkaline reaction, and proves a ready means of forming the compounds too soluble to be got by double decomposition. The dry salt dissolves completely in a little absolute alcohol; the addition of good ether to this fluid causes in a little time the deposition of bicarbonate in very fine colourless prismatic crystals, of great refractive power. In the analyses which follow, the carbonic acid was estimated as before mentioned, and the specimens were of different preparation:—

I. { 4·525 grains, dried *in vacuo*, gave
   { 2·150 ... carbonate of baryta.

II. { 4·990 ... dried at 212° Fahr., gave
    { 2·330 ... carbonate of baryta.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .			65·09	C_{46}	276
Hydrogen, . . .			6·60	H_{28}	28
Nitrogen, . . .			6·60	N_2	28
Oxygen, . . .			11·34	O_6	48
Carbonic Acid, . .	10·62	10·44	10·37	$2(CO_2)$	44
			100·00		424

These numbers agree perfectly with those required theoretically by a salt of the base analogous to bicarbonate of potass, as expressed in the formula—

$$C_{46}\,H_{26}\,N_2\,O_4\,HO\,CO_2,\ HO\,CO_2 = C_{47}\,H_{27}\,N_2\,O_4\,CO_3,\ HCO_3.$$

This salt is somewhat interesting, inasmuch as it is, I think, the first combination of one of the artificial ammonium bases with carbonic acid which has proved definite by analysis. While on the subject, it will not be out of place to mention some experiments made with the natural alkaloids, for the purpose of preparing, if possible, similar salts with these bodies, as it gives the opportunity of correcting an error to be found in the manuals. Thus, it is stated in Liebig's Traité,* that a carbonate of strychnine is obtained both by precipitation of its salts with carbonated alkalies and solution of the base itself in water, through which a stream

* Liebig. Traité de Chimie Organique, par Gerhardt ii., p. 630.

of carbonic acid gas is passed, it being deposited in this case in beautiful crystals. I have carefully gone over those experiments, and satisfied myself that the substance in both cases is the pure base. A carbonate is certainly obtained by double decomposition, between hydrochlorate of strychnine and carbonate of silver, but it exists only for a short time, even in solution, as the gas escapes on standing, and the pure alkaloid is deposited in fine, though small, crystals.

I could obtain no dry carbonates of morphia, codeine, papaverine, or narcotine; they behave like strychnine under the same circumstances. While these experiments were in progress, my attention was called to the production of a definite carbonate of quinine by LANGLOIS,* and I gave up the idea I had entertained of extending the observations to the vegetable alkaloids generally, as it is possible the subject will be gone into at greater length by this chemist.

Hydrate of Ethylostrychnine.—I have mentioned already that potass and ammonia fail to separate this base from its iodine combination, which is thrown down unaltered from its aqueous solution by strong potass, and is also less soluble in ammoniacal than in pure water. Oxide of silver readily effects the elimination of the iodine; in fact, when the solid salt is covered by moist oxide of silver, a few minutes' contact, in the cold, suffices to complete the change, iodide of silver remaining undissolved, and a fluid of a red purple colour being formed, which contains all the base. When this is suffered to evaporate to dryness spontaneously, a crystalline purplish mass results, which contains some carbonic acid, and dissolves in a small quantity of cold water; leaving, however, constantly, a minute residue of the nature before spoken of in describing the monocarbonate. Evaporation *in vacuo* over sulphuric acid produces a less crystalline residue, of a similar colour, which is found to be completely soluble in hot absolute alcohol to a purple fluid; a concentrated solution of this kind deposited, on cooling, a substance in colourless, small, prismatic crystals. These were collected on a filter, washed from the coloured mother liquor with a little absolute alcohol and ether, and finally dried out of contact of air. Tests, to be mentioned presently, having shewn the deposit to be, as nearly as it is possible to be procured, the pure base, it was retained *in vacuo* till it ceased to lose weight, and then gave these results on analysis:—

{ 4·643 grains, dried *in vacuo*, gave
11·600 ... carbonic acid, and
3·425 ... water.

	Experiment.	Calculation.		
Carbon,	68·13	67·81	C_{46}	276
Hydrogen,	8·19	7·61	H_{31}	31
Nitrogen,		6·87	N_2	28
Oxygen,		17·71	O_9	72
		100·00		407

* Chemical Gazette, 1853, p. 470.

I must mention that the complete desiccation of this substance was extremely tedious, occupying above a fortnight, and that in the state in which it was burned, it was a very light and bulky powder, highly electrical and hygroscopic, so that the high percentage of hydrogen is not remarkable. The numerical results of the above analysis admit of translation into the formula, which, in analogy with the corresponding combination of potassa, the assumed congener of ethylostrychnine, may be thus written :—

$$C_{46}\,H_{27}\,N_2\,O_4\,O,\,HO + 3\,aq.;$$

and this hydrate of oxide of ethylostrychnine differs from the known crystallized hydrate of potassa, in containing an atom of water less.

The substance as analysed was not deliquescent; it dissolved immediately on being covered with cold water, furnishing a red fluid, and leaving only a minute flocky residue, which seemed to increase on application of heat; the solution, made in the cold, became filled, almost immediately on the addition of a little nitric acid, with the crystalline salt before spoken of as characteristic of the base, and no effervescence was perceptible.

When the hydrate, carefully dried *in vacuo*, is dissolved in a quantity of absolute alcohol, sufficient to prevent a deposit on cooling, the addition of some ether causes the formation of a tremulous jelly, which renders the whole fluid semi-solid. On stirring for some time with a glass rod, this jelly gradually changes into a crystalline substance; by management of the respective amounts of alcohol and ether, very beautiful small crystals, perfectly colourless, may be obtained at once, or on standing. To effect this result the hydrate must be well dried, or it falls in purple, oily drops.

The hydrate cannot be freed of its water of crystallization by exposure to 212°, as at this temperature it undergoes decomposition in a much greater degree; a portion so heated was found to effervesce in a lively manner with acids, and though it yielded plenty of the crystalline nitrate, yet when treated with cold water it left a considerable residue of the character already described. I made an analysis of a part of the substance giving these reactions with a view of ascertaining how far decomposition had proceeded, and give it with the understanding of its approximative value :—

4·925 grains, dried at 212°, gave
12·600 ... carbonic acid, and
3·480 ... water.

	Found.	Calculation.		
Carbon,	69·77	69·34	C_{46}	276
Hydrogen,	7·85	7·53	H_{30}	30
Nitrogen,		7·05	N_2	28
Oxygen,		16·08	O_8	64
		100·00		398

These numbers agree closely with those corresponding to a formula containing an atom of water less than the preceding,

$$C_{46}\,H_{27}\,N_2\,O_4\,O,\,HO + 2\,aq.;$$

and it is just possible that the errors occasioned by the presence of carbonic acid and the basic product of decomposition, both of which, after all, bear but a small proportion to the whole substance, may counterbalance each other; I leave the fact of the analysis, since it is some sort of control over that immediately preceding.

A freshly prepared solution of the base, by action of oxide of silver on the iodide, has a red purple colour and an extremely bitter taste; its reaction is highly alkaline. It yields no immediate precipitate with solutions of barium and calcium chlorides, but causes partial precipitation on application of heat; it throws down from sulphate of magnesia, in the cold, a flocky precipitate, and also gives with solutions of the heavy metallic oxides, and with alumina, immediate deposits; but I had not material sufficient to determine its solvent power in particular cases. On being boiled, it evolves the odour of a volatile base. With chlorine, bromine, and iodine, it yields products. When it, or its carbonate, is treated with hydrosulphuric acid, and suffered to stand, a hyposulphite is found to result, which may be obtained crystalline by evaporation, and is soluble in alcohol. This base gives the same reaction as strychnine with sulphuric acid and chromate of potass.

When iodide of ethylostrychnine is heated in a retort with excess of soda-lime, a heavy oil distills over which is partly basic and partly insoluble in acids: the hydrochloric solution of the soluble portion gave with bichloride of platinum an uncrystalline salt, but I had not sufficient to determine if, in this decomposition, the ethyl molecule attaches itself to the volatile base, which is no doubt either leucoline or ethyloleucoline.

Action of Iodide of Ethyl on Ethylostrychnine.—This reaction appears to result in the formation of iodide of ethylostrychnine and some other product. I employed for the experiment the mother liquor of the hydrate which was analysed: this alcoholic fluid was sealed up in a tube with iodide of ethyl, and the whole was heated for half-an-hour in boiling water. The liquid remained clear while hot, but soon became turbid on cooling, and finally deposited some rounded transparent yellowish grains. These proved, when separated from the mother liquor by decantation, to be completely soluble in hot water; their solution deposited a little flocky matter, and when filtered from this and evaporated, groups of stellate crystals. These crystals agreed in characters with iodide of ethylostrychnine, and furnished with oxide of silver a caustic yellow liquor, which became filled with prismatic crystals on the addition of a little nitric acid. The alkaline solution

had not, it is true, the rich red-purple afforded in former instances, but in all other characters was quite the same.

The mother liquor of the grains, when distilled to a syrup, and, after the addition of some water, finally evaporated to dryness, left a red and green resinous residue, which dissolved incompletely in hot water; the aqueous solution again left a resin, only partially soluble, on evaporation; there exists in this solution a base—partially, at least—precipitable by ammonia, as an iodine compound. It is possible this secondary product may somewhat affect the characters of the iodide of ethylostrychnine, which, I think, is certainly formed, as might have been expected.

Action of Chloride of Amyl upon Strychnine.

Chloride of Amylostrychnine.—Strychnine, placed in contact with chloride of amyl, is found to undergo, very slowly, a change quite analogous to that with iodide of ethyl under the same circumstances. About eighty grains of the finely powdered alkaloid being sealed up in a strong tube with two fluid drachms of chloride of amyl, and some ten of absolute alcohol, the vessel was placed in boiling water, and retained at the same temperature for a long time. Complete solution took place in about fifty hours, and the heat was continued nearly forty-eight hours longer. The rather oily liquid deposited nothing on cooling, and left, on distillation to a syrup, a thick mass which finally dried up to a crystalline residue. This proved to be the chlorine salt of a base generally analogous to the ethyl product; it was very soluble in hot water, and crystallizable from a very strong boiling solution on cooling, or a more dilute one by evaporation, in fine colourless prisms.

I must mention that, in the first place, the whole of the product was dissolved in hot water, and the fluid evaporated to crystallization; the resulting crystals have their composition represented in analysis I. below, and must have contained any strychnine salt, or a great part of it, had any been originally present, as this is less soluble than the new one.

The entire mother liquor of these crystals was super-saturated by ammonia, no precipitate appeared for some time; on stirring, however, a certain quantity of a crystalline deposit fell, which presented the characters of strychnine. The ammoniacal fluid was suffered to stand two hours or so, and then filtered and evaporated to dryness at 212°. The residue was found to be completely soluble in hot water, or left but a little insoluble strychnine, and was made to crystallize like the first product. These are the crystals employed in analysis II., and could contain no strychnine.

The identity of the analytical results furnished by these two salts, and the appearance of the strychnine, caused me to make direct experiments with ammonia, to be described presently; in the meantime, the analyses of the salts, obtained as

above, are given; the chlorine was determined in a specimen different from either of the others:—

I. { 4·505 grains, dried at 212°, gave
11·425 ... carbonic acid, and
3·165 ... water.

II. { 4·213 ... dried at 212°, gave
10·750 ... carbonic acid, and
2·990 ... water.

III. { 4·140 ... dried at 212°, gave
1·345 ... chloride of silver.

	Found.				Calculation.		
	I.	II.	III.	Mean.			
Carbon, . . .	69·16	69·58		69·37	69·41	C_{52}	312
Hydrogen, . .	7·80	7·88		7·84	7·56	H_{34}	34
Nitrogen, . . .					6·22	N_2	28
Oxygen, . . .					8·92	O_5	40
Chlorine, . . .			8·03	8·03	7·89	Cl	35·5
					100·00		449·5

These numbers indicate a change quite analogous to that before seen to obtain with iodide of ethyl, and it may be expressed thus:—

$$\underbrace{C_{42}H_{22}N_2O_4}_{\text{Strychnine.}} + C_{10}H_{11}Cl = \underbrace{C_{42}H_{22}N_2O_4, C_{10}H_{11}, Cl}_{\text{New Salt.}} = C_{52}H_{33}N_2O_4Cl$$

but the salt, as deposited from water, retains an equivalent of the same at 212°, and is, according to the above analysis—

$$C_{52}H_{33}N_2O_4Cl + HO;$$

while, in the air-dry state, the crystals contain in addition seven atoms, which they readily part with, as these numbers shew:—

{ 8·170 grains, air-dry, lost at 212°
1·025 ... water;

which give a percentage of 12·54; and 12·29 is that required by a loss of seven atoms of water in a salt of the formula,—

$$C_{52}H_{33}N_2O_4Cl, HO + 7\ aq.$$

The hydrated chloride of amylostrychnine crystallizes from water in fine, thick, colourless, oblique rhombic prisms, which have a peculiar greasy appearance when dry; it is very soluble in alcohol. Its aqueous solution is not affected by dilute potass, or by ammonia, except when left long in contact; but is immediately precipitated by strong potass, the salt being thrown down unchanged. Oxide of silver eliminates the chlorine, leaving the base dissolved. When heated, the dry salt

fuses and gives off, first water, then acid vapours, and finally, with intumescence and blackening, alkaline fumes of a very nauseous odour, which also affect the throat most unpleasantly.

Its aqueous solution gives, with that of mercuric chloride, a heavy white precipitate, which is difficultly soluble in boiling water, and is deposited in the cold in very small and separate crystals; these seem, when magnified, to be prisms and octahedra; it furnishes, with chloride of gold, an insoluble amorphous yellow precipitate. With bichloride of platinum a pale yellow uncrystalline precipitate is obtained, but this could not, in three trials, be obtained of constant composition; the platinum came always much too high, even above that of the strychnine salt itself, in two cases, by 0·3 to 0·7 per cent.

Bichromate of Amylostrychnine.—This salt was obtained from the mother liquor of salt II. above, by addition of bichromate of potass; it fell as a crystalline yellow salt, soluble in boiling water; when ignited, it gave the following result:—

{ 6·418 grains, dried at 212°, gave
{ 0·965 ... sesquioxide of chromium.

The percentage of anhydrous chromic acid corresponding to this number is 19·63; and 19·37 is that required by a salt, analogous to that of the ethyl base, of the formula—

$$C_{52}\,H_{33}\,N_2\,O_4\,CrO_4,\,H\,CrO_4.$$

There exists also a crystalline chromate.

Nitrate of Amylostrychnine.—This was prepared from the crystals of the chloride I., by double decomposition with nitrate of silver in warm liquids. It is more soluble than the corresponding salt of ethylostrychnine; but it is by no means of great solubility in cold water, though readily taken up in the heat. It crystallizes from hot water in very beautiful radiated groups of colourless needles; the analysis of these gave the numbers which follow:—

{ 3·695 grains, dried at 212°, gave
{ 8·850 ... carbonic acid, and
{ 2·415 ... water.

	Found.	Calculation.		
Carbon,	65·32	65·54	C_{52}	312
Hydrogen,	7·26	7·14	H_{34}	34
Nitrogen,		8·82	N_3	42
Oxygen,		18·50	O_{11}	88
		100·00		476

and it appears that this salt is also not anhydrous at 212°, its formula being—

$$C_{52}\,H_{33}\,N_2\,O_4\,NO_6 + HO.$$

The air-dried crystals contain, in addition, ten atoms of water:—

{ 4·390 grains air-dry salt, lost at 212°
{ 0·685 ... water.

This number leads to a percentage of 15·60; and 15·90 is that corresponding to a loss of ten aq. by a salt of the composition—

$$C_{52}\,H_{33}\,N_2\,O_4\,NO_6,\,HO + 10\text{ aq.}$$

The aqueous solution of the nitrate gives, with that of mercurous nitrate, a crystalline deposit in the form of colourless needles; these are no doubt a double salt.

Hydrate of Amylostrychnine.—The chloride furnishes, with oxide of silver in water, a rich red purple fluid of a highly alkaline nature, which agrees most strikingly in its characters with the solution of ethylostrychnine; indeed, the two might be readily mistaken for each other; their deportment with metallic salts, and on evaporation, *in vacuo* or at 212°, is precisely the same. The flocky basic product by evaporation in the case of each, bears possibly the same relation to its congener which the parent bases mutually possess.

The residue left *in vacuo* dissolves to a very rich purple fluid in hot absolute alcohol, and ether causes in this solution an abundant and quickly appearing deposition of radiated white needles. I have not as yet analysed these, but I apprehend they must bear a close analogy to the hydrate of ethylostrychnine.

The chloride of amylostrychnine seeming to have a tendency to decompose in contact with ammonia, some solutions of the salt were supersaturated with this alkali; in no instance did any immediate precipitate occur; but in all, after the lapse of a shorter or longer period, crystalline deposits were formed, while the fluids assumed a reddish-brown colour; in some cases the product was but microscopic at the end of a fortnight, while other specimens furnished within this time quite a decided crystallization on the sides of the vessels. These crystals had the qualitative characters of strychnine; and it is easy to imagine a decomposition taking place in which this alkaloid should be regenerated with the formation of amylamine; thus—

$$C_{52}\,H_{33}\,N_2\,O_4\,Cl + NH_3 = C_{42}\,H_{22}\,N_2\,O_4 + C_{10}\,H_{13}\,N,\,HCl.$$

I have not as yet been able to verify this equation; for there has remained much of the amylostrychnine undecomposed in those cases where I tried to prove the existence of amylamine.

In the hope of hastening the decomposition, some of the mother liquor of the chloride was sealed in a tube with strong ammonia; the vessel was kept in boiling water for many days; a rather dark-coloured deposit gradually formed, while the liquid itself became brown and rather mucilaginous. At the end of a

week the tube was opened, but I could gain nothing decisive as to the evident decomposition which had taken place, and which seems of a more complex nature than had been anticipated.

The brown deposit was for the most part soluble in hydrochloric acid, but did not present the characters of pure strychnine when reprecipitated by ammonia. Its mother liquor, from the tube, was evaporated to dryness at 212° to expel the excess of ammonia, and it left a residue which had a most remarkable resemblance to common glue in appearance and odour: this contained the chlorine salt of some base, and when dissolved in water, and decomposed by nitrate of silver in excess, the fluid, filtered from the chloride of silver, gave in a few minutes a brilliant metallic mirror, but no crystalline salt.

These decompositions, at both temperatures, seem worthy of study. I may mention that chloride of ethylostrychnine, in contact with aqueous ammonia in the cold, also yields, after the lapse of some days, a small deposit of most minute crystals.

I have not at present experimented further with these substances, but hope to be able to render their history more complete, and, if possible, to clear up some points touched upon in this paper, by studying their especial products of decomposition in these cases. I have the intention also of submitting some of the other alkaloids, yet unexamined, to a similar investigation.

With regard to what has been actually brought forward in the present communication, I would draw the following inferences:—

That the new basic products, ethylo and amylo-strychnine are analogous to the compound ammonium bases of Hofmann, and quite distinct from the natural fixed alkaloids. In their resemblance to metallic oxides, and the combinations they form with water and acids, they shew, in common with these, general family features, while special distinctions exist as individual characteristics, and are to be paralleled in many saline compounds.

That the already complex molecule in the vegetable alkaloids is rendered more susceptible of change by association with additional hydrocarbons.

That as regards the constitution of strychnine, this alkaloid appears to be made up of a complicated molecule, in which the one atom of nitrogen, as in ammonia, etc., is associated with a nitrogenous aggregate of elements, whose function is that of three atoms of hydrogen, and whose nitrogen is in a distinct form of combination, as yet undetermined, from that of the first, generic atom. The expression of such a composition may be attempted thus:—

$$\underbrace{C_{42}\ H_{22}\ N_2\ O_4}_{\text{Strychnine.}} = \left\{ C_{42}\ H_{22}\ N\ O_4 = \left\{ \begin{matrix} H \\ H \\ H \end{matrix} \right\} + N \right\}$$

And the association of this molecule with any alcohol hydrocarbon occasions the production of an ammonium congener, which, as it exists in combination with some electronegative element, may be written thus :—

$$\left\{ C_{42}\ H_{22}\ N\ O_4 = \left\{ \begin{matrix} H \\ H \\ H \\ C_n\ H_{(n+1)} \end{matrix} \right\} + N + X. \right\}$$

In conclusion, I give a tabular view of the new compounds mentioned in the preceding pages ; and, in doing so, I have adopted the termination " ium " for the bases, in analogy with the name ammonium ; strychnia, the old name of the alkaloid in English, being, it seems to me, the more appropriate designation for the analogue of ammonia ; and it would be but consistent with the ammonium theory, to look upon the alkaloid in entering into combination, with hydrochloric acid for instance, as undergoing the same process as the volatile type, and to speak of the new salt as chloride of strychnium. The products of this investigation, then, are the following :—

Hydriodate of Papaverine, . .		$C_{40}\ H_{21}\ NO_8\ HI.$
Iodide of Ethylostrychnium, . .		$C_{46}\ H_{27}\ N_2\ O_4\ I.$
Hydrated oxide of do., . . .	crystallized	$C_{46}\ H_{27}\ N_2\ O_4\ O, HO, +3$ aq.
Nitrate of do., . . .	,,	$C_{46}\ H_{27}\ N_2\ O_4\ NO_6$
Bichromate, ...	dried at 212°	$C_{46}\ H_{27}\ N_2\ O_4\ CrO_4, HCrO_4.$
,, ...	crystallized	$C_{46}\ H_{27}\ N_2\ O_4\ CrO_4, HCrO_4 + 2$ aq.
Bicarbonate, ...	dry	$C_{46}\ H_{27}\ N_2\ O_4\ CO_3, HCO_3.$
Platinum compound, . . .	crystallized	$C_{46}\ H_{27}\ N_2\ O_4\ Cl, PtCl_2.$
Chloride of Amylostrychnium, .	dried at 212°	$C_{52}\ H_{33}\ N_2\ O_4\ Cl, HO.$
,, ,, ,, . .	crystallized	$C_{52}\ H_{33}\ N_2\ O_4\ Cl, HO + 7$ aq.
Nitrate of Oxide, . . .	dried at 212°	$C_{52}\ H_{33}\ N_2\ O_4\ NO_6, HO.$
,, ,, ...	crystallized	$C_{52}\ H_{33}\ N_2\ O_4\ NO_6, HO + 10$ aq.
Bichromate, ...	dried at 212°	$C_{52}\ H_{33}\ N_2\ O_4\ CrO_4, HCrO_4.$

V. — *On a General Method of Substituting Iodine for Hydrogen in Organic Compounds, and on the Properties of Iodopyromeconic Acid.* By Mr James F. Brown,* Assistant to Professor Anderson, Glasgow.

(Read 3d April 1854.)

In a paper on pyromeconic acid read before this Society, and since published in the Philosophical Magazine for September 1852, I have detailed the preparation and properties of a compound obtained by the substitution of an equivalent of bromine for an equivalent of hydrogen in that acid. Having observed that this substitution was very easily effected, I was induced to attempt the formation of an iodopyromeconic acid, in the hope of adding one to the very few instances in which the direct substitution of iodine for hydrogen has been found possible. For this purpose, I digested pyromeconic acid with tincture of iodine, but no success attended the experiment, the acid remaining entirely unchanged. The failure of this attempt led me to speculate as to its cause, and to contrive a method of producing the required substitution which has proved entirely successful, and has the further advantage of being perfectly general, so that its application will enable chemists to obtain iodine substitution compounds in cases in which they have hitherto failed.

A few preliminary observations on the cause of substitution will render intelligible the nature of the method in question. Selecting the production of bromopyromeconic acid and of trichloracetic acid, as characteristic examples of substitution, we have the following formulæ representing the changes which occur :—

$$\underbrace{C_{10}\,H_4\,O_6}_{\text{Pyromeconic Acid.}} + Br_2 = \underbrace{C_{10}\,H_3\,Br\,O_6}_{\text{Bromopyromeconic Acid.}} + H\,Br$$

$$\underbrace{C_4\,H_4\,O_4}_{\text{Acetic Acid.}} + Cl_6 = \underbrace{C_4\,H\,Cl_3\,O_4}_{\text{Trichloracetic Acid.}} + 3\,H\,Cl$$

In these, as in every other case of substitution, the chlorine and bromine obviously perform a twofold function, one portion entering into the complex atom in the place of an equivalent quantity of hydrogen, which is eliminated in combination with another quantity either of chlorine or bromine ; and the new product contains the same number of atoms, and is commonly said to belong to the same type. In talking of such substitution, it is not unfrequently said, that the

* It is with deep regret I have to record here the early death of Mr James Brown, who, in this and a previous communication read before this Society, had given such high promise of future eminence. He died at Glasgow on the 2d July, after an illness of only twelve hours' duration.—T. A.

atom of chlorine simply displaces or pushes out the atom of hydrogen, and so comes to occupy its place, but a very slight consideration enables us to see that it is really dependent, not so much on the quantity of chlorine which remains in the compound as on that which escapes in combination with hydrogen. The original substance in all cases of substitution, forms a perfect molecular group in which the individual affinities of the different elementary atoms are properly balanced, and the whole remains in a quiescent state. But when another element, such as chlorine, comes in contact with this complex group of atoms, it immediately exerts its affinity for the hydrogen, with which it easily combines, and withdraws the whole or part of it from the compound. In this way a gap is produced in what was before a perfect group, the balance of the affinities of its elements is destroyed, and there must either be a complete readjustment of its molecular arrangements, or some other element must stop the gap and produce another perfect molecule not differing from the original substance in the arrangement of its parts, but only in the presence of one or more atoms of a different sort from those which it previously contained. Considered in this point of view, it becomes at once obvious that we fail to produce the substitution of iodine for hydrogen, not from any inability of the former to occupy the place of the latter, but simply because it has not a sufficiently powerful affinity for hydrogen to withdraw it from the compound, so as to leave an empty space into which another portion may enter. It occurred to me that as chlorine and bromine in causing a substitution exercise the two different functions already alluded to, one quantity withdrawing hydrogen, and the other simply slipping into the vacant space, it might be possible to produce an iodine substitution by associating that element with some substance having a sufficiently powerful affinity for hydrogen, to open the door as it were, and leave nothing for it to do but to step into the place prepared for it.

For this purpose I selected the bromide of iodine, as being most likely to fulfil the required conditions. It was prepared by agitating bromine water with a considerable excess of iodine, and decanting the reddish-brown solution from the undissolved residue. When this bromide was added to pyromeconic acid, a change rapidly occurred, the solution became colourless, and iodopyromeconic acid was produced. Having succeeded in this way, I then tried the chloride of iodine, and having found it to act equally well, I made use of it in all my subsequent experiments. The chloride of iodine was prepared by passing a rapid current of chlorine through finely-pounded iodine suspended in a small quantity of water, and kept in continual agitation, care being taken to stop the process before the iodine was entirely dissolved. A moderate heat is produced during the combination, and the fluid should be kept as cool as possible.

In order, therefore, to obtain iodopyromeconic acid, a freshly-prepared solution of the chloride of iodine is mixed with a cold saturated solution of pyrome-

conic acid. The solution is instantly decolorized, and the new acid is deposited in the form of fine delicate plates, which are so abundant as to render the fluid almost semisolid. The only precautions necessary are to avoid the use of a hot solution of the acid, and an excess of the chloride, as the product, under such circumstances, becomes coloured, owing to the occurrence of a further decomposition, to be afterwards referred to. The change which first takes place is represented by the equation:—

$$C_{10}H_4O_6 + ICl = C_{10}H_3IO_6 + HCl.$$

The crystals of iodopyromeconic thus precipitated are in a short time filtered off and washed with cold water; they are then finally dissolved in boiling alcohol, from which they again deposit themselves, upon cooling, in perfectly colourless plates, having a high degree of lustre. The acid is sparingly soluble in cold water; but at a boiling heat it dissolves more readily, and crystallizes again from the solution in long, slender needles, possessing a slightly acid reaction. Acids and alkalies increase its solubility in water, but it is easily decomposed, if boiled with strong caustic potash. It is also decomposed by concentrated nitric acid, with the separation of free iodine. It gives a yellowish-white precipitate with nitrate of silver, soluble in ammonia, and with perchloride of iron it produces a deep purple colour, but no precipitate. It suffers no loss of weight at 212°, but heated to a higher temperature, it first fuses to a black fluid, and is then suddenly decomposed, with the evolution of a large quantity of iodine.

The combustion of iodopyromeconic acid was attended with some difficulty, for it was found that not only the acid itself, but even its lead salt permitted the iodine to escape in the free state, when burned either with chromate of lead, or with a mixture of oxide of copper and litharge. This would have been of little moment in determining the constitution of a substance such as iodopyromeconic acid, where the mode of its formation sufficiently indicates its composition, and the determination of the carbon and iodine would have been quite sufficient to fix its formula; but having observed the same peculiarity in another substance afterwards to be described, in which the exact determination of the hydrogen was essential to the establishment of its formula, I was compelled to devise some method by which the iodine might be retained, and the following is that which I found most successful.

The substance to be analysed was mixed with chromate of lead, and a small quantity of fused litharge reduced to a fine powder; the mixture was then introduced into a long combustion tube, held with its point downwards, and at the same time there were dropped into it small pieces of metallic lead, which remained at the under-side of the tube, and so arranged as to be about three inches apart. After the whole of the mixture had been introduced, and the remaining space in the interior of the tube filled up with chromate of lead, the point was turned

upwards, and by slight tapping a passage opened throughout the w and so of the tube, while the pieces of lead projecting above the surface melted that application of heat, and by exposing a metallic surface during the time combustion was going on, served to retain all the iodine.

The results of analysis are as follows: The hydrogen of No. 1 *was not weighed*, as, from its combustion being effected in the ordinary way without lead, the small end of the chloride of calcium tube was completely covered with small plates of iodine.

No. 1. 4·893 grains substance burned with chromate of lead, gave
4·620 ... carbonic acid.

No. 2. 7·298 grains substance burned with chromate of lead, litharge, and metallic lead, gave
6·759 ... carbonic acid, and
·975 ... water.

No. 3. 6·85 grains substance heated with carbonate of potash, gave
6·707 ... iodide of silver.

	No. 1.	No. 2.	Calculation.		
Carbon, . . .	25·750	25·25	25·19	C_{10}	60
Hydrogen, . .	...	1·48	1·26	H_3	3
Oxygen, . . .	...	...	20·17	O_6	48
Iodine, ...	52·909	...	53·38	I	127·1
	100·000	100·00	100·00		238·1

These results correspond exactly with the formula, $C_{10}\,H_2\,I\,O_5 + HO$.

Iodopyromeconic acid is monobasic, and forms salts, of which I have minutely examined only those of baryta and lead. It does not appear to form an ammonia salt.

Iodopyromeconate of baryta is readily prepared by mixing together alcoholic solutions of acetate of baryta, and of the acid made slightly alkaline with ammonia. After a short time it deposits a fine network of delicate crystals, of little solubility either in cold or hot water or alcohol. It is alkaline to litmus paper, and at 212° it suffers no loss of weight.

The following result was obtained on igniting the salt with sulphuric acid:—

4·49 grains substance gave
1·63 ... sulphate of baryta.

which corresponds to the formula, $Ba\,O\,C_{10}\,H_2\,IO_5 + HO$, as shown by the following calculation:—

	Experiment.	Calculation.		
Carbon, . . .	...	19·068	C_{10}	60
Hydrogen, . .	...	·954	H_3	3
Oxygen, . . .	...	15·255	O_6	48
Iodine, . . .	...	40·394	I	127·10
Baryta, . . .	23·84	24·329	Ba O	76·55
	100·00	100·000		314·65

romeconate of lead is readily obtained as a fine colourless amorphous conic acid. . in the fo..., on mixing alcoholic solutions of the acid and acetate of lead, with the addition of a small quantity of ammonia. As thus prepared, it is apt to carry down an excess of oxide of lead, which is easily removed by warm acetic acid. It is of sparing solubility in alcohol or water, and becomes highly electrical when rubbed. This salt, contrary to expectation, evolves free iodine, like the acid itself, when burned with chromate of lead; but the phenomenon is probably owing to the decomposition of the salt taking place at a temperature much lower than that sufficient to enable the iodine to combine with lead. For this reason I contented myself with a determination of its oxide of lead, which was effected by igniting the salt, after the addition of a few drops of concentrated sulphuric acid:—

6·15 grains substance, dried in the air, gave
2·76 ... sulphate of lead.

corresponding to the formula, $PbO, C_{10} H_2 IO_5$, as shown by the subjoined calculation:—

	Experiment.	Calculation.		
Carbon, . . .	...	17·612	C_{10}	60
Hydrogen, . . .	...	·587	H_2	2
Oxygen, . . .	...	11·743	O_5	40
Iodine, ...	...	37·309	I	127·1
Oxide of lead, . .	33·03	32·749	PbO	111·56
	100·00	100·000		340·66

I have already mentioned, that when the quantity of chloride of iodine employed is larger than is requisite for the production of iodopyromeconic acid, the fluid acquires a yellow colour, due to the presence of another compound, of very remarkable characters, produced by a further decomposition of pyromeconic acid, and to which I gave the name of iodomecone. When potash is gradually added to the fluid, after separation of the iodopyromeconic acid, a blackish precipitate immediately falls, which rapidly dissolves on agitation of the fluid, while a peculiar odour is evolved. After the addition of the potash has been continued for some time, a point is reached at which the precipitate assumes a lighter colour, is no longer dissolved, and is not increased by further addition of the alkali. The precipitate is then filtered from the alkaline fluid, washed with cold water, and purified by repeated crystallization from boiling alcohol. By subsequent experiments, I ascertained that it was easy to convert pyromeconic acid entirely into this compound, by adding a large quantity of chloride of iodine, when the crystals of iodopyromeconic acid at first formed rapidly disappeared, and carbonic acid was evolved. Exactly similar effects are produced by bromide of iodine.

Iodomecone is obtained in large hexagonal plates of a bright yellow colour

and brilliant lustre, and having an odour resembling that of saffron. It is insoluble in water, soluble in alcohol, especially on boiling, and in ether. It is insoluble in hydrochloric acid, and may be boiled with that reagent, without suffering decomposition. Strong nitric acid attacks it with great violence, but does not effect a complete oxidation of all the iodine. It is unacted on in the cold by strong sulphuric acid, but if heated, it is decomposed with the liberation of iodine. Caustic potash when long boiled with it, removes a very small quantity of iodine. It does not affect litmus paper, and seems to possess neither acid nor basic properties. It sublimes unaltered at a temperature greatly below that of boiling water.

These characters closely approximate to those of iodoform, and I at first considered it to be that substance. The analysis, however, made with every care, and on specimens prepared at different times, gave results which cannot be made to agree with the formula of that substance.

The analysis are as follows:—

No. 1. 7·141 grains substance, air-dry, gave
0·913 ... carbonic acid, and
... ... water.

No. 2. 4·365 grains substance, air-dry, gave
... ... carbonic acid, and
0·164 ... water.

No. 3. 8·153 grains substance, air-dry, gave
0·958 ... carbonic acid, and
0·346 ... water.

No. 1. 5·697 grains substance, air-dry, gave
9·706 ... iodide of silver.

No. 2. 3·611 grains substance, air-dry, gave
6·140 ... iodide of silver.

	No. 1.	No. 2.	No. 3.
Carbon,	3·48	lost.	3·204
Hydrogen,	...	0·417	0·471
Oxygen,	...	...	4·440
Iodine,	...	92·060	91·885
	100·000	100·000	100·000

	Mean.	Calculation.		
Carbon,	3·337	3·258	C_6	36
Hydrogen,	0·444	0·364	H_4	4
Oxygen,	4·247	4·344	O_6	48
Iodine,	91·972	92·034	I_8	1016·8
	100·000	100·000		1104·8

The formula is therefore $C_6 H_4 I_8 O_6$.

The decomposition by which iodomecone is produced from pyromeconic acid, now becomes obvious. It is represented by the following equation:—

$$C_{10} H_4 O_6 + 8 I Cl + 8 HO = C_6 H_4 I_8 O_6 + 4 CO_2 + 8 HCl$$

This being the mode of its formation, it seemed probable that meconic and comenic acids, which differ from pyromeconic acid only by the elements of carbonic acid, would yield the same substance, when acted on by chloride of iodine. Accordingly, I have found that it is immediately produced with all its characteristic properties from these acids, by the same process. The relations of this substance to meconic and pyromeconic acids, are of a very remarkable character, and cannot at present be distinctly brought out. It obviously belongs to the same class as the very curious product obtained by CAHOURS, by the action of bromine on citric acid, and which he has called bromoxaform. According to this chemist, when bromine is added to citrate of potash, effervescence takes place from the evolution of carbonic acid, and on the addition of potash, an oily matter is thrown down, which consists of three substances, one bromoform, the other a crystalline solid bromoxaform, and the third apparently an accidental product, for it is obtained in too small quantities to admit of examination. It can scarcely be doubted that bromoxaform would be the only product, if the action could be properly moderated, and that the bromoform is a secondary product of the former substance, from which indeed it is readily obtained, by treatment with caustic potash. If this be the case, the decomposition of citrate of potash would be quite analogous to that of pyromeconic acid, as represented in the equation:—

$$\underbrace{C_{12} H_5 K_3 O_{14}}_{\text{Citrate of Potash.}} + 2 HO + Br_{14} = \underbrace{C_6 H Br_5 O_4}_{\text{Bromoxaform.}} + 6 CO_2 + 3 K Br + 6 H Br.$$

The relation which these curious substances bear to their parent acids is very obscure, and cannot be elucidated without further experiments. In regard to iodomecone, the small quantity in which I was able to obtain it, has prevented my following out its decompositions as I could have wished, but I propose extending this investigation to some of the stronger acids by which means some light may probably be thrown upon the constitution of these bodies.

It was my desire to have extended my examination of the iodine substitution products obtained by chloride of iodine, to some other substances. As yet, however, I have only tried codeine; but the instability of the compound produced has occasioned such difficulties, that I have hitherto been unable to arrive at satisfactory results. When chloride of iodine is added to a concentrated solution of hydrochlorate of codeine, a fine yellow crystalline precipitate makes its appearance. It is insoluble in water, but readily soluble in boiling alcohol. If this solution is carefully effected, and too much of the substance be not added, it crystallizes on cooling in stellar groups, of a fine red colour, but if a large quantity is dissolved,

it is deposited as a perfectly amorphous mass. Unfortunately, the iodine is retained with a very feeble affinity, and I have found that at every crystallization, a small quantity is separated and remains in the fluid, so that results of a satisfactory character could not be obtained on its analysis. It is soluble in hydrochloric acid, and if the solution be made hot, it deposits at first an oily substance, which aftewards concretes to a flocky mass. Both ammonia and potash precipitate it from its solution in hydrochloric acid, the former giving a slightly coloured substance. With chloride of platinum it yields a bright yellow precipitate, one determination of the platinum in which gave 12·20 per cent.; 11·95 corresponds to the formula, $C_{36}\ H_{19}\ I_2\ N\ O_6\ H\,Cl\ Pt\,Cl_2 + H\,O.$, which represents the hydrated salt of a base, which may be called di-iodocodeine, as being derived from codeine by the substitution of two atoms of iodine for two of hydrogen.

These experiments were conducted in the laboratory of Professor ANDERSON, to whom I am much indebted for assistance during their prosecution.

VI.—*Note on the Possible Density of the Luminiferous Medium and on the Mechanical Value of a Cubic Mile of Sunlight.* By Professor WILLIAM THOMSON.

(Read 1st May 1854.)

That there must be a medium forming a continuous material communication throughout space to the remotest visible body is a fundamental assumption in the undulatory Theory of Light. Whether or not this medium is (as appears to me most probable) a continuation of our own atmosphere, its existence is a fact that cannot be questioned, when the overwhelming evidence in favour of the undulatory theory is considered; and the investigation of its properties in every possible way becomes an object of the greatest interest. A first question would naturally occur, What is the absolute density of the luminiferous ether in any part of space? I am not aware of any attempt having hitherto been made to answer this question, and the present state of science does not in fact afford sufficient data. It has, however, occurred to me that we may assign an inferior limit to the density of the luminiferous medium in interplanetary space by considering the mechanical value of sunlight as deduced in preceding communications to the Royal Society from POUILLET's data on solar radiation, and JOULE's mechanical equivalent of the thermal unit. Thus the value of solar radiation per second per square foot at the earth's distance from the sun, estimated at ·06 of a thermal unit centigrade, or 83 foot-pounds, is the same as the mechanical value of sunlight in the luminiferous medium through a space of as many cubic feet as the number of linear feet of propagation of light per second. Hence the mechanical value of the whole energy, actual and potential, of the disturbance kept up in the space of a cubic foot at the earth's distance from the sun,* is $\frac{83}{192000 \times 5280}$, or $\frac{\cdot 819}{10^7}$ of a foot-pound. The mechanical value of a cubic mile of sunlight is consequently 12050 foot-pounds, equivalent to the work of one-horse power for a third of a minute. This result may give some idea of the actual amount of mechanical energy of the luminiferous motions and forces within our own atmosphere.

* The mechanical value of sunlight in any space near the sun's surface must be greater than in an equal space at the earth's distance, in the ratio of the square of the earth's distance to the square of the sun's radius, that is, in the ratio of 46,400 to 1 nearly. The mechanical value of a cubic foot of sunlight near the sun must, therefore, be about ·0038 of a foot-pound, and that of a cubic mile 560,000,000 foot-pounds.

Merely to commence the illumination of three cubic miles, requires an amount of work equal to that of a horse-power for a minute; the same amount of energy exists in that space as long as light continues to traverse it; and, if the source of light be suddenly stopped, must be emitted from it before the illumination ceases.* The matter which possesses this energy is the luminiferous medium. If, then, we knew the velocities of the vibratory motions, we might ascertain the density of the luminiferous medium; or, conversely, if we knew the density of the medium, we might determine the average velocity of the moving particles. Without any such definite knowledge, we may assign a superior limit to the velocities, and deduce an inferior limit to the quantity of matter, by considering the nature of the motions which constitute waves of light. For it appears certain that the amplitudes of the vibrations constituting radiant heat and light must be but small fractions of the wave lengths, and that the greatest velocities of the vibrating particles must be very small in comparison with the velocity of propagation of the waves. Let us consider, for instance, plane polarized light, and let the greatest velocity of vibration be denoted by v; the distance to which a particle vibrates on each side of its position of equilibrium, by A; and the wave length, by λ. Then if V denote the velocity of propagation of light or radiant heat, we have

$$\frac{v}{V} = 2\pi \frac{A}{\lambda};$$

and therefore if A be a small fraction of λ, v must also be a small fraction (2π times as great) of V. The same relation holds for circularly polarized light, since in the time during which a particle revolves once round in a circle of radius A, the wave has been propagated over a space equal to λ. Now the whole mechanical value of homogeneous plane polarized light in any infinitely small space containing only particles sensibly in the same phase of vibration, which consists entirely of potential energy at the instants when the particles are at rest at the extremities of their excursions, partly of potential and partly of actual energy when they are moving to or from their positions of equilibrium, and wholly of actual energy when they are passing through these positions, is of constant amount, and must therefore be at every instant equal to half the mass multiplied by the square of the velocity the particles have in the last-mentioned case. But the velocity of any particle passing through its position of equilibrium is the greatest velocity of vibration, which has been denoted by v; and, therefore, if ρ denote the quantity of vibrating matter contained in a certain space, a space of unit volume for instance, the whole mechanical value of all the energy, both

* Similarly we find 15000 horse-power for a minute as the amount of work required to generate the energy existing in a cubic mile of light near the sun.

actual and potential, of the disturbance within that space at any time is $\frac{1}{2}\rho v^2$. The mechanical energy of circularly polarized light at every instant is (as has been pointed out to me by Professor Stokes) half actual energy of the revolving particles and half potential energy of the distortion kept up in the luminiferous medium; and, therefore, v being now taken to denote the constant velocity of motion of each particle, double the preceding expression gives the mechanical value of the whole disturbance in a unit of volume in the present case. Hence it is clear, that for any elliptically polarized light the mechanical value of the disturbance in a unit of volume will be between $\frac{1}{2}\rho v^2$ and ρv^2, if v still denote the greatest velocity of the vibrating particles. The mechanical value of the disturbance kept up by a number of coexisting series of waves of different periods, polarized in the same plane, is the sum of the mechanical values due to each homogeneous series separately, and the greatest velocity that can possibly be acquired by any vibrating particle is the sum of the separate velocities due to the different series. Exactly the same remark applies to coexistent series of circularly polarized waves of different periods. Hence the mechanical value is certainly less than *half* the mass multiplied into the square of the greatest velocity acquired by a particle, when the disturbance consists in the superposition of different series of plane polarized waves; and we may conclude, for every kind of radiation of light or heat except a series of homogeneous circularly polarized waves, that *the mechanical value of the disturbance kept up in any space is less than the product of the mass into the square of the greatest velocity acquired by a vibrating particle in the varying phases of its motion.* How much less in such a complex radiation as that of sunlight and heat we cannot tell, because we do not know how much the velocity of a particle may mount up, perhaps even to a considerable value in comparison with the velocity of propagation, at some instant by the superposition of different motions chancing to agree; but we may be sure that the product of the mass into the square of an ordinary maximum velocity, or of the mean of a great many successive maximum velocities of a vibrating particle, cannot exceed in any great ratio the true mechanical value of the disturbance. Recurring, however, to the definite expression for the mechanical value of the disturbance in the case of homogeneous circularly polarized light, the only case in which the velocities of all particles are constant and the same, we may define the mean velocity of vibration in any case as such a velocity that the product of its square into the mass of the vibrating particles is equal to the whole mechanical value, in actual and potential energy, of the disturbance in a certain space traversed by it; and from all we know of the mechanical theory of undulations, it seems certain that this velocity must be a very small fraction of the velocity of propagation in the most intense light or radiant heat which is propagated according to known laws. Denoting this velocity for the case of sunlight at the earth's distance from the sun by v, and calling W the mass in pounds

of any volume of the luminiferous ether, we have for the mechanical value of the disturbance in the same space,

$$\frac{W}{g} v^2,$$

where g is the number 32·2, measuring in absolute units of force, the force of gravity on a pound. Now we found above, from observation, $\frac{83}{V}$ for the mechanical value, in foot-pounds, of a cubic foot of sunlight; and therefore the mass, in pounds, of a cubic foot of the ether, must be given by the equation,

$$W = \frac{32{\cdot}2 \times 83}{v^2 V}.$$

If we assume $v = \frac{1}{n} V$, this becomes

$$W = \frac{32{\cdot}2 \times 83}{V^3} \times n^2 = \frac{32{\cdot}2 \times 83}{(192000 \times 5280)^3} \times n^2 = \frac{n^2}{3899 \times 10^{20}};$$

and for the mass, in pounds, of a cubic mile we have

$$\frac{32{\cdot}2 \times 83}{(192000)^3} \times n^2 = \frac{n^2}{2649 \times 10^9}.$$

It is quite impossible to fix a definite limit to the ratio which v may bear to V; but it appears improbable that it could be more, for instance, than $\frac{1}{50}$, for any kind of light following the observed laws. We may conclude that probably a cubic foot of the luminiferous medium in the space traversed by the earth contains not less than $\frac{1}{1560 \times 10^{17}}$ of a pound of matter, and a cubic mile not less than $\frac{1}{1060 \times 10^6}$.

If the mean velocity of the vibrations of light within a spherical surface concentric with the sun and passing through the earth were equal to the earth's velocity—a very tolerable supposition—since this is $\frac{1}{10170}$ of the velocity of light, the whole mass of the luminiferous medium within that space would be $\frac{1}{30000}$ of the earth's mass, since the mechanical value of the light within it, being as much as the sun radiates in about 8 minutes, is about $\frac{1}{15000}$ of the mechanical value of the earth's motion. As the mean velocity of the vibrations might be many times greater than has been supposed in this case, the mass of the medium might be considerably less than this; but we may be sure it is not incomparably less, not 100,000 times as small for instance. On the other hand, it is worth remarking that the preceding estimate shows that what we know of the mechanical value of light renders it in no way probable that the masses of luminiferous medium in interplanetary spaces, or all round the sun in volumes of which the linear dimensions are comparable with the dimensions of the planets' orbits, are otherwise than excessively small in comparison with the masses of the planets.

But it is also worth observing that the luminiferous medium is enormously denser than the continuation of the terrestrial atmosphere would be in interplane-

tary space, if rarefied according to BOYLE's law always, and if the earth were at rest in a space of constant temperature with an atmosphere of the actual density at its surface.* Thus the mass of air in a cubic foot of distant space several times the earth's radius off, on this hypothesis, would be $\frac{1 \text{ lb.}}{442 \times 10^{345}}$; while there cannot, according to the preceding estimate, be in reality less than $\frac{1 \text{ lb.}}{1560 \times 10^{17}}$, which is 9×10^{327} times as much, of matter in every cubic foot of space traversed by the earth.

* "NEWTON has calculated (Princ. iii., p. 512) that a globe of ordinary density at the earth's surface, of one inch in diameter, if reduced to the density due to the altitude above the surface of one radius of the earth, would occupy a sphere exceeding in radius the orbit of Saturn."—(*Herschel's Astronomy, Note on* § 559.) It would (on the hypothesis stated in the text) we may now say occupy a sphere exceeding in radius millions of millions of times the distances of any stars of which the parallaxes have been determined. A pound of the medium, in the space traversed by the earth, cannot occupy more than the bulk of a cube 1000 miles in side. The earth itself, in moving through it, cannot displace less than 250 pounds of matter.

VII.—*On the Mechanical Energies of the Solar System.*

By Professor William Thomson.

(Read 17th April 1854.)

The mutual actions and motions of the heavenly bodies have long been regarded as the grandest phenomena of mechanical energy in nature. Their light has been seen, and their heat has been felt, without the slightest suspicion that we had thus a direct perception of mechanical energy at all. Even after it has been shewn* that the almost inconceivably minute fraction of the Sun's heat and light reaching the earth is the source of energy from which all the mechanical actions of organic life, and nearly every motion of inorganic nature at its surface, are derived, the energy of this source has been scarcely thought of as a development of mechanical power.

Little more than ten years ago the true relation of heat to force, in every electric, magnetic, and chemical action, as well as in the ordinary operations of mechanics, was pointed out;† and it is a simple corollary from this that the Sun, within the historical period of human observation, has emitted hundreds of times as much mechanical energy‡ as that of the motions of all the known planets taken together. The energy, that of light and radiant heat, thus emitted, is dissipated always more and more widely through endless space, and never has been, probably never can be, restored to the Sun, without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be. Hence the question arises, What is the source of mechanical energy, drawn upon by the Sun, in emitting heat, to be dissipated through space? In speculating on the answer, we may consider whether the source in question consists of dynamical energy, that is, energy of motion,§ or of "potential energy," (as Mr Rankine has called the energy of force acting between bodies, which will give way to it unless held); or whether it consists partly of dynamical and partly of potential energy.

And again, we may consider whether the source in question, or any part of it, is in the Sun, or exists in surrounding matter, until taken and sent out again

* Herschel's Astronomy, Edition 1833.—See last Ed., § (399).

† Joule "On the Generation of Heat in the Galvanic Circuit," communicated to the Royal Society of London, Dec. 17, 1840, and published, Phil. Mag., Oct. 1841. "On the Heat evolved during the Electrolysis of Water," Literary and Phil. Soc. of Manchester, 1843, Vol. vii., Part 3., Second Series. "On the Calorific Effects of Magneto-Electricity, and the Mechanical Value of Heat," communicated to the British Association, August 1843, and published, Phil. Mag., Sept. 1843. "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," communicated to the Royal Society, June 1844, and published, Phil. Mag., May 1845. Joule and Scoresby "On the Powers of Electromagnetism, Steam, and Horses," Phil. Mag., June 1846.

‡ Once every 20 years or so.—See Table of Mechanical Energies of the Solar System, appended.

§ "Actual energy," as Mr Rankine has called it.

by the Sun, or exists as energy only convertible into heat by mutual actions between the Sun and surrounding matter.

If it be dynamical and entirely in the Sun, it can only be primitive heat; if potential and in the Sun, it can only be energy of chemical forces ready to act. If not in the Sun, it must be due to matter coming to the Sun; (for it certainly is not a mere communication of motion to solar particles from external energy, as such could only be effected by undulations like sound or radiant heat, and we know that no such anti-radiation can be experienced by a body in the Sun's circumstances); but whether intrinsically in such external matter, or developed by mutual action between this matter and the Sun, and whether dynamical or potential in either case, requires careful consideration, as will be shewn in the course of this communication. We see, then, that all the theories which have been yet proposed, as well as every conceivable theory, must be one or other, or a combination of the following three:—

I. That the Sun is a heated body, losing heat.

II. That the heat emitted from the Sun is due to chemical action among materials originally belonging to his mass, or that the Sun is a great fire.

III. That meteors falling into the Sun give rise to the heat which he emits.

In alluding to theories of solar heat in former communications to the Royal Society, I pointed out that the first hypothesis is quite untenable. In fact, it is demonstrable that, unless the Sun be of matter inconceivably more conductive for heat, and less volatile, than any terrestrial meteoric matter we know, he would become dark in two or three minutes, or days, or months, or years, at his present rate of emission, if he had no source of energy to draw from but primitive heat.* The second has been not only held by the Fire-worshippers, but has probably been conceived of by all men in all times, and considered as more or less probable by every philosopher who has ever speculated on the subject. The third may have occurred at any time to ingenious minds, and may have occurred and been set aside as not worth considering; but was never brought forward in any definite form, so far as I am aware, until Mr Waterston communicated to the British Association, during its last meeting at Hull, a remarkable speculation on cosmical dynamics, in which he proposed the Theory that solar heat is produced by the impact of meteors falling from extra-planetary space, and striking his surface with

* This assertion is founded on the supposition that conduction is the only means by which heat could reach the Sun's surface from the interior, and perhaps requires limitation. For it might be supposed that, as the Sun is no doubt a melted mass, the brightness of his surface is constantly refreshed by incandescent fluid rushing from below to take the place of matter falling upon the surface after becoming somewhat cooled and consequently denser—a process which might go on for many years without any sensible loss of brightness. If we consider, however, the whole annual emission at the present actual rate, we find, even if the Sun's thermal capacity were as great as that of an equal mass of water, that his mean temperature would be lowered by about 3° cent. in two years. We may, I think, safely conclude that primitive heat within the Sun is not a sufficient source for the emission which has continued without sensible (if any) abatement for 6000 years.—(May 4, 1854.)

velocities which they have acquired by his attraction. This is a form of what may be called the Gravitation Theory of Solar Heat, which is itself included in the general meteoric theory.

The objects of the present communication are to consider the relative capabilities of the second and third hypothesis to account for the phenomena; to examine the relation of the gravitation theory to the meteoric theory in general; and to determine what form of the gravitation theory is required to explain solar heat consistently with other astronomical phenomena.

In the first place, it may be remarked, that in all probability there must always be meteors falling into the Sun, since the fact of meteors coming to the earth* proves the existence of such bodies moving about in space; and even if the motions of these bodies are at any instant such as to correspond to elliptical or circular orbits round the Sun, the effects of the resisting medium would gradually bring them in to strike his surface. Also, it is easy to prove dynamically that meteors falling in to the Sun, whatever may have been their previous state of motion, must enter his atmosphere, or strike his surface, with, on the whole, immensely greater relative velocities than those with which meteors falling to the earth enter the earth's atmosphere, or strike the earth's surface. Now, JOULE has shewn what enormous quantities of heat must be generated from this relative motion in the case of meteors coming to the earth; and by his explanation† of "falling stars," has made it all but certain that, in a vast majority of cases, this generation of heat is so intense as to raise the body in temperature gradually up to an intense white heat, and cause it ultimately to burst into sparks in the air (and burn if it be of metallic iron) before it reaches the surface. Such effects must be experienced to an enormously greater degree before reaching his surface, by meteors falling to the Sun, if, as is highly probable, he has a dense atmosphere; or they would take place yet more intensely on striking his solid or liquid surface, were they to reach it still possessing great velocities. Hence, it is certain that *some* heat and light radiating from the Sun is due to meteors. It is excessively probable that there is much more of this from any part of the Sun's surface than from an equal area of the earth's, because of the enormously greater action that an equal amount of meteoric matter would produce in entering the Sun, and because the Sun, by his greater attraction, must draw in meteoric matter much more copiously with reference to equal areas of surface. We would have no right then, as was done till Mr WATERSTON brought forward his theory, to neglect meteoric action in speculating on solar heat, unless we could prove, which we certainly

* To make the argument perfectly conclusive, it would have to be assumed that meteors not only are, but have been, always falling to the earth for some immense period of time. The conclusion, however, appears sufficiently probable with the facts we know.

† See Philosophical Magazine, May 1848, for reference to a lecture in Manchester, on the 28th April 1847, in which Mr JOULE said, that "the velocity of a meteoric stone is checked by the atmosphere and its *vis viva* converted into heat, which at last becomes so intense, as to melt the body and dissipate it in fragments too small probably to be noticed in their fall to the ground, in most cases."

cannot do, that its influence is insensible. It is in fact not only proved to exis as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence.

To test the possibility of this being the *principal or the sole cause* of the phenomenon, let us estimate at what rate meteoric matter would have to fall on the Sun, to generate as much heat as is emitted. According to POUILLET's data,* ·06 of a thermal unit centigrade is the amount of heat incident per second on a square foot directly exposed to solar radiation at the Earth's distance from the Sun, which being 95,000,000 miles, and the Sun's radius being 441,000 miles, we infer that the rate of emission of heat from the Sun is

$$\cdot 06 \times \left(\frac{95{,}000{,}000}{441{,}000}\right)^2 = 2781 \text{ thermal units per second}$$

per square foot of his surface.

The mechanical value of this (obtained by multiplying it by JOULE's equivalent, 1390) is

$$83{\cdot}4 \times \left(\frac{95{,}000{,}000}{441{,}000}\right)^2 = 386{,}900 \text{ ft. lbs.}$$

Now if, as Mr WATERSTON supposes, a meteor either strikes the Sun, or enters an atmosphere where the luminous and thermal excitation takes place, *without having previously experienced any sensible resistance*, it may be shewn dynamically (the velocity of rotation of the Sun's surface, which at his equator is only a mile and a quarter per second, being neglected) that the least relative velocity which it can have is the velocity it would acquire by solar gravitation in falling from an infinite distance, which is equal to the velocity it would acquire by the action of a constant force equal to its weight at the Sun's surface, operating through a space equal to his radius. The force of gravity at the Sun's surface being about 28 times that at the earth's surface, this velocity is

$$\sqrt{\frac{2 \times 28 \times 32{\cdot}2 \times 441{,}000}{5280}} = 390 \text{ miles per second;}$$

and its mechanical value per pound of meteoric matter is

$$28 \times 441{,}000 \times 5280 = 65{,}000{,}000{,}000 \text{ ft. lbs.}$$

Hence the quantity of meteoric matter that would be required, according to Mr WATERSTON's form of the Gravitation Theory, to strike the Sun per square foot is 0·000060 pounds per second (or about a pound every five hours.) At this rate the surface would be covered to a depth of thirty feet in a year, if the density of the deposit is the same as that of water, which is a little less than the mean density of the Sun.† A greater rate of deposit than this could not be required, if the hypothesis of no resistance, except in the locality of resistance with luminous reaction, were true; but a less rate would suffice if, as is probable enough,

* Mémoire sur la Chaleur Solaire, &c., Paris 1838; See Comptes Rendus, July 1838; or POUILLET, Traité de Physique, vol. ii.

† This is rather more than double the estimate Mr WATERSTON has given. The velocity of impact which he has taken is 545 miles per second, in the calculation of which, unless I am mistaken, there must be some error.

the meteors in remote space had velocities relative to the Sun not incomparably smaller than the velocity calculated above as due to solar gravitation.

But it appears to me that the hypothesis of no sensible resistance until the "Sun's atmosphere" is reached, or the Sun's surface struck, is not probable;* because if meteors were falling in to the Sun in straight lines, or in parabolic or hyperbolic paths, in anything like sufficient quantities for generating all the heat he emits, the earth in crossing their paths would be, if not intolerably pelted, at least struck much more copiously by meteors than we can believe it to be from what we observe; and because the meteors we see appear to come generally in directions corresponding to motions which have been elliptic or circular, and rarely if ever in such directions as could correspond to previous parabolic, hyperbolic, or rectilineal paths towards the Sun. If this opinion and the first mentioned reason for it be correct, the meteors containing the stores of energy for future Sun light must be principally within the earth's orbit: and we actually see them there as the "Zodiacal Light," an illuminated shower or rather tornado of stones (HERSCHEL, § 897). The inner parts of this tornado are always getting caught in the Sun's atmosphere, and drawn to his mass by gravitation. The bodies in all parts of it, in consequence of the same actions, must be approaching the Sun, although but very gradually; yet, in consequence of their comparative minuteness, much more rapidly than the planets. The outer edge of the zodiacal light appears to reach to near the earth at present (HERSCHEL, § 897); and in past times it may be that the earth has been in a dense enough part of it to be kept hot, just as the Sun is now, by drawing in meteors to its surface.

According to this form of the gravitation theory, a meteor would approach the Sun by a very gradual spiral, moving with a velocity very little more than that corresponding to a circular path at the same distance, until it begins to be much more resisted, and to be consequently rapidly deflected towards the Sun; then the phenomenon of ignition commences; after a few seconds of time all the dynamical energy the body had at the commencement of the sudden change is converted into heat and radiated off; and the mass itself settles incorporated in the Sun. It appears, therefore, that the velocity which a meteor loses in entering the Sun is that of a satellite at his surface, which (being $\frac{1}{\sqrt{2}}$ of that due to gravitation from an infinite distance) is 276 miles per second. The mechanical value (being half that of a body falling to the Sun from a state of comparatively slow motion in space) is about 32,500,000,000 ft. lb. per pound of meteoric matter; hence the fall of meteors must be just twice that which was determined above according to Mr WATERSTON's form of the theory, and must consequently amount to 3800 lbs. annually per square foot. If, as was before supposed, the density of the deposit is the same as that of water, the whole surface would be covered

* For a demonstration that it is not possible, see Addition No. 1.

annually to a depth of 60 feet, from which the Sun would grow in diameter by a mile in 88 years. It would take 4000 years at this rate to grow a tenth of a second in apparent diameter, which could scarcely be perceived by the most refined of modern observations, or 40,000 years to grow 1″, which would be utterly insensible by any kind of observation (that of eclipses included) unassisted by powerful telescopes. We may be confident, then, that the gradual augmentation of the Sun's bulk required by the meteoric theory to account for this heat, may have been going on in time past during the whole existence of the human race, and yet could not possibly have been discovered by observation, and that at the same rate it may go on for thousands of years yet without being discoverable by the most refined observations of modern astronomy. It would take, always at the same rate, about 2,000,000 years for the Sun to grow in reality as much as he appears to grow from June to December by the variation of the earth's distance, which is quite imperceptible to ordinary observation. This leaves for the speculations of geologists on ancient natural history a wide enough range of time with a Sun not sensibly less than our present luminary: Still more, the meteoric theory affords the simplest possible explanation of past changes of climate on the earth. For a time the earth may have been kept melted by the heat of meteors striking it. A period may have followed when the earth was not too hot for vegetation, but was still kept, by the heat of meteors falling through its atmosphere, at a much higher temperature than at present, and illuminated in all regions, polar as well as equatorial, before the existence of night and day. Lastly; although a very little smaller, the Sun may have been been at some remote period much hotter than at present by having a more copious meteoric supply.

A dark body of dimensions such as the Sun, in any part of space, might, by entering a cloud of meteors, become incandescent as intensely in a few seconds as it could in years of continuance of the same meteoric circumstances; and on again getting to a position in space comparatively free from meteors, it might almost as suddenly become dark again. It is far from improbable that this is the explanation of the appearance and disappearance of bright stars, and of the strange variations of brilliancy of others which have caused so much astonishment.*

The amount of matter, drawn by the Sun in any time from surrounding space, would be such as in $47\frac{1}{2}$ years to amount to a mass equal to that of the earth. Now there is no reason whatever to suppose that 100 times the earth's mass drawn in to the Sun, would be missed from the zodiacal light (or from meteors revolving inside the orbit of Mercury, whether visible as the "zodiacal light" or not); and we may conclude that there is no difficulty whatever in accounting for a constancy of solar heat during 5000 years of time past or to come. Even physical astronomy can raise no objection by shewing that the Sun's mass has not ex-

* The star which Mr Hind discovered in April 1848, and which only remained visible for a few weeks, during which period it varied considerably in appearance and brightness, but was always of a "ruddy" colour, may have not experienced meteoric impact enough to make its surface more than red hot.

perienced such an augmentation; for according to the form of the gravitation theory which I have proposed, the added matter is drawn from a space where it acts on the planets with very nearly the same forces as when incorporated in the Sun. This form of the gravitation theory then, which may be proved to require a greater mass of meteoric matter to produce the solar heat than would be required on any other assumption that could be made regarding the previous positions and motions of the meteors, requires not more than it is perfectly possible does fall in to the Sun. Hence I think we may regard the adequacy of the meteoric theory to be fully established.

Let us now consider how much chemical action would be required to produce the same effects, with a view both to test the adequacy of the theory that the Sun is merely a burning mass without a supply of either fuel or dynamical energy from without, and to ascertain the extent to which, in the third theory, the combustion of meteors may contribute, along with their dynamical energies, to the supply of solar heat. Taking the former estimate, 2781 thermal units centigrade, or 3,869,000 foot-lbs. as the rate per second of emission of energy from a square foot of the Sun's surface, equivalent to 7000 horse power, we find that more than ·42 of a lb. of coal per second, or 1500 lbs. per hour would be required to produce heat at the same rate. Now if all the fires of the whole Baltic fleet were heaped up and kept in full combustion, over one or two square yards of surface, and if the surface of a globe all round had every square yard so occupied, where could a sufficient supply of air come from to sustain the combustion? yet such is the condition we must suppose the Sun to be in, according to the hypothesis now under consideration, at least if one of the combining elements be oxygen or any other gas drawn from the surrounding atmosphere. If the products of combustion were gaseous, they would in rising check the necessary supply of fresh air; or if they be solid or liquid (as they might be wholly or partly if the fuel be metallic) they would interfere with the supply of the elements from below. In either or in both ways the fire would be choked, and I think it may be safely affirmed that no such fire could keep alight for more than a few minutes, by any conceivable adaptation of air and fuel. If then the Sun be a burning mass, it must be more analogous to burning gunpowder than to a fire burning in air; and it is quite conceivable that a solid mass, containing within itself all the elements required for combustion, *provided the products of combustion are permanently gaseous*,* could burn off at its surface all round, and actually emit heat as copiously as the Sun. Thus an enormous globe of gun-cotton might, if at first cold, and once set on fire round its surface, get to a permanent rate of burning, in which any internal part would become heated by conduction, sufficiently to ignite, only when nearly approached by the diminishing surface. It is highly probable indeed that such a body might for a time be as large as the Sun, and give out luminous heat as copiously, to be freely radiated into space, without suf-

* On this account gunpowder would not do.

fering more absorption from its atmosphere of transparent gaseous products* than the light of the Sun actually does experience from the dense atmosphere through which it passes. Let us therefore consider at what rate such a body, giving out heat so copiously, would diminish by burning away. The heat of combustion could probably not be so much as 4000 thermal units per pound of matter burned,† the greatest thermal equivalent of chemical action yet ascertained falling considerably short of this. But 2781 thermal units (as found above) are emitted per second from each square foot of the Sun; hence there would be a loss of about ·7 of a pound of matter per square foot per second. Such a loss of matter from every square foot, if of the mean density of the Sun (a little more than that of water), would take off from the mass a layer of about ·5 of a foot thick in a minute, or of about 55 miles thick in a year. At the same rate continued, a mass as large as the Sun is at present would burn away in 8000 years. If the Sun has been burning at that rate in past time, he must have been of double diameter, of quadruple heating power, and of eight-fold mass, only 8000 years ago. We may quite safely conclude then that the Sun does not get its heat by chemical action among particles of matter primitively belonging to his own mass, and we must therefore look to the meteoric theory for fuel, even if we retain the idea of a fire. Now, according to ANDREWS, the heat of combustion of a pound of iron in oxygen gas is 1301 thermal units, and of a pound of potassium in chlorine 2655; a pound of potassium in oxygen 1700 according to JOULE; and carbon in oxygen, according to various observers, 8000. The greatest of these numbers, multiplied by 1390 to reduce to foot-pounds, expresses only the 6000th part, according to Mr WATERSTON's theory, and, according to the form of the Gravitation Theory now proposed, only the 3000th part, of the least amount of dynamical energy a meteor can have on entering the region of ignition in the Sun's atmosphere. Hence a mass of carbon entering the Sun's atmosphere, and there burning with oxygen, could only by combustion give out heat equal to the 3000th part of the heat it cannot but give out from its motion. Probably no kind of known matter (and no meteors reaching the earth have yet brought us decidedly new elements) entering the Sun's atmosphere from space, whatever may be its chemical nature, and whatever its dynamical antecedents, could emit by combustion as much as $\frac{1}{1000}$ of the heat inevitably generated from its motion. It is highly probable that many, if not all, meteors entering the Sun's atmosphere do burn, or enter into some chemical combination with substances which they meet. Probably meteoric iron comes to the Sun in enormous quantities, and burns in his atmosphere just as it does to the earth. But (while probably nearly all the heat and light of the sparks which fly from a steel struck by a flint is due to combustion alone) only $\frac{1}{18000}$ part of the heat and light of a mass of iron entering the

* These would rise and be regularly diffused into space.

† Both the elements that enter into combination are of course included in the weight of the burning matter.

Sun's atmosphere or $\frac{1}{5}$th of the heat and light of such a meteor entering our own, can possibly be due to combustion. Hence the combustion of meteors may be quite disregarded as a source of solar heat.

At the commencement of this communication, it was shown that the heat radiated from the Sun is either taken from a stock of primitive solar heat, or generated by chemical action among materials originally belonging to his mass, or due to meteors falling in from surrounding space. We saw that there are sufficient reasons for utterly rejecting the first hypothesis; we have now proved that the second is untenable; and we may consequently conclude that the third is true, or that meteors falling in from space give rise to the heat which is continually radiated off by the Sun. We have also seen that no appreciable portion of the heat thus produced is due to chemical action, either between the meteors and substances which they meet at the Sun, or among elements of the meteors themselves; and that whatever may have been their original positions or motions relatively to one another or to the Sun, the greater part of them fall in gradually from a state of approximately circular motion, and strike the Sun with the velocity due to half the potential energy of gravitation lost in coming in from an infinite distance to his surface. The other half of this energy goes to generate heat very slowly and diffusely in the resisting medium. Many a meteor, however, we cannot doubt, comes in to the Sun at once in the course of a rectilineal or hyperbolic path, without having spent any appreciable energy in the resisting medium; and, consequently, enters the region of ignition at his surface with a velocity due to the descent from its previous state of motion or rest, and there converts both the dynamical effect of the potential energy of gravitation, and the energy of its previous motion, if it had any, into heat which is instantly radiated off to space. But the reasons stated above make it improbable that more than a very small fraction of the whole solar heat is obtained by meteors coming in thus directly from extra-planetary space.

In conclusion, then, the source of energy from which solar heat is derived is undoubtedly meteoric. It is not any intrinsic energy in the meteors themselves, either potential, as of mutual gravitation or chemical affinities among their elements; or actual, as of relative motions among them. It is altogether dependent on mutual relations between those bodies and the Sun. A portion of it, although very probably not an appreciable portion, is that of motions relative to the Sun, and of independent origin. The principal source, perhaps the sole appreciably efficient source, is in bodies circulating round the Sun at present inside the earth's orbit, and probably seen in the sunlight by us and called "The Zodiacal Light." The store of energy for future sunlight is at present partly dynamical, that of the motions of these bodies round the Sun; and partly potential, that of their gravitation towards the Sun. This latter is gradually being spent, half against the resisting medium, and half in causing a continuous increase of the former. Each meteor thus goes on moving faster and faster, and getting

nearer and nearer the centre, until some time, very suddenly, it gets so much entangled in the solar atmosphere, as to begin to lose velocity. In a few seconds more, it is at rest on the Sun's surface, and the energy given up is vibrated in a minute or two across the district where it was gathered during so many ages, ultimately to penetrate as light the remotest regions of space.

Explanation of Tables.

The following Tables exhibit the principal numerical data regarding the Mechanical Energies of the Solar System.

In Table I., the mass of the Earth is estimated on the assumption that its mean density is five times that of water, and the other masses are shown in their true proportions to that of the Earth, according to data which Professor Piazzi Smyth has kindly communicated to the author.

In Table II., the mechanical values of the rotations of the Sun and Earth are computed on the hypothesis, that the moment of inertia of each sphere is equal the square of its radius multiplied by only one-third of its mass, instead of two-fifths of its mass as would be the case if its matter were of uniform density. These two estimates are only introduced for the sake of comparison with other mechanical values shown in the Table, not having been used in the reasoning.

The numbers in the last column of Table II., showing the times during which the Sun emits quantities of heat mechanically equivalent to the Earth's motion in its orbit, and to its motion of rotation, were first communicated to the Royal Society on the 9th January 1852, in a paper "On the Sources Available to Man for the production of Mechanical Effect." These, and the other numbers in the same column, are the only part of the numerical data either shown in the Tables, or used directly or indirectly in the reasoning on which the present theory is founded, that can possibly require any considerable correction; depending as they do on M. Pouillet's estimate of Solar Heat in thermal units. The extreme difficulties in the way of arriving at this estimate, notwithstanding the remarkably able manner in which they have been met, necessarily leave much uncertainty as to the degree of accuracy of the result. But even if it were two or three times too great or too small, (and there appears no possibility that it can be so far from the truth), the general reasoning by which the Theory of Solar Heat at present communicated is supported, would hold with scarcely altered force.

The mechanical equivalent of the thermic unit, by which the Solar radiation has been reduced to mechanical units is Mr Joule's result—1390 foot-pounds for the thermal unit centigrade—which he determined by direct experiment with so much accuracy, that any correction it may be found to require can scarcely amount to $\frac{1}{200}$ or $\frac{1}{300}$ of its own value.

Table I. Forces and Motions in the Solar System.

	Masses in pounds.	Distances from the Sun's centre, in miles.	Forces of attraction towards the Sun, in terrestrial pounds.	Velocities, in miles per second.
Sun,	$4{,}230{,}000{,}000 \times 10^{21}$	(surface) 441,000	28·61 per lb. of matter	(equator) 1·27
Imaginary solid planet close to the Sun,	1×20^{21}	441,000	$286{,}100 \times 10^{17}$	277
Mercury,	870×10^{21}	36,800,000	$35{,}710 \times 10^{17}$	30·36
Venus,	$10{,}530 \times 10^{21}$	68,700,000	$124{,}200 \times 10^{17}$	22·22
Earth,	$11{,}920 \times 10^{21}$	95,000,000	$73{,}490 \times 10^{17}$	18·89
Mars,	$1{,}579 \times 10^{21}$	144,800,000	$4{,}211 \times 10^{17}$	15·28
Jupiter,	$4{,}037{,}000 \times 10^{21}$	494,300,000	$919{,}400 \times 10^{17}$	8·28
Saturn,	$1{,}208{,}000 \times 10^{21}$	906,200,000	$81{,}855 \times 10^{17}$	6·11
Uranus,	$201{,}490 \times 10^{21}$	1,822,000,000	3.377×10^{17}	4·31
Neptune,	$236{,}380 \times 10^{21}$	2,854,000,000	$1{,}615 \times 10^{17}$	3·44
		Distances from Earth's centre.	Attraction towards Earth in terrestrial pounds.	Velocities relatively to Earth's centre, in miles.
Moon,	136×10^{21}	237,000	378×10^{17}	0·615
Earth's equator,		3,956	1 per lb. of matter.	0·291

TABLE II. MECHANICAL ENERGIES of the SOLAR SYSTEM.

	Potential Energy of gravitation to Sun's surface.		Actual Energy relatively to Sun's centre.	
	In foot-pounds.	Equivalent to supply of Solar Heat, at the present rate of radiation for a period of	In foot-pounds.	Equivalent to supply of Solar Heat, at the present rate of radiation for a period of
Sun, . . .			$967{,}000 \times 10^{30}$	116 yrs. 6 days.
Imaginary planet, of 10^{21} lb. of matter, close to the Sun, . .	0	0	333×10^{29}	1·44 ···
Mercury, . .	57×10^{33}	6 yrs. 214 days	347×10^{30}	15·2 ···
Venus, . . .	697×10^{33}	83 ··· 227 ···	$2{,}252 \times 10^{30}$	98·5 ···
Earth, . . .	790×10^{33}	94 ··· 303 ···	$1{,}843 \times 10^{30}$	80·7 ···
Mars, . . .	105×10^{33}	12 ··· 252 ···	160×10^{30}	7·0 ···
Jupiter, . .	$268{,}800 \times 10^{33}$	32,240	$119{,}980 \times 10^{30}$	14 yrs. 144 ···
Saturn, . .	$80{,}440 \times 10^{33}$	9,650	$19{,}580 \times 10^{30}$	2 ··· 127 ···
Uranus, . .	$13{,}430 \times 10^{33}$	1,610	$1{,}625 \times 10^{30}$	71·2 ···
Neptune, . .	$15{,}750 \times 10^{33}$	1,890	$1{,}217 \times 10^{30}$	53·3 ···
	To the Earth's surface.		Relatively to Earth's centre.	
Moon, . . .	$2{,}846 \times 10^{27}$	3·0 hours	$2{,}347 \times 10^{25}$	1·48 minutes
Earth (rotation),			$14{,}310 \times 10^{25}$	9·03 ···
Total, . . .	$380{,}000 \times 10^{33}$	45,589 years	$1{,}114{,}004 \times 10^{30}$	134 years.

ADDITIONS (May 9, 1854), No. I. *Conclusion of Physical Astronomy against the Extra-planetary Meteoric Theory.*

Meteors which when at great distances possessed, relatively to the centre of gravity of the solar system, velocities not incomparably smaller than the velocity due to gravitation to the Sun's surface, must strike the surfaces of the earth and of the other planets not incomparably less frequently than equal areas of the Sun's surface, and with not incomparably smaller velocities, and consequently must generate heat at the surfaces of the earth and other planets not incomparably less copiously than at equal areas of the Sun's surface. But the whole heat emitted from any part of the Sun's surface is incomparably greater than all that is generated by meteors on an equal area of the earth's surface, and therefore is incomparably greater than all that can be generated at his own surface by meteors coming in with velocities exceeding considerably the velocity due to his attraction from an infinite distance. Hence upon the extra-planetary Meteoric Theory of Solar Heat the quantity of matter required to fall in cannot be much, if at all, less than that required upon the hypothesis that the work done by the Sun's attraction is equal to the mechanical value of the heat emitted from his surface, and must therefore be, as found above, about ·000060 of a pound per square foot per second, or 1900 lb. per square foot in a year. The mean density

of the Sun being about $1\frac{1}{4}$ times that of water, the matter in a pyramidal portion from his centre to a square foot of his surface is about

$$\tfrac{1}{3} \times 441{,}000 \times 5280 \times 1\tfrac{1}{4} \times 64 = 62{,}100{,}000{,}000 \text{ lb.}$$

and the whole annual addition of meteoric matter to the Sun would therefore be

$$\frac{1900}{62{,}100{,}000{,}000} = \frac{1}{32{,}400{,}000}$$

of his own mass. In about six thousand years the Sun would therefore be augmented by $\frac{1}{5000}$ in mass from extra-planetary space. Since the time occupied by each meteor in falling to the Sun from any distance would be much less than the periodic time of a planet revolving at that distance, and since the periodic times of the most distant of the planets is but a small fraction of 6000 years, it follows that the chief effect on the motions of the planetary system produced during such a period by the attraction of the matter falling in would be that depending simply on the augmentation of the central force. To determine this, let M be the Sun's mass at any time t, measured from an epoch 6000 years ago; ω the Earth's mean angular velocity, and a its mean distance at the same time; and $2h$ the constant area described by its radius vector per second. Then we have—

$$\omega^2 a = \frac{M}{a^2}, \quad \text{(centrifugal force)}$$

$$\omega a^2 = h; \quad \text{(equable description of areas)}$$

from which we deduce,

$$a = \frac{h^2}{M},$$

and

$$\omega = \frac{M^2}{h^3}.$$

Now, if M_0 denote the mass of the sun at the epoch from which time is reckoned; since the annual augmentation is about $\frac{1}{32400000}$ of the mass itself, we have

$$M = M_0 \left(1 + \frac{t}{32{,}400{,}000}\right),$$

and

$$M^2 = M_0^2 \left(1 + \frac{2t}{32{,}400{,}000}\right).$$

Hence, if Ω_0 and Ω_T denote the angular velocities at the epoch and at the present time, T; the angular velocity, which is uniformly accelerated during the interval, will have a mean value, Ω, expressed as follows:—

$$\Omega = \tfrac{1}{2}\left(\Omega_0 + \Omega_T\right) = \Omega_T \left\{1 - \tfrac{1}{2}\frac{\Omega_T - \Omega_0}{\Omega_T}\right\} = \Omega_T \left(1 - \frac{T}{32{,}400{,}000}\right);$$

and if Θ denote the angle described in the time T, we have

$$\Theta = \Omega_T \left(T - \frac{T^2}{32{,}400{,}000}\right).$$

To test this conclusion for the case of the earth, let T′ denote the number of revolutions round the Sun in the time T. Then, if the unit in which T is measured be the time of a revolution with the angular velocity Ω_T, we have

$$T' = T - \frac{T^2}{32{,}400{,}000.}$$

Thus, if T be 4000 years, we have

$$T' = 4000 - \frac{16{,}000{,}000}{32{,}400{,}000} = 3999\tfrac{1}{2};$$

or only $3999\frac{1}{2}$ actual years in a period of 4000 times the present year. Similarly, we should find a loss of $\frac{1}{8}$ of a year on a period of 2000 years ago; that is, of about a month and a-half since the Christian era. Thus, if we reckon back about 2000 times the number of days at present in the year, we should find seasons, new and full moons, and eclipses, a month and a half later than would be if the year had been constantly what it is. Now we have abundant historical evidence that there is no such dislocation as this, either in the seasons, or in the lunar phenomena; and it follows that the central attracting mass of the solar system does not receive the augmentation required by the extra-planetary meteoric theory of solar heat. But the reasoning in the preceding paper establishes, with very great probability, a meteoric theory of Solar Heat; and we may therefore conclude that the meteors supplying the Sun with heat have been for thousands of years far within the Earth's orbit.

No. II. *Friction between Vortices of Meteoric Vapour and the Sun's Atmosphere the immediate Cause of Solar Heat.*

It has been shown that the meteors which contribute the energy for Solar Heat must be for thousands of years within the Earth's orbit before falling to the Sun. But a meteor could not remain for half a year there, unless it were revolving round the Sun, with at each instant the elements of a circular or elliptic orbit. Hence, meteors, on their way in to the Sun, must revolve, each thousands of times round him, in orbits which, whatever may have been their primitive eccentricities, must tend to become more and more nearly circular as they become smaller by the effects of the resisting medium. The resistance must be excessively small, even very near the Sun; since a body of such tenuity as a comet, darting at the rate of 365 miles per second within one-seventh of his radius from his surface, comes away without sensible loss of energy. If, as is probable, the atmosphere of that part of space is carried in a *vortex* round the Sun by the meteors and other planets, it may be revolving at nearly the same rates as these bodies at different distances in the principal plane of the solar system; but we cannot conceive it to be revolving in any locality more rapidly than a planet at the same distance.

At one-seventh of the Sun's radius from his surface, this would be about 258 miles per second; and, therefore, a comet approaching so near the Sun, could not have a less velocity relatively to the resisting medium than 107 miles per second, and, if going against the stream, might have as great a relative velocity as 623 miles. On the other hand, the great body of the meteors circulating round the Sun, and carrying the resisting medium along with them, may be moving through it with but small relative velocities; the smaller for each individual meteor, the smaller its dimensions. The effects of the resistance must, therefore, be very gradual in bringing the meteors in to the Sun, even when they are very near his surface: and we cannot tell how many years, or centuries, or thousands of years, each meteor, according to its dimensions, might revolve within a fraction of the Sun's radius from his surface, before falling in, if it continued solid; but we may be sure that it would so revolve long enough to take, in its outer parts at least, nearly the temperature of that portion of space; and therefore, probably, unless it be of some substance infinitely less volatile than any terrestrial or meteoric matter known to us, long enough to be wholly converted into vapour: (the mere fact of a comet* escaping from so near the Sun as has been stated, being enough to show that there is, at such a distance, no sufficient atmospheric pressure to prevent evaporation with so high a temperature). Even the planet Mercury, if the Sun is still bright when it falls in, will, in all probability, be dissipated in vapour long before it reaches the region of intense resistance; instead of (as it would inevitably do if not volatile) falling in solid, and in a very short time (perhaps a few seconds) generating three years' heat, to be radiated off in a flash which would certainly scorch one half of the earth's surface, or perhaps the whole, as we do not know that such an extensive disturbance of the luminiferous medium would be confined by the law of rectilineal propagation. Each meteor, when volatilized, will contribute the actual energy it had before evaporation to a vortex of revolving vapours, approaching the sun spirally to supply the place of the inner parts, which, from moving with enormously greater velocities than the parts of the Sun's surface near them, first lose motion by intense resistance, emitting an equivalent of radiant heat and light, and then, from want of centrifugal force, fall in to the Sun, and, consequently, become condensed to a liquid or solid state at his surface, where they settle. The latent heat absorbed by the meteors in evaporation, and afterwards partially emitted in their condensation at a higher temperature, is

* That a comet may escape with only a slight loss by evaporation, if the resistance is not too great to allow it to escape at all, is easily understood, when we consider that it cannot be for more than a few hours exposed to very intense heat (not more than two or three hours within a distance equal to the Sun's radius from his surface). If it consist of a cloud of solid meteors, the smallest fragments may be wholly evaporated immediately; but all whose dimensions exceed some very moderate limit of a few feet would, unless kept back by the resisting medium and made to circulate round the Sun until evaporated, get away with only a little boiled off from their surfaces.

probably as insensible, in comparison with the heat of friction, as it has been shown the heat of any combustion or chemical action they can experience must be, or as we have tacitly assumed the heat is which is taken and kept by the meteors themselves in approaching from cold space to lodge permanently in the Sun. We may conclude that the Sun's heat is caused, not by solids striking him, or darting through his atmosphere, but by friction in an atmosphere of evaporated meteors, drawn in and condensed by gravitation while brought to rest by the resistance of the Sun's surface. The quantity of meteoric matter required, if falling in solid, would, as we have seen, be such that half the work done by Solar Gravitation on it, in coming from an infinite distance, is equal to the energy of heat emitted from the Sun, and would, therefore, amount to a pound every 2·3 hours per square foot of the Sun's surface; and it will be the same as this, notwithstanding the process of evaporation and condensation actually going on, if, as appears probable enough, the velocity of the vortex of vapour immediately external to the region of intense resistance in all latitudes be nearly equal to that of a planet close to the Sun.

No. III. *On the Distribution of Temperature over the Sun's Surface.*

Not only the larger planets, but the great mass of meteors revolving round the Sun, appear to revolve in planes nearly coinciding with his equator, and therefore such bodies, if solid when drawn in to the Sun, would strike him principally in his equatorial regions, and would cause so much a more copious radiation of heat from those regions than from any other parts of his surface, that the appearance would probably be a line or band of light, instead of the round, bright disc which we see. The nearly uniform radiation which actually takes place from different parts of the Sun's surface appears to be sufficiently accounted for by the distillation of meteors, which, we have seen, must, in all probability, take place from an external region of evaporation at a considerable distance (perhaps several times his radius) inwards to his surface where they are condensed. Whatever be the dynamical condition of the luminous atmosphere of intense resistance, it is clear that there must be a very strong tendency to an equality of atmospheric pressure over the probably liquid surface of the Sun, and that the temperature of the surface must be everywhere kept near that of the physical equilibrium between the vapours and the liquid or solid into which they are distilling. A lowering of temperature in any part would therefore immediately increase the rate of condensation of vapour into it, and so bring a more copious influx of meteoric matter with dynamical energy to supply the deficiency of heat. The various deviations from uniformity which have been observed in the Sun's disc are probably due to eddies which must be continually produced throughout the atmosphere of intense resistance between his surface (which at the equator revolves only at the rate of 1·3 miles per second) and the great vortex of meteoric

vapour, which a few miles outside revolves at the rate of 277 miles per second about the equatorial regions, and (if not at the same) certainly at enormously great rates a few miles from the Sun's surface in other localities. Such eddies may ordinarily be seen as the streaks which have been compared to "the streamers of our northern lights" (HERSCHEL, § 387), and when any one of them sends a root down to the Sun's surface it may cause one of the "minute dark dots or pores" which have been observed, and which, when attentively watched, are found to be always changing in appearance (HERSCHEL). A great rotatory storm, like the tropical hurricanes in the earth's atmosphere, may occasionally result from smaller eddies accidentally combining, or from some disturbing cause originating at once an eddy on a much larger scale than usual, and may traverse the Sun's surface, preventing the distillation of meteoric vapour over a great area, and consequently checking both the supply of dynamical energy for radiant heat in the luminous atmosphere of resistance, and the torrents of condensed meteoric vapours falling to the surface below it. The consequence would be, that the meteoric rain (HERSCHEL's "cloudy stratum,") would be cleared away for a certain space under the central parts of the storm by falling down to the liquid or solid surface, and the luminous atmosphere would lose intensity over a larger space bounded very irregularly by a region of minor eddies, which would cause varying streaks of light. These are exactly the circumstances assumed by Sir WILLIAM HERSCHEL to account for the great spots with their dark centres surrounded by sharply terminated penumbræ inside the abrupt ragged boundaries of the bright surface, and the branching luminous streaks or "faculæ" in the bright surface outside in their neighbourhood.—(Sir JOHN HERSCHEL's *Astronomy*, § 388).

No. IV. (Added August 15, 1854.) *On the Age of the Sun.*

The moment of the Sun's rotatory motion (according to the hypothesis mentioned above in the "Explanation of the Tables" regarding the moment of inertia of his mass) is one-third of his mass multiplied by his radius, multiplied by the linear velocity of his equator; and is therefore equal to that of a planet at his surface having a mass equal to $\frac{1{\cdot}27}{3\times 277}=\frac{1}{650}$ of his own mass. This is equal to the quantity of meteoric matter which would fall in during 25,000 years, at the present rate; and therefore 25,000 years is the time the Sun would take to acquire his actual motion of rotation, by the incorporation of meteors, if these bodies were each revolving in the plane of his equator immediately before entering the region of intense resistance. But it has been shown to be probable that a great space round the Sun is occupied by a vortex of evaporated meteors, and that the incorporation of meteoric matter takes place in reality by the condensation of vapour in a stratum close to his surface all round. It appears not improbable that the

tangential velocity of this vortex immediately external to the radiant region of intense resistance may be found to be, in all solar latitudes, very nearly that of a planet close to the Sun. If it be so, the moment of the motion communicated to the Sun by any mass of meteoric matter will be $\frac{3{\cdot}14159\ldots\ldots}{4}$ of what would be communicated by the incorporation of an equatorial planet of equal mass: as much as $\frac{1}{510}$ of the Sun's mass would have to fall in to produce his present rotation: and 32,000 years would be the time in which this would take place, at the present rate of meteoric incorporation as estimated above.

It will be a very interesting hydrodynamical problem, to fully investigate the motion of the meteoric vortex; and among results to be derived from it will be strict estimates of the contribution to the Sun's rotatory motion, and of the quantity of heat generated, by any amount of meteoric matter in becoming incorporated. With these, and with an accurate determination of the rate at which the Sun radiates heat, we should be able to fix, with certainty, the augmentation of his velocity of rotation actually taking place at present from year to year, and to estimate the time during which the existing rotation would be acquired by meteoric incorporation going on always at the present rate and in the present manner. Whatever this time (which I shall call T years, to avoid circumlocution below) may be, it probably will not be found to differ very widely from the preceding estimate of 32,000 years.

Now, from the fact that the Sun's equator, the planets' orbits, and the Zodiacal Light, all lie nearly in one plane, it appears highly probable that the Sun's present motion has really been acquired by the incorporation of meteors. It is certain that the present manner and rate of meteoric action cannot have been going on for more than the indicated period (T), without giving the Sun a greater rotatory motion than he has, unless (which is very improbable) he were previously rotating in a contrary direction round the same axis: and, at only the present rate, it cannot have been going on for less than that time, unless the Sun has been created with a rotatory motion round his present axis, or has acquired such a motion from some independent mechanical action. The actual rate of Solar radiation in time past may, for all we now know, have been sometimes much greater and sometimes much less than at present; and there probably has been a time before, when meteors in abundance fell direct to the Sun from extra-planetary space, some getting stopped on their way by the Earth, and illuminating it by friction in its atmosphere and impact at its surface. But the kind of meteoric action now going on, has in all probability produced neither more nor less than T times the quantity of heat now emitted from the Sun in one year. All things considered, it seems not improbable that the Earth has been efficiently illuminated by the Sun alone for not many times more or less than 32,000 years.

As for the future, it will be a most interesting problem to determine the mass

of the Zodiacal Light (that is, matter external to the Sun's mass, and within the Earth's orbit), by the perturbations it may probably enough be discovered to produce in the motions of the visible planets. It could scarcely, I think, amount to $\frac{1}{50}$ of the Sun's mass (probably not to nearly as much), without producing such perturbations as could not have been overlooked in the present state of astronomical science; and we have seen that meteors amounting to $\frac{1}{5000}$ of the Sun's mass, must, at the present estimated rate, fall in in 3000 years. I conclude that Sunlight cannot last as at present for 300,000 years.

The continual acceleration of the Sun's rotatory motion, which the preceding theory indicates, must, sooner or later, be tested by direct observation. The rate of acceleration (which for many thousands of years past and to come must remain sensibly constant, if the solar radiation continues so), is such that the angular velocity is increased annually by $\frac{1}{T}$ of its present value. If T be 32,000, according to the preceding conjectural estimate, the effect in 53 years would amount to diminishing the period of the Sun's revolution by an hour; and the actual effect cannot, according to the theory, be incomparably greater or less. It is just possible that a careful comparison of early with recent observations on the apparent motions of the dark spots may demonstrate this variation; but as some of the most accurate of recent observations of this kind have led to estimates of the period of revolution* differing from one another by as much as 8 hours, it is more probable that, unless some way be discovered for taking into account the motions of the spots themselves with reference to the mass, centuries will elapse before direct evidence can be had either for or against the anticipated acceleration of the Sun's rotatory motion.

	Days.	Hours.	Minutes.
* According to Böhm, . .	25	12	30
„ Laugier, . .	25	8	10
„ Petersen, . .	25	4	30

(See Encyc. Brit., 8th edit. vol. iv., p. 87.) The discrepancies are probably due to proper motions of the spots, which, from the explanation given above in Addition iii., may be expected to be very considerable.

VIII.—*On the Meteorology of the English Lake District, including the Results of Experiments on the Fall of Rain, the Temperature, the Dew Point, and the Humidity of the Atmosphere, at various heights on the Mountains, up to* 3166 *feet above the Sea Level,—for the years* 1851, 1852, *and* 1853. By John Fletcher Miller, Ph. D., F.R.S., Fellow of the Royal Astronomical Society, Associate of the Institution of Civil Engineers, Member of the British Meteorological Society, Corresponding Member of the Literary and Philosophical Society of Manchester, &c.

(Communicated 1st May 1854.)

INTRODUCTORY REMARKS.

The experiments on the rain-fall in the lake district valleys were commenced in 1844,—the mountain gauges were planted in the spring of 1846,—the hygrometrical observations were instituted at the beginning of the year 1852,—and the entire investigation was brought to a close at the end of the year 1853, when its main objects were considered to have been satisfactorily answered and attained.

The tabular observations and general results of the inquiry up to the close of the year 1850, were communicated to the Royal Society of London in four papers, which are published in the Philosophical Transactions of that body. The fifth and concluding paper (comprising the observations, &c. for the years 1851–53 inclusive) I have now the honour to present to the Royal Society of Edinburgh.

In all previous publications, I have thought it desirable to notice any accidents or occurrences which interfered with the continuity of the experiments, or which might affect the accuracy of the tabular or comparative results. As was to be expected, from the great number of instruments in operation, and the exposed situation of many of them, such mishaps, either designed or fortuitous, occasionally occur; nor is the past triennial period exempted from its share of casualties.

Early in February 1852, the Langdale Head Gauge was thrown into the river by some mischievous person, and rendered useless; a new instrument was immediately made and forwarded, which was stolen early in November the same year, and the station was then abandoned.

On the 6th of March 1852, a new and extra strong metal bottle was forwarded for the Stye Head Gauge. The old receiver, when examined at Whitehaven, was found to be water-tight when perfectly upright or level, but it leaked very slowly when placed on a slightly inclined surface. The loss by leakage at this station during the winter of 1851–2, if any, must have been trifling.

The rain gauges used were 5 inches in diameter, and their height above the ground was about 2 feet. They were all made under my own superintendence. The receivers of the mountain gauges were capacious vessels made of extra heavy copper double lapped at the seams, with the bottoms convex inwards, to enable them more readily to resist the expansive force of the water during its conversion into ice; they were calculated to hold 60 or 70 inches of rain.

The glass metres (HOWARD'S) employed were graduated by Mr BATE of the Poultry, or by his successor, Mr POTTER.

The Roman numerals at the head of the Tables refer to corresponding numbers on a map of the Lake District, on which the gauges are laid down in small coloured circles, each with its tabular number attached.

TABLE I.—SYNOPSIS of the FALL of RAIN in the ENGLISH LAKE DISTRICT, &c., in the Year 1851.

No.	I.	III.	IV.	V.		VI.	VII.	VIII.	IX.	X.	XI.	XIII.	XIV.		XV.	XVI.	XVII.	XVIII.	XIX.	XX.	Prevailing Winds.		
	Whitehaven													Westmoreland.*				Borrowdale.					
1851.	High Street, 90 feet above Sea.	St James' Church Steeple, 78 feet above the Street.	The Flosh, 3 miles south of Whitehaven.	Tarn Bank, near Cockermouth, 207 feet above the Sea.	Broughton Moor, near Maryport, 410 feet above the Sea.	Bassenthwaite Halls, 210 feet above Sea.	Keswick, 258 feet above the Sea.	Gillerthwaite, Ennerdale, 396 feet above Sea.	Loweswater, 336 feet above Sea.	Foot of Crummock Lake, 260 feet above Sea.	Gatesgarth, 290 feet above Sea.	Eskdale Head.	Wastdale Head, 247 ft. above Sea.	Bowness,† 240 feet above Sea.	The How, Troutbeck, 300 feet above Sea.	Ambleside, 190 feet above Sea.	Langdale Head, 250 feet above Sea.	Seathwaite, 368 ft. ab. Sea. At 10 inches above surface.	Seathwaite, 368 ft. ab. Sea. At 22 inches above surface.	Stonethwaite, 330 feet above Sea.	At Crummock Lake, 2 observations daily.	At Seathwaite, 1 observation daily.	At the Coast, 2 observations daily.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.			
January	9·412	7·180	10·09	10·96	4·94	10·18	15·212	17·04	15·045	21·69	20·70	15·59	14·47	13·288	17·636	19·54	25·53	28·63	28·06	25·85	S.	S. & SW.	SW.
February	3·775	2·430	4·56	3·38	4·21	3·19	4·909	9·00	6·602	6·95	10·90	7·05	11·66	5·778	8·928	9·58	12·79	15·33	14·98	11·76	SW.	SW.	SW.
March	3·458	3·102	4·38	2·71	3·17	4·27	3·712	6·68	4·350	5·26	6·75	5·36	6·94	4·853	4·610	5·31	8·28	9·36	9·07	7·42	W.	W.	SW.
April .	2·301	1·790	2·66	2·30	1·31	2·18	2·856	3·89	3·515	4·02	4·47	2·50	4·16	2·846	3·449	3·41	5·89	6·08	5·55	4·04	E.	NE.	NE.
May .	1·065	·683	1·25	·94	1·06	1·27	2·196	2·81	1·920	2·74	3·70	3·94	4·36	1·806	2·278	2·34	3·79	4·53	4·42	3·67	NW. var.	NW. var.	NW.
June .	3·883	2·757	4·56	2·26	2·62	3·25	4·008	6·90	4·540	6·09	8·23	6·97	8·90	5·106	6·928	6·55	8·52	11·63	11·51	9·50	NW. & SW.	NW. & SW.	W.
July .	3·470	2·174	3·83	3·55	2·82	5·21	7·970	8·18	6·820	7·71	13·05	7·78	9·01	6·086	5·949	8·46	11·37	14·47	14·20	13·50	NW.	NW.	NW. var.
August	4·068	3·345	5·82	3·70	3·41	3·56	4·736	6·89	6·615	7·19	8·17	7·67	10·89	5·213	7·477	7·20	11·68	13·16	12·62	9·98	SW. & W.	SW.	SW.
Sept. .	2·737	2·119	3·64	2·42	1·49	2·80	3·962	4·60	2·965	3·92	4·99	6·30	3·79	2·468	4·123	4·52	4·03	4·30	4·29	3·81	NW.	S. var.	SW.
October	4·726	3·043	5·51	4·82	4·22	5·80	7·206	11·36	8·770	10·97	15·57	9·16	14·74	6·994	10·138	8·38	15·77	20·38	20·04	14·29	S. & W.	SW. & NW.	SW. & W.
November	2·547	2·107	3·69	1·17	1·19	1·74	2·192	3·00	2·927	3·04	1·83	2·15	3·96	2·313	2·978	2·52	3·21	3·74	3·58	2·68	E.	NE.	NE.
December	1·678	1·378	1·67	2·56	1·74	1·96	3·368	3·93	3·030	4·50	8·64	4·11	5·06	3·488	6·268	6·41	5·99	7·99	7·54	7·62	SW. & NW.	NW.	SW.
Inches	43·120	32·108	51·66	40·77‡	32·18	45·41	62·327	84·38§	67·099	84·08	107·00	78·58	97·94	60·239	80·762	84·22	116·85	139·60	135·86	114·12	NW.	NW.	SW.

* At Kendal, 47·561 inches; at Selside, near Kendal, 70·518 inches; and at Cartmel, 44·55 inches.
‡ Mean (for 3 years) at Tarn Bank, near Cockermouth, 40·16 inches, and on Broughton Moor, 32·78 inches.
† Windermere Lake is 128 feet above the Sea.
§ Mean (for 8 years) at Gillerthwaite, 79·83 inches.

TABLE II.—WET DAYS.

1851.	Whitehaven.	The Flosh.	Tarn Bank, Cockermouth.	Bassenthwaite Halls.	Keswick.	Loweswater Lake.	Crummock Lake.	Wastdale Head.	Troutbeck.	Ambleside.	Rowness, Westmoreland.	Langdale Head.	Seathwaite.	Stonethwaite.
January	26	26	24	26	29	28	27	29	28	24	28	29	27	26
February	15	15	14	13	15	14	13	16	13	13	15	16	15	15
March .	17	22	16	20	19	21	21	26	19	19	22	22	22	21
April .	16	16	15	15	14	14	15	20	14	14	15	18	17	17
May . .	15	15	13	13	17	16	19	20	16	12	16	16	21	20
June . .	16	18	12	16	17	19	17	20	16	15	16	20	20	17
July . .	13	15	15	23	18	14	15	15	17	14	17	15	18	16
August .	18	18	19	17	18	19	19	19	19	17	19	19	19	19
September	8	9	7	8	9	9	9	8	8	7	20	8	9	8
October .	24	25	21	22	25	24	24	25	23	21	25	24	25	23
November	16	14	16	12	16	13	15	17	11	9	14	16	15	13
December	12	11	14	16	14	11	12	17	11	13	15	12	11	11
1851. ·	196	204	186	201	211	202	206	232	195	178	222	215	219	206

TABLE III.—Shewing the QUANTITY of RAIN received by the MOUNTAIN GAUGES, in the year 1851.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
							THE VALLEY.		BORROWDALE.		
1851.	Sca Fell Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.	Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Rigg, 924 feet above the Sea.	To the West, Wastdale, 247 ft. above Sea.	To the S.E., Eskdale, height unknown.	Seatollar Common, 1388 feet above the Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 feet above Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . .	Fr.	15·23	*6·90	22·00	14·40	13·50	14·47	15·59	26·90	38·86	28·63
February . .	†Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	11·66	7·05	15·00	17·36	15·33
March . . .	Fr.	14·55	Fr.	18·26	16·26	16·86	6·94	5·36	7·78	9·93	9·36
April . . .	Fr.	3·36	Fr.	3·10	4·36	4·14	4·16	2·50	5·07	6·02	6·08
May	16·89	5·50	19·43	5·17	3·92	4·35	4·36	3·94	3·85	4·72	4·53
June . . .	6·83	8·54	6·91	10·92	7·80	8·20	8·90	6·97	11·79	12·88	11·63
July . . .	§11·30	9·69	11·35	14·00	9·71	8·98	9·01	7·78	18·44	19·12	14·47
August . .	10·80	10·92	11·22	15·72	12·60	8·95	10·89	7·67	13·36	17·04	13·16
September .	4·72	5·75	4·08	4·90	5·56	5·62	3·79	6·30	4·13	5·86	4·30
October . .	‖9·11	11·00	8·81	19·90	16·11	11·25	14·74	9·16	21·16	23·45	20·38
November .	Fr.	Fr.	Fr.	Fr.	Fr.	4·08	3·96	2·15	3·84	4·89	3·74
December .	11·64	11·75	12·53	11·32	9·44	3·58	5·06	4·11	10·10	9·49	7·99
Inches . . .	71·29	96·29	81·23	125·29	100·16	89·51	97·94	78·58	141·42	169·62	139·60

* The Gabel Gauge was also frozen, and the funnel filled with snow; the Receiver was brought down to the valley and its contents liquefied.

† *February* 28. The Mountain Gauges were all frozen up.

‡ *May* 31. The Sca Fell Gauge was frozen up for seven months, viz., from the latter part of October 1850, till near the end of May 1851, an unusually long period. Snow fell on the tops so late as the 4th of June.

The temperature was unnaturally low, rarely reaching 60°, till the 27th of June, when it suddenly rose to 77° at the coast; and on the 28th, 29th and 30th, the thermometer attained to 82°, 83°·5, and 79°. At Seathwaite, the maxima on these days were 79°, 79°, and 76°, respectively.

§ The return of rain on Sca Fell for July, is only 4·19 inches. The registrar says he cannot account for the relative smallness of the quantity, unless it has been caused by partial thunder rains; but as I conceive no adequate physical cause can be adduced for so great a deficiency, I have ventured to make the quantity nearly the same as on the Gabel.

‖ Some ice left in Sca Fell and Gabel Gauges,—*October* 31*st*.

December 31. The Sca Fell and Gabel Receivers were brought down to the hamlet, and their frozen contents liquefied.

TABLE IV.—For the SUMMER MONTHS.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
1851.	Sca Fell Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.	Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Rigg, 924 feet above the Sea.	THE VALLEY.		BORROWDALE.		
							To the West, Wastdale, 247 ft. above Sea.	To the S.E., Eskdale, ht. unknown.	Seatollar Common, 1388 ft. above Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 ft. above the Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
May . . .	3·20	5·50	3·40	5·17	3·92	4·35	4·36	3·94	3·85	4·72	4·53
June . . .	6·83	8·54	6·91	10·92	7·80	8·20	8·90	6·97	11·79	12·88	11·63
July . . .	11·30	9·69	11·35	14·00	9·71	8·98	9·01	7·78	18·44	19·12	14·47
August . .	10·80	10·92	11·22	15·72	12·60	8·95	10·89	7·67	13·36	17·04	13·16
September .	4·72	5·75	4·08	4·90	5·56	5·62	3·79	6·30	4·13	5·86	4·30
October . .	9·11	11·00	8·81	19·90	16·11	11·25	14·74	9·16	21·16	23·45	20·38
Inches. . .	45·96	51·40	45·77	70·61	55·70	47·35	51·69	41·82	72·73	83·07	68·47

TABLE V.—For the WINTER MONTHS.

1851.	Sca Fell Pike.	Lingmell.	Great Gabel.	Sprinkling Tarn.	Stye Head.	Brant Rigg.	THE VALLEY.		Seatollar Common.	The Stye.	The Valley, Seathwaite.
							To the West, Wastdale.	To the SE. Eskdale.			
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January .	Fr.	15·23	6·90	22·00	14·40	13·50	14·47	15·59	26·90	38·86	28·63
February .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	11·66	7·05	15·00	17·36	15·33
March . .	Fr.	14·55	Fr.	18·26	16·26	16·86	6·94	5·36	7·78	9·93	9·36
April . .	13·69	3·36	16·03	3·10	4·36	4·14	4·16	2·50	5·07	6·02	6·08
November .	Fr.	Fr.	Fr.	Fr.	Fr.	4·08	3·96	2·15	3·84	4·89	3·74
December .	11·64	11·75	12·53	11·32	9·44	3·58	5·06	4·11	10·10	9·49	7·99
Inches. . .	25·33	44·89	35·46	54·68	44·46	42·16	46·25	36·76	68·69	86·55	71·13

TABLE VI.—TEMPERATURE at SEATHWAITE, BORROWDALE, 368 feet above the Sea-level.

1851.	Absolute.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Mean at 9 A.M.	On Grass.				Prevailing Winds.
	Maximum.	Minimum.					Absolute Minimum.	Mean.	Radiation.		
									Maximum.	Minimum.	
	°		°	°	°	°	°	°	°	°	
January .	52	27	44·56	31·92	39·74	40·61	21·5	31·50	9·5	3·42	S. & SW.
February .	50·5	29	43·89	36·41	40·15	39·51	21	29·20	11·5	7·21	SW.
March . .	49	28	44·50	37·63	41·06	40·03	23·5	32·38	10·5	5·25	W.
April . .	54	31	48·82	38·68	43·75	42·26	18	30·85	14	7·83	NE.
May . . .	66	33·5	54·13	41·15	49·44	49·11	21·5	36·38	16	8·07	NW. var.
June . . .	79	40	59·71	50·26	54·98	54·16	29	44·56	16·5	5·70	NW. SW.
July . . .	80	44	61·10	52·25	56·82	56·90	30	47·12	15	5·13	NW.
August . .	70	47·5	62·77	53·51	58·15	57·21	37	47·51	12·5	6·00	SW.
September .	66	39	59·11	47·46	53·28	52·43	28·5	40·53	17	6·93	S. var.
October . .	58	34	53·06	45·80	49·43	49·85	23·5	40·93	12	4·87	SW. NW.
November .	48	27	40·68	34·26	37·47	36·30	16	27·10	12·5	7·16	NE.
December .	53	26	44·42	38·37	41·39	40·50	17	33·51	14	4·80	NW.
1851. . .	60·45	33·8	51·54	42·83	47·13	46·60	23·9	36·80	13·4	6·03	NW.

Note.—The maximum and minimum thermometers were selected and tested by Mr GLAISHER, and found to be practically correct; the spirit thermometer, exposed on grass, has been compared with the maximum thermometer throughout the scale, and its readings are corrected for index error.

TABLE VII.—TEMPERATURE at WHITEHAVEN, on the West Coast, 90 feet above the Sea-level, and 17 miles distant in a direct line from the Hamlet of Seathwaite, Borrowdale.

1851.	Absolute.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Mean at 9 A.M.	Naked Thermometers on Grass Plot.								In Sun's rays.		
							Absolute Minimum.		Mean.		Radiation.						
											Minimum.		Mean.				
	Maximum.	Minimum.					On Grass.	On Wool on Grass.	On Grass.	On Wool on Grass.	On Grass.	On Wool on Grass	On Grass.	On Wool on Grass.	Maximum.	Mean.	Solar Radiation.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January	52	31	45·35	39·79	42·57	42·02	24·5	20	34·99	33·66	8·5	12	4·80	6·13	53	47·2	1·85
February	50	32·5	45·43	37·91	41·67	40·70	24	20	32·74	30·72	9·3	13	5·17	7·19	69	54·7	9·27
March .	54	30·5	48·00	38·64	43·32	43·16	23	17	34·48	32·12	7·5	14·5	4·16	6·52	81·5	66·7	18·70
April .	60	31	51·85	39·20	45·52	45·66	23	18	33·79	30·53	11	16	5·41	8·67	99	75·3	23·45
May . .	66·3	33	56·46	45·34	50·90	52·24	26·5	20·5	39·31	36·31	15	19·5	6·03	9·03	105	85·5	29·04
June .	83·5	41	62·38	51·13	56·75	58·10	35	31·5	45·41	43·11	12·5	16·5	5·72	8·02	110	86·5	24·12
July . .	74	44	64·33	53·98	59·16	59·89	37	32	49·25	47·42	7·5	13·5	4·73	6·56	104	86·5	22·17
August .	74	47·5	65·76	54·77	60·26	60·34	39·5	34·5	49·88	47·63	10	17·5	4·89	7·14	105	91	25·24
September	69·5	41	61·45	48·43	54·94	56·55	35	27·5	43·62	39·59	13·5	21·5	4·81	8·81	98	80	18·55
October	63	33	56·98	48·24	52·61	51·94	29·5	28	43·87	41·27	8	12·5	4·37	6·97	76	65·5	8·52
November	49	28·5	43·46	35·71	39·59	38·94	19·8	16·5	29·14	27·57	12·4	12·5	6·57	8·14	62	49·8	6·34
December	52·5	29	44·88	39·61	42·24	41·71	18·3	14	34·49	32·47	12·8	17	5·12	7·14	52	45·3	0·42
1851.	62·3	35·1	53·86	44·39	49·12	49·28	27·9	23·3	39·21	36·86	10·6	16·3	5·15	7·53	81·5	69·5	15·64

TABLE VIII.—SYNOPSIS of the FALL of RAIN in the ENGLISH LAKE DISTRICT, &c., in the Year 1852.

No.	I.	III.	IV.	VI.	VII.	IX.	X.	XI.	XIII.	XIV.				XV.	XVI.	XVII.	XVIII.	XIX.	XX.	Prevailing Winds.		
	Whitehaven										Westmoreland.						Borrowdale.					
																	Seathwaite.					
1852.	High Street, 90 feet above the Sea.	St James's Church Steeple, 78 feet above the Street.	The Flosh, 3 miles south of Whitehaven.	Bassenthwaite Halls, 210 feet above the Sea.	Keswick, 258 feet above the Sea.	Loweswater, 336 feet above the Sea.	Foot of Crummock Lake, 260 feet above the Sea.	Gatesgarth, 290 feet above the Sea.	Eskdale Head, height unknown.	Wastdale Head, 247 feet above the Sea.	Kendal, 126 feet above the Sea.	Selside, 6 miles N. of Kendal, 736 feet above the Sea.	Bowness, Windermere, 240 feet above the Sea.	The How, Troutbeck, 503 feet above the Sea.	Ambleside, 190 ? feet above the Sea.	Langdale Head, 250 ? feet above the Sea.	At 10 inches above the surface.	At 22 inches above the surface.	Stonethwaite, 340 feet above the Sea.	At Crummock Lake, two observations daily.	At Seathwaite, one observation daily.	At the Coast, two observations daily.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.			
January	8·011	4·995	9·09	10·21	14·641	14·860	19·76	24·76	15·54	17·32	9·886	14·156	12.635	21·732	20·37	22·37	27·65	26·16	24·56	SW.	SW.	SW.
February	3·551	2·535	4·75	4·18	7·054	6·560	9·08	13·89	8·67	13·07	7·907	13·874	10·134	15·722	13·00	17·55	20·05	18·27	15·72	NW.	W.	SW.
March .	·258	·158	·23	·46	·388	·275	·60	·62	·37	·70	·210	·293	·425	·264	·23	·30	·98	1·15	.81	SE. and E.	SE.	Easterly.
April .	1·191	1·109	1·21	·89	.550	·665	·63	·75	1·95	1·47	1·112	1·197	1·110	·961	·91	·90	·74	·74	·60	E. and SE.	E. var.	Easterly.
May . .	4·203	3·640	4·95	3·33	4·580	5·370	7.24	10·45	5·78	8·83	4·052	6·058	4·004	5·341	6·31	7·41	11·59	10·86	8·64	E.	E. var.	NE.
June . .	3·513	2·558	3·90	4·15	5·980	5·029	7·54	10·55	7·34	8·75	4·984	8·984	7·487	7·510	5·69	10·41	12·33	12·36	9·23	SW.	SW.	SW.
July . .	2·923	2·330	2·46	3·02	3·022	4·575	5·26	4·94	3·93	5·65	3·448	7·574	3·449	5·056	4·72	5·96	7·65	7·57	4.60	S. and SE.	S.	NE. & SW.
August .	5·150	4·526	6·10	5·03	6.699	7·245	8·81	10·08	9·12	9.77	7·618	7.871	6·507	9·954	8·64	9·23	12·37	12·30	9·82	S.	S.	SW.
September	2·136	1·534	2·26	4·30	3·553	2·740	3·72	4·42	2·69	3·20	2·121	2·810	2·688	3·891	3·49	4·71	4·64	4·76	4·23	Variable.	W. var.	N.Easterly.
October .	3·041	2·272	3·95	3·96	4·958	5·480	6·15	7·26	6·05	7·71	3·668	4·935	4·933	5·742	4·78	7·08	8·44	8·16	5·90	NW. var.	NW. var.	SW.
November	5·051	3·053	5·05	6·27	9·480	8·030	11·34	15·52	10·08	11·70	7·582	8·942	9·910	13·696	11·80	15·47	17·47	17·08	16·72	SW. var.	SW. var.	SW.
December	11·002	7·247	11·23	13·91	19·069	19·700	23·27	31·64	17·33	21·41	12·766	15·791	16·315	25·765	24·39	*32·70	32·83	31·47	33·03	SW.	SW.	SW.
1852.	50·030	35·957	55·18	59·71	79·974	80·529	103·40	134·88	88·85	109·58	65·354	92·485	79·597	115·634	104·33	134·09	156·74	150·88	133·86	SW. var.	SW. var.	SW.
1851.	43·120	32·110	51·66	45·41	62·327	67.100	84·08	107·00	78·58	97·94	47·561	70·518	60·240	80·760	84·22	116·85	139·60	135·86	114·12	NW.	NW.	SW.
1850.	40·473	28·636	51·28	46·97	59·528	68·798	85·66	108·84	81·69	108·76	49·576	76·391	64·284	79·808	79·85	126·56	143 96	...	105·81	SW. & NW.	NW. & SW.	SW.
1849.	38·999	28·210	45·13	40·00	48·800	55·280	70·21	97·09	71·22	107·22	48·068	70·387	61·557	75·420	65·32	107·83	125·47	121·57	94·27	SW.	SW.	SW.
1848.	47·342	36·344	60·82	47·06	66·407	76·668	98·07	133·55	86·78	115·32	56·314	...	...	91·347	77·58	130·38	160·89	157·22	130·24	SW.	SW.	SW.
1847.	42·921	30·713	47·80	44·45	58·286	66·296	82·32	106·25	74·93	96·34	52·197	...	...	78·004	...	112·95	129·24	126·80	106·21	W.	SW.	SW.
1846.	49·134	35·422	55·16	...	67·678	79·249	96·47	121·90	...	106·93	52·365	...	...	77·719	...	127·40	143·51	...	...	SW.	SW.	SW.
1845.	49·207	33·489	53·00	...	62·202	69·542	87·48	124·13	...	108·55	53·346	...	...	76·305	...	136·00	151·87	...	...	SW. var.		SW. var.
1844.	36·723	27·862	39·31	...	40·629	49·829	61·46	...	...	80·01	43·102	..	...	51·986	...	...	...	...	...	...	...	...
Means.	44·216	32·082	51·26	47·26	60·648	68·143	85·46	116·70	80·34	103·40	51·977	77·495	61·412	80·776	82·26	124·01	143·91 †	138·46	114·08	SW.	SW.	SW.

* See Introductory Remarks.

† Mean at Seathwaite, for 5 years, 142·38 inches.

TABLE IX.—WET DAYS.

1852.	Whitehaven.	The Flosh.	Bassenthwaite Halls.	Keswick.	Loweswater Lake.	Crummock Lake.	Wastdale Head.	Kendal.	Selside.	Bowness.	Troutbeck.	Ambleside.	Langdale Head.	Seathwaite.	Stonethwaite.
January	23	23	21	29	27	26	26	21	24	26	26	25	27	28	26
February	16	16	12	15	18	16	17	13	16	17	18	15	17	17	16
March .	5	4	4	4	4	4	5	3	4	4	5	3	5	6	6
April .	4	4	3	4	3	3	3	3	4	5	3	4	4	3	3
May . .	12	13	12	14	13	14	14	15	14	12	10	14	15	16	16
June . .	19	21	20	25	21	20	26	21	15	26	21	21	26	25	25
July . .	15	13	13	14	15	15	15	14	17	19	15	16	18	18	16
August	19	19	16	20	21	19	19	17	17	21	19	28	19	22	19
September	10	13	10	13	14	10	15	14	5	16	15	11	14	16	14
October	16	15	23	16	15	16	18	15	8	17	17	15	16	17	16
November	21	23	22	22	22	21	24	24	10	24	24	24	23	23	21
December	30	30	28	28	28	30	30	31	22	30	29	28	30	30	29
Days.	190	191	187	204	201	194	212	194	156	217	202	204	214	221	207

TABLE X.—Shewing the QUANTITY of RAIN received by the MOUNTAIN GAUGES, in the year 1852.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
	Sca Fell.						The Valley.		Borrowdale.		
1852.	The Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.	Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Rigg, 924 feet above the Sea.	To the West, Wastdale, 247 ft. above Sea.	To the SE. Eskdale, height unknown.	Seatollar Common, 1338 feet above the Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 feet above Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . .	Fr.	16·00	Fr.	19·25	17·18	13·21	17·32	15·54	30·08	27·60	27·65
February . .	Fr.	Fr.	Fr.	Fr.	12·40	10·29	13·07	8·67	19·76	22·18	20·05
March . .	Fr. 1·11	11·62	Fr. 4·60	15·13	·45	·53	·70	·37	1·07	·92	·98
April . . .	18·68	1·15	15·80	1·08	1·19	1·06	1·17	1·95	·67	·86	·74
May . . .	6·63	8·06	6·18	11·01	9·57	7·69	8·83	5·78	11·38	12·01	11·59
June . . .	8·20	8·88	8·00	12·80	12·00	9·57	8·75	7·34	12·14	14·13	12·33
July . . .	5·68	5·63	8·00	8·40	8·25	7·07	5·65	3·93	7·13	9·08	7·65
August . .	8·98	10·00	10·58	13·71	12·55	10·00	9·77	9·12	11·88	14·12	12·37
September .	4·00	4·28	5·07	4·72	4·60	4·34	3·20	2·69	4·50	5·13	4·64
October .. .	8·02	7·12	6·03	7·74	8·00	7·05	7·71	6·05	8·37	9·07	8·44
November .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	11·70	10·08	17·23	17·70	17·47
December .	20·00	27·48	21·19	40·62	38·00	27·40	21·41	17·33	32·38	31·60	32·83
1852.	81·30	100·22	85·75	134·79	124·19	98·21	109·58	88·85	156·59	167·73	156·74
1851.	71·29	96·29	81·23	125·29	100·16	89·51	97·94	78·58	141·42	169·62	139·60

TABLE XI.—For the SUMMER MONTHS.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
1852.	SCA FELL.		Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Rigg, 924 feet above the Sea.	THE VALLEY.		BORROWDALE.		
	Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.					To the West, Wastdale, 247 ft. above Sea.	To the SE., Eskdale, height unknown.	Seatollar Common, 1338 feet above the Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 feet above Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
May . . .	6·63	8·06	6·48	11·04	9·57	7·69	8·83	5·78	11·38	12·04	11·59
June . . .	8·20	8·88	8·00	12·80	12·00	9·57	8·75	7·31	12·14	14·13	12·33
July . . .	5·68	5·63	8·00	8·40	8·25	7·07	5·65	3·93	7·13	9·08	7·65
August . .	8·98	10·00	10·58	13·71	12·55	10·00	9·77	9·12	11·88	14·12	12·37
September .	4·00	4·28	5·07	4·72	4·60	4·31	3·20	2·69	4·50	5·43	4·64
October . .	8·02	7·12	6·03	7·74	8·00	7·05	7·71	6·05	8·37	9·07	8·44
1852.	41·51	43·97	41·16	58·41	54·97	45·72	43·91	34·91	55·40	63·87	57·02
1851.	45·96	51·40	45·77	70·61	55·70	47·35	51·69	41·82	72·73	83·07	68·47

TABLE XII.—For the WINTER MONTHS.

1852.	SCA FELL.		Great Gabel.	Sprinkling Tarn.	Stye Head.	Brant Rigg.	THE VALLEY.		BORROWDALE.		
	The Pike.	Lingmell.					To the West, Wastdale.	To the SE., Eskdale.	Seatollar Common.	On the Stye.	The Valley, Seathwaite.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . .	Fr.	16·00	Fr.	19·25	17·18	13·24	17·32	15·54	30·08	27·60	27·65
February . .	Fr.	Fr.	Fr.	Fr.	12·40	10·29	13·07	8·67	19·76	22·18	20·05
March . . .	Fr. 1·11	11·62	Fr. 4·60	15·43	·45	·53	·70	·37	1·07	·92	·98
April . . .	18·68	1·15	15·80	1·08	1·19	1·06	1·47	1·95	·67	·86	·74
November .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	11·70	10·08	17·23	17·70	17·47
December .	20·00	27·48	21·19	40·62	38·00	27·40	21·41	17·33	32·38	34·60	32·83
1852.	39·79	56·25	41·59	76·38	69·22	52·52	65·67	53·94	101·19	103·86	99·72
1851.	25·33	44·89	35·46	54·68	44·46	42·16	46·25	36·76	68·69	86·55	71·13

TABLE XIII.—TEMPERATURE at SEATHWAITE, BORROWDALE, 368 feet above the Sea-level.

1852.	Absolute.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Mean at 9 A.M.	On Grass.				Prevailing Winds.
	Maximum.	Minimum.					Absolute Minimum.	Mean.	Radiation. Maximum.	Radiation. Mean.	
			°	°	°	°	°	°		°	
January . .	49	29	43·48	37·67	40·57	40·25	19	32·75	11·0	4·92	SW.
February . .	50	26	42·29	35·46	38·87	37·84	14·5	30·39	11·5	5·07	W.
March . . .	60	28	46·71	36·13	41·57	38·82	18	28·77	13	7·66	SE.
April . . .	65	33	54·33	40·25	47·29	44·30	20	29·38	15	10·87	E. var.
May . . .	65	37	54·74	45·30	50·02	49·45	23	37·27	18	8·03	E. var.
June . . .	64·5	44	57·78	50·33	51·01	53·80	31	44·23	15	6·10	SW.
July . . .	77	50	69·17	58·35	63·76	63·32	39	51·96	14·5	6·39	S.
August . .	69	50	62·79	51·50	58·64	58·06	38	47·77	12·5	6·73	S.
September .	68	39·5	57·75	50·03	53·89	53·06	26	43·05	15	6·98	W. var.
October . .	57	31	48·32	41·43	44·87	43·61	24	35·12	10	6·31	NW. var.
November .	57	25	45·55	40·26	42·90	42·73	15·5	35·66	9·5	4·60	SW. var.
December .	54	31	47·30	42·11	44·70	44·54	27	38·38	9	3·73	SW.
1852. . . .	61·3	35·5	52·52	44·34	48·13	47·48	24·6	37·89	12·8	6·45	SW. var.
1851. . . .	60·4	33·8	51·41	42·83	47·13	46·60	23·9	36·80	13·4	6·03	N.W.
1850. . . .	59·6	30·3	51·05	42·23	46·64	46·02	...	...	...	...	NW. & SW.
1849. . . .	61·8	31·1	51·78	41·81	46·79	46·18	21·5	35·67	12·4	6·23	SW.
1848. . . .	62·4	30·5	52·15	42·06	47·10	46·76	20·5	35·18	12·9	6·91	SW.
1847. . . .	62·7	29·9	52·89	42·04	47·46	47·21	...	...	...	...	SW.
1846. . . .	63·	33·	53·77	44·05	48·91	48·13	...	...	...	...	...

TABLE XIV.—TEMPERATURE at WHITEHAVEN, on the West Coast, 90 feet above the Sea-level, and 17 miles distant in a direct line, bearing NNW. from the Hamlet of Seathwaite, Borrowdale.

1852.	Absolute.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Mean at 9 A.M.	Naked Thermometer on Grass Plot.				Prevailing Winds.
	Maximum.	Minimum.					Absolute Minimum on Raw Wool.	Mean Nocturnal Temperature.	Radiation. Maximum.	Radiation. Mean.	
	°	°	°	°	°		°	°	°	°	
January	50·	31·5	44·40	39·43	41·915		21·2	32·84	13·	6·59	SW.
February	51·	26·5	43·72	37·07	40·395		9·	28·47	17·5	8·60	SW.
March .	59·	25·5	48·00	36·95	42·475		8·8	26·25	19·	10·70	Easterly.
April .	67·	32·5	55·06	40·63	47·845		15·	26·83	19·	13·80	Easterly.
May . .	71·	36·	58·66	41·87	51·765		21·5	36·20	17·	8·67	NE.
June . .	73·	43·	63·70	51·13	57·415		32·5	42·88	16·	8·25	SW.
July . .	83·	52·	71·37	58·72	65·015		42·5	53·24	12·5	5·48	NE. & SW.
August .	71·	50·	67·25	54·93	61·090		36·	48·03	17·	6·90	SW.
September	73·	40·	61·38	50·70	56·040		23·5	42·15	16·5	8·55	NE.
October	61·	31·5	52·21	43·58	47·895		22·5	34·50	14·	9·08	SW.
November	58·5	26·5	47·76	42·02	44·890		16·	36·14	10·5	5·88	SW.
December	53·	32·	47·76	42·42	45·090		21·	36·43	11·5	5·99	SW.
1852.	61·2	35·8	55·10	45·20	50·155		22·4	36·99	15·3	8·20	SW.
1851.	62·3	35·1	53·86	44·39	49·129		23·3	36·86	16·3	7·53	SW.
1850.	63·1	33·5	51·13	44·07	49·101		20·8	36·26	15·2	7·80	SW.
1849.	62·3	33·7	53·21	44·15	48·696		18·8	35·05	18·4	9·09	SW.
1848.	62·9	32·6	53·77	43·79	48·785		20·2	35·73	15·9	8·06	SW.
1847.	62·3	33·7	53·85	43·50	48·679		20·5	35·95	15·1	7·45	SW.
1846.	61·8	36·1	55·95	45·75	50·858		23·1	38·30	14·6	7·45	SW.

TABLE XV.—MINIMUM TEMPERATURE of each MONTH on Sca Fell Pike and the Gabel, and at Sprinkling Tarn, from July 1851 to December 1852, inclusive.

1851.	Sprinkling Tarn.	Great Gabel.	Sca Fell.	Seathwaite.	CHARACTER OF THE MONTH.
July . .	30°	26°	26°	44°	Cold. On the morning of the 4th, ice was seen in some of the valleys.
August .	35	30	28	47·5	Temperature 0°·72 above the average.
September	31	25	23	39	A beautifully fine and dry month. 25th, first snow on the mountain tops.
October	28	23	22	34	Mild and wet. Some ice left in Sca Fell and Gabel gauges on 31st.
November	19	13	11	27	The coldest November on record, at Whitehaven.
December	23	21	20	26	A mild, but exceedingly dull, damp month. Sun shone out on 11 days only.
1852.					
January	21	18	17	29	An exceedingly wet month. Rain fell on 28 days.
February	18	11	9	26	Very wet till the 17th; afterwards, fine and frosty.
March .	21	15	12	28	A fine and remarkably dry month. Rain fell on 5 days only.
April .	28	23	22	33	A remarkably fine, mild, and dry month. Rain fell on 4 days only.
May . .	30	26	24	37	Cold and wet till the 19th; afterwards, fine and clear.
June . .	34	29	26	44	Rain fell on 25 days. Snow on Skiddaw on the 3d.
July . .	41	37	36	50	The hottest July on record at Whitehaven. Mean of minimum temperature at Seathwaite, 58°·35.
August .	44	40	37	50	Warm and wet.
September	34	28	26	39·5	Fine and dry.
October	27	22	21	34	1st, Snow on Skiddaw and Great End, first time this season.
November	21	18	17	25	Mild and wet. Hard frost in the valleys on the 29th and 30th.
December	21	21	19	31	By far the wettest December in the last 20 years, at Whitehaven. Rain fell on 30 days.
Means. .	28·0	23·6	22·0	35·8	

TABLE XVI.—HYGROMETRICAL OBSERVATIONS taken at the MOUNTAIN

DATE.	WASTDALE HEAD. (A)								BRANT RIGG. (B)			
	On leaving.				On returning.							
	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
1851.				h. m.				h. m.				h. m.
Dec. 30.	45°	41°3	37°2	9·0 a.m.	42°3	39°2	35°4	6·0 p.m.	37°9	37°2	36°2	4·0 p.m.
,, 31.	...	...	...	...	...	...	...	...	...	...	...	...
1852.												
Jan. 31.	39·9	38·2	36·1	8·30 a.m.	42·5	41·8	41·	4·30 p.m.	36·9	35·7	33·8	3·0 p.m.
March 1.	41·	36·5	31·2	7·10 a.m.	39·5	35·7	30·7	4·45 p.m.	36·9	33·2	27·6	2·30 p.m.
,, 31.	43·5	40·2	36·2	8·0 a.m.	46·5	42·	37·	4·0 p.m.	44·8	40·3	35·2	3·0 p.m.
April 30,	48·8	48·4	47·9	7·15 a.m.	49·8	46·9	44·	4·0 p.m.	41·	40·7	40·3	2·30 p.m
May 31.	47·3	42·4	37·	7·15 a.m.	48·	42·	35·4	7·45 p.m.	46·3	41·3	35·8	3·0 p.m.
July 1.	54·1	51·1	48·2	7·15 a.m.	56·7	54·6	53·1	7·0 p.m.	55·1	52·1	50·	2·30 p.m.
August 2.	53·1	49·9	46·7	5·45 a.m.	65·4	58·7	54·7	3·0 p.m.	64·9	58·7	54·9	0·0 p.m.
,, 31.	54·6	52·6	50·5	6·15 a.m.	48·8	44·8	40·4	5·0 p.m.	54·1	51·6	49·2	2·0 p.m.
Sept. 30.	45·	43·3	41·4	8·0 a.m.	...	...	...	...	45·	44·3	43·5	1·30 p.m.
Nov. 1.	52·6	50·1	47·6	8·15 a.m.	56·7	55·7	55·	4·30 p.m.	52·6	51·7	50·8	3·0 p.m.
,, 30.	31·3	29·5	25·2	8·30 a.m.	33·8	30·2	24·4	4·0 p.m.	33·3	31·2	27·7	2·30 p.m.
Dec. 31.	48·3	45·4	42·2	9·0 a.m.	47.3	46·4	45·1	3·30 p.m.	44·5	44·3	43·	2·0 p.m.
Means.	46·5	43·7	40·6	7·42 a.m.	48·1	44·8	41·3	4·50 p.m.	45·8	43·2	40·6	2·25 p.m.

DATE.	SPRINKLING TARN. (D)				GREAT GABEL. (E)				LINGMELL. (F)			
	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
1851.				h. m.				h. m.				h. m.
Dec. 30.	35°9	35°2	34°1	1·0 p.m.	30·6	30°5	29°7	3·0 p.m.	36°9	36°2	35°1	10·0 a.m.
,, 3.	...	...	...	...	31·8	31·2	29·8	1·30 p.m.	...	...	...	...
1852.												
Jan. 31.	30·8	30·7	30·3	0·30 a.m,	28·6	28·5	27·8	2·0 p.m.	32·3	31·7	30·7	10·0 a.m.
March 1.	31·	29·5	25·5	11·15 a.m.	29·	28·7	27·5	1·30 p.m.	31·	29·	23·7	9·0 a.m.
,, 31.	38·	36·	33·	0·30 p.m.	33·3	30·7	26·6	2·10 p.m.	41·	38·7	35·9	10·0 a.m.
April 30.	46·8	45·4	43·8	0·0 p.m.	41·	40·8	40·6	2·0 p.m.	48·3	47·4	46·4	9·0 a.m.
May 31.	38·9	36·2	32·1	0·15 p.m.	35·9	34·2	31·6	2·0 p.m.	42·5	39·2	35·2	9·15 a.m.
July 1.	46·8	44·3	41·5	10·30 a.m.	43·	42·3	41·4	1·45 p.m.	46·8	45·9	44·9	9·15 a.m.
August 2.	53·6	49·9	46·2	9·15 a.m.	49·3	48·4	47·4	10·50 a.m.	55·6	51·5	48·	7·15 a.m.
,, 31.	49·3	46·4	43·3	10·20 a.m.	43·5	43·3	43·1	0·45 p.m.	49·3	47·9	46·4	8·0 a.m.
Sept. 30.	41·	40·3	39·4	11·0 a.m.	39·2	38·9	38·7	0·30 p.m.	41·5	40·8	40·2	8·45 a.m.
Nov. 1.	46·3	45·9	45·5	11·0 a.m.	44·5	44·3	44·1	2·0 p.m.	46·3	45·4	44·4	9·45 a.m.
,, 30.	25·8	25·	20·9	0·0 p.m.	22·5	21·	9·8	1·30 p.m.	25·8	25·	21·	10·0 a.m.
Dec. 31.	40·5	40.3	40·	0·0 p.m.	38·9	38·3	37·3	1·30 p.m.	41·	40·3	39·4	10·0 a.m.
Means.	40·3	38·8	36·6	11·30 a.m.	36·5	35·8	34·0	1·37 p.m.	41·4	39·9	37·8	9·15 a.m.

STATIONS adjacent to the Vale of Wastdale, in the Year 1852.

STYE HEAD. (C)

Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
			h. m.
36°·9	36°·2	35°·1	1·30 p.m.
...	...	...	...
33·8	32·7	30·9	1·0 p.m.
32·8	31·7	29·6	11·45 a.m.
41·5	38·2	34·2	1·0 p.m.
48·3	47·4	46·4	0·45 p.m.
43·	38·2	32·4	1·0 p.m.
49·3	46·4	43·3	0·0 p.m.
54·6	50·9	47·5	9·45 a.m.
50·3	48·4	46·5	11·20 a.m.
43·	42·3	41·4	11·30 a.m.
48·8	48·4	47·9	1·0 p.m.
29·6	28·5	24·5	0·30 p.m.
43·	42·3	41·4	0·30 p.m.
42·7	40·9	38·5	0·16 p.m.

SCA FELL PIKE. (G)

Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
			h. m.
30·6	30°·5	29°·7	11·30 a.m.
29·6	29·5	28·4	11·0 a.m.
27·6	27·	24·2	11·0 a.m.
24·8	24·	19·7	10·20 a.m.
37·	36·2	35·	11·20 a.m.
41·5	40·3	38·8	10·20 a.m.
30·6	29·	24·5	10·30 a.m.
41·	40·8	40·8	10·30 a.m.
47·3	44·3	41·	8·15 a.m.
41·5	41·3	41·	9·15 a.m.
33·8	33·2	32·2	10·0 a.m.
42·	41·8	41·8	11·0 a.m.
20·5	19·	7·7	11·0 a.m.
36·4	36·2	35·9	11·0 a.m.
34·6	33·8	31·5	10·30 a.m.

STATE OF THE WEATHER AT THE DIFFERENT STATIONS.

1851.

Dec. 30. Damp mist at all the stations.

„ 31. The same.

1852.

Jan. 31. A, $8\frac{1}{2}^h$ A.M., fine morning; $4\frac{1}{2}^h$ P.M., rain; B, heavy rain; C, heavy rain, but below the mist. Misty, with snow or rain, at all the other stations.

March 1. A and B, fine and clear; C and D, large flakes of snow falling, no mist; E, misty, occasional gleams; F, clear and frosty; G, hard frost, some snow falling.

„ 31. Overcast, with an absence of mist at all the stations. Faint gleams at F, and bright sunshine at G.

April 30. Dense mist, with small misty rain, at most of the stations. Fair at C, F, G, and at A, on returning.

May 31. Overcast, no mist or sunhine.

July 1. A, at $7^h\ 15^m$, overcast, fair; at 7 P.M., fair; mist low down on the mountains. B, gleams. C and D, overcast, without mist. E, misty, with small misty rain. F, just entering the mist. G, very dense mist, with small misty rain.

August 2. A, cloudy. B, hot sunshine. C and D, neither mist nor sunshine. E, misty. F and G, fair and clear.

„ 31. A, at $6^h\ 15^m$, dark morning, with some rain; at 5^h P.M., fair and clear. B, fair, a heavy shower just ceased. C and D, fair, and clear of mist. E, very wet and misty. F, misty. G, dense mist.

Sept. 30. A, dark morning, with appearance of rain. B, C, and D, very wet, but below the mist. E, very wet and misty. F, rain began close to the under surface of the mist. G, very wet and misty.

Nov. 1. A, on leaving, overcast, but fair; on returning, very wet. B and C, very wet, but below the mist; dense mist with small rain at all the other stations.

„ 30. Hard frost, overcast, a transient gleam; no mist at any of the stations.

Dec. 31. A, on leaving, fair but misty; heavy rain on returning. B, wet, but now below the mist. C, misty, but nearly fair;—dense mist with rain at all the other stations. A complete hurricane on Sca Fell Pike. Gauges all free from ice.

Note.—The readings of the Dry and Wet Bulb Thermometers have all been reduced to a standard instrument, selected by Mr GLAISHER of the Royal Observatory.

TABLE XVII.—DEDUCTIONS relative to the HUMIDITY of the ATMOSPHERE at the MOUNTAIN STATIONS, in the year 1852.

STATION.	Dry Bulb.	Wet Bulb.	Dew Point.	WEIGHT OF VAPOUR.		Degree of Humidity, (complete Saturation 1·000.)
				In a cubic foot of Air.	Required for Saturation of a cubic ft. of Air.	
h. m.	°	°	°	Grains.	Grains.	
Wastdale Head, 7·42 a.m., 247 above the Sea,	46·5	43·7	40·6	3·12	0·70	0·818
Do. do. 4·50 p.m., „ „	48·1	44·8	41·3	3·17	·85	·788
Brant Rigg, . . 924 „ „	45·8	43·2	40·6	3·10	·64	·828
Stye Head, . . 1448 „ „	42·7	40·9	38·5	2·95	·43	·873
Lingmell, . . . 1778 „ „	41·4	39·9	37·8	2·90	·34	·896
Sprinkling Tarn, . 1900 „ „	40·3	38·8	36·6	2·79	·33	·894
Great Gabel, . . 2925 „ „	36·5	35·8	34·0	2·60	·16	·945
Sca Fell Pike, . . 3166 „ „	34·6	33·8	31·5	2·40	·18	·933

In addition to the above systematic readings, I find the following casual observations dispersed through the Registers:—

September 5, 1845.—The thermometer on the summit of Skiddaw, at noon, stood at 41°; sky overcast, sun gleaming out at intervals. Temperature of a strong spring, about 2 miles from the summit, also 41°. Air at foot of mountain, $3^h\ 30^m$ P.M., 58°.

May 6, 1847.—Temperature of air at foot of Sca Fell, at $10^h\ 40^m$ A.M., 52°; on the summit of the Pike, at 1 P.M., 37°, and intensely cold. Extensive drifts of snow on the east side of the mountain. Temperature of a spring near Sprinkling Tarn, 37°. Between 4^h and $5^h\ 30^m$, P.M., whilst passing over Stye Head in the direction of Borrowdale, there occurred one of the most dreadful storms of thunder and lightning which it has been my lot to witness. The electric discharges were frequent and extremely dazzling, and many of them followed, almost instantaneously, by deafening peals of thunder reverberating from hill to hill. A large quantity of hail fell, (unaccompanied by rain,) quite sufficient to give me a thorough drenching. The storm was confined to the mountains, and the hail did not reach the valleys. I made two attempts to ascend the Gabel, and on both occasions was obliged to retreat before the fury of the elements.

September 9, 1847.—Ascended Snowdon in Wales, and found the temperature at the Farm-House on the Beddgelert side, to be 56°·7; at the summit, 46°·8, a difference of only 9°·9 in 3571 feet, or a descent of 1° in every 357 feet of elevation. We ascended Snowdon through an exceedingly dense mist, which enveloped the mountain nearly to its base. The fine white vesicles composing the cloud settled upon our garments, and long before arriving at the summit, they appeared as if covered with minute particles of snow. I am inclined to think that the latent heat evolved by the vapour during its conversion into mist (Cirrostratus) tended to equalize the temperature between the top and bottom of the mountain, and that a much greater difference would be found in a clear, or even a moderately clear atmosphere.

TABLE XVIII.—HYGROMETRICAL OBSERVATIONS, taken April 22, 1848.

STATION.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Degree of Humidity, (complete Saturation 1·000).	STATE OF THE WEATHER.
	h. m.	°	°	°		
Wastdale Head, .	11·15 A.M.	54·5	49·0	44·4	0·708	Sky overcast, strong breeze.
Brant Rigg, . .	1·45 P.M.	46	44	41·8	·864	Mist extending below Sprinkling Tarn.
Stye Head, . . .	2·45 P.M.	44	42	39·6	·858	
Sprinkling Tarn, .	4·0 P.M.	41·5	40·5	39·3	·928	Enveloped in dense mist.
Great Gabel, . .	6·0 P.M.	40	39	37·8	·926	A gale with heavy rain, partially frozen.

April 24, 1848.—*Seatollar Common.* Temperature at Seathwaite, at 11 A.M., 46°; Wet Bulb, 41°; Dew Point, 35°·5; Humidity, 0·694; at summit of Common, air temperature, 43°; Wet Bulb, 39°; Dew Point, 34°·2; and Humidity, 0·736.

TABLE XIX.—HYGROMETRICAL OBSERVATIONS taken on SCA FELL, April 21, and July 17, 1848.

1848.	STATION.	Hour.	Dry Bulb.	Wet Buld.	Dew Point.	Degree of Humidity, (complete Saturation 1·000).	STATE OF THE WEATHER.
		h. m.	°	°	°		
April 21,	Wastdale Head,	11·40 A.M.	58	52	47·8	0·708	A strong breeze prevailed in the valley throughout the afternoon.
„	Top of the Pike,	4·30 P.M.	50	45	40	·708	Calm, overcast, followed by rain. The clouds (evidently electric) are generally below the summit.
July 17,	Foot of Mountain,	11·45 A.M.	60	54	49·8	·710	Light breeze, overcast.
„	Top of Lingmell,	2·10 P.M.	48	47·5	47	·965	Fresh breeze, evercast.
„	Spring on do.,	„	47	...	...	...	
„	Top of the Pike,	3·30 P.M.	44·2	44·2	44·2	1·000	Fresh breeze; enveloped in mist, air saturated.

July 16, 1848.—*Stye Head.* Temperature near Valley, 68°; Wet Bulb, 61°; Dew Point, 56°·8; Humidity, 0·691; 5 P.M., temperature on Stye Head, 60°; Wet Bulb, 56°·5; Dew Point, 54°; Humidity, 0·820.

TABLE XX.—HYGROMETRICAL OBSERVATIONS, taken July 1st and 2d, 1851.

1851.	STATION.	HOUR.	Dry Bulb.	Wet Bulb.	Dew Point.	Degree of Humidity, (complete Saturation 1·000).	STATE OF THE WEATHER.
		h. m.	°	°	°		
July 1, .	Wastdale Head,	7·0 P.M.	69·1	64·4	61·6	0·783	Very fine and sultry, Cumulus; 5 to 7 P.M., heavy rain with much thunder and lightning at Wastdale Head; afterwards, fair but cloudy.
,,	Stye Head, . .	9·15 P.M.	61·8	57·7	54·8	·793	
,,	Stye Head Tarn,	9·30 P.M.	63·9	57·7	53·4	·703*	Thermometer at Seathwaite rose to 80°.
July 2, .	Seathwaite, . .	1·30 P.M.	70	61·9	57·9	·669	Very fine and sultry, mostly clear, light breeze; evening, cloudy on mountain tops.
,,	Seatollar Gauge,	3·0 P.M.	63·9	56·7	51·7	·664	
,,	Top of Seatollar,	3·30 P.M.	62·9	56·7	52·4	·703	

* The Humidity at Stye Head Tarn is apparently too low; it is probable the Dry Bulb Thermometer was incorrectly read off, in the dusk of the evening.

TABLE XXI.—HYGROMETRICAL OBSERVATIONS, taken December 15th, 16th, and 17th, 1850.

1850.	STATION.	Hour.	Dry Bulb	Wet Bulb.	Dew Point	Degree of Humidity, (complete Saturation 1·000).	STATE OF THE WEATHER.
		h. m.	°	°	°		
Dec. 15,	Foot of Seatollar Common, .	2·45 P.M.	42	38·7	34·7	0·779	
,,	Gauge on do., .	3·30 P.M.	37	34·7	31·2	·816	1388 feet above Sea.
,, 16,	Seathwaite, . .	11·30 A.M.	43	39·2	34·6	0·748	
,,	Gauge on "Stye,"	0·0 P.M.	40	37·2	33·8	·806	948 feet above Sea.
,,	Stye Head Tarn,	1·30 P.M.	38	36·7	34·7	·894	1290 feet above Sea.
,, 17,	Gauge on Lingmell	11·30 A.M.	32·8	32·2	31·2	0·943	Heavy rain.
,,	Wastdale Head,	1·0 P.M.	40	39	37·8	·926	Heavy rain throughout the day; air completely saturated at noon; dry and wet, both 40°.
,,	Do. do., .	4·30 P.M.	40	38·2	36	·871	Humidity rapidly decreasing.
,,	Brant Rigg, . .	5·37 P.M.	37	35·2	32·5	·854	Fine starlight evening.
,,	Gauge, Stye Head,	6·40 P.M.	35·8	34·2	31·8	·868	The same.

TABLE XXII.—SYNOPSIS of the FALL of RAIN in the ENGLISH LAKE DISTRICT, in the Year 1853.

No.	I.	III.	IV.	VII.	IX.	X.	XI.	XIII.	XIV.			XV.	XVI.	XVIII.	XIX.	XX.	PREVAILING WINDS.		
	WHITEHAVEN.									WESTMORELAND.				BORROWDALE.					
														Seathwaite.					
1853.	High Street, 90 feet above the Sea.	St James's Church Steeple, 78 feet above the Street.	The Flosh, 3 miles south of Whitehaven.	Keswick, 258 feet above the Sea.	Loweswater, 336 feet above the Sea.	Foot of Crummock Lake, 260 feet above the Sea.	Gatesgarth, 290 feet above the Sea.	Eskdale Head, height unknown.	Wastdale Head, 247 feet above the Sea.	Kendal, 126 feet above the Sea.	Selside, 6 miles N. of Kendal, 736 feet above the Sea.	The How, Troutbeck, 503 feet above the Sea.	Ambleside, 190 ? feet above the Sea.	At 10 inches above the surface.	At 22 inches above the surface.	Stonethwaite, 340 feet above the Sea.	At Crummock Lake, two observations daily.	At Seathwaite, one observation daily.	At the Coast, two observations daily.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.			
Jan.	5·886	3·362	6·00	11·728	13·225	15·84	21·16	10·43	13·71	6·696	10·222	17·778	13·25	23·12	22·32	23·99	SW.	SW.	SW.
Feb.	2·244	1·553	2·33	1·604	3·515	2·39	2·79	2·75	3·61	1·077	1·836	2·231	1·37	3·84	3·46	2·95	NE.	NE.	NE.
March	1·204	·888	1·48	1·236	1·975	2·52	3·73	3·18	3·57	2·143	2·473	3·086	3·77	4·59	4·43	3·45	E.	SE.	Easterly.
April	2·478	1·850	2·91	4·620	4·585	6·97	10·29	6·93	7·96	3·081	5·454	5·351	6·05	12·67	12·75	10·38	W.	NW.	Westerly.
May	·272	·080	·33	1·160	·790	·83	·85	·68	·66	·804	·339	·855	·72	·89	0·86	·69	SE.	SE.	NE.
June	3·145	2·616	3·54	1·710	2·780	2·80	3·86	2·91	3·92	3·084	5·125	3·260	3·09	4·07	4·13	3·74	SW.	W.	SW.
July	4·927	3·330	6·68	6·646	8·365	9·93	16·13	8·93	15·97	6·829	11·082	8·157	7·84	19·67	19·37	13·68	SW.	SW.	SW.
August	3·119	2·392	4·13	4·070	4·835	6·05	7·75	7·09	8·00	2·680	4·560	4·455	4·08	10·47	10·45	7·81	NW.	NW.	Westerly.
Sept.	2·733	1·818	3·27	4·170	4·935	5·80	8·02	5·62	8·07	3·524	4·974	5·538	6·54	10·42	10·32	7·33	SW.	NW.	SW.
October	6·591	4·058	5·14	7·492	8·105	9·43	13·18	8·36	9·11	5·004	6·413	8·038	9·04	13·25	13·29	12·56	SW. var.	SW. var.	SW.
Nov.	4·301	2·953	4·99	5·520	5·840	8·48	9·68	5·01	7·56	3·998	5·166	6·571	9·59	9·47	9·07	8·12	SW.	SW.	SW.
Dec.	·504	·284	·58	·490	·660	1·05	1·66	1·15	1·25	·535	·340	·650	·93	1·23	1·16	·95	E.	NE. & SE.	Easterly.
1853.	37·404	25·184	41·38	50·446	59·610	72·09	99·10	63·07	83·39	39·455	57·984	65·970	66·27	113·69	111·61	95·65	SW.	SW. var.	SW.
1852.	50·032	35·957	55·18	79·974	80·529	103·40	134·88	88·85	109·58	65·354	92·485	115·364	104·33	156·74	150·88	133·86	SW. var.	SW. var.	SW.
1851.	43·120	32·110	51·66	62·327	67·100	84·08	107·00	78·58	97·94	47·351	70·581	80·760	84·22	139·60	135·86	114·12	NW.	NW.	SW.
1850.	40·473	28·636	51·28	59·528	68·798	85·66	108·84	81·69	108·76	49·576	76·391	79·808	79·85	143·96	...	105·81	SW. & NW.	NW. & SW.	SW.
1849.	38·999	28·210	45·13	48·800	55·280	70·21	97·09	71·22	107·22	48·068	70·387	75·420	65·32	125·47	121·57	94·27	SW.	SW.	SW.
1848.	47·342	36·344	60·82	66·407	76·668	98·07	133·55	86·78	115·32	56·314		91·347	77·58	160·89	157·22	130·24	SW.	SW.	SW.
1847.	42·921	30.713	47·80	58·286	66·296	82·32	106·25	74·93	96·34	52·197		78·004		129·24	126·80	106·21	W.	SW.	SW.
1846.	49·134	35·442	55·16	67·678	79·249	96·47	121·90		106·93	52·365		77·719		143·51			SW.	SW.	SW.
1845.	49·207	33·489	53·00	62·202	69·542	87·48	124·13		108·55	53·346		76·309		151·87			SW. var.		SW. var.
1844.	36·723	27·862	39·31	40·629	49·829	61·46			80·01	43·102		51·986							
Means.	43·535	31·392	50·07	59·627	67·290	84·12	114·75	77·87	101·40	50·730	73·553	79·295	79·59	140·55 {137·60 for 6 yrs.}	133·99	111·45			

TABLE XXIII.—WET DAYS.

1853.	Whitehaven.	The Flosh.	Keswick.	Loweswater Lake.	Crummock Lake.	Wastdale Head.	Kendal.	Selside.	Troutbeck.	Ambleside.	Seathwaite.	Stonethwaite.
January	20	22	19	22	21	22	22	20	22	20	21	21
February	17	13	12	15	11	13	5	4	8	6	13	10
March .	14	17	12	14	15	17	13	10	16	9	18	17
April .	18	19	16	18	18	20	17	12	20	15	21	19
May . .	5	6	6	7	6	4	5	1	5	4	11	8
June . .	16	17	11	15	13	16	11	12	12	12	16	16
July . .	21	22	21	19	19	23	21	18	22	19	25	22
August .	13	14	10	12	12	14	9	10	10	9	15	11
September	15	18	14	18	16	19	12	12	16	14	20	16
October	21	22	26	21	22	25	18	15	20	18	25	21
November	17	18	14	17	18	18	13	8	13	15	18	17
December	14	14	11	11	12	12	4	3	9	8	14	13
1853.	194	202	172	189	183	203	150	125	173	149	217	191
1852.	190	191	201	201	191	212	191	156	202	201	221	207
1851.	196	201	211	202	206	232	173		195	178	219	206
1850.	189	201	209	198	201	218	168		185	182	223	199
1849.	189	185	205	191	185	236	161		186	159	193	183
1848.	210	207	229	217	207	213	190		201		232	224
1847.	191	183	201	190	199	226	174		188		202	195
1846.	200	208	213	198	216	231	191		194		219	
1845.	193	175	195	195	202	211	178		180		211	
Mean Number.	195	195	201	198	199	227	176		189	174	215	201

TABLE XXIV.—Shewing the QUANTITY of RAIN received by the MOUNTAIN GAUGES, in the Year 1853.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
	SCA FELL.						THE VALLEY.		BORROWDALE.		
1853.	The Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.	Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Iligg, 924 feet above the Sea.	To the West, Wastdale, 247 feet above Sea.	To the SE., Eskdale, height unknown.	Seatollar Common, 1338 feet above the Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 feet above Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	13·71	10·43	23·02	24·20	23·12
February . .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	3·01	2·75	Fr.	Fr.	3·84
March . . .	Fr.	17·70	Fr. 1·34	Fr. 14·00	23·41	18·00	3·57	3·18	6·54	8·37	4·59
April . . .	Fr. 6·05	4·30	Fr. 6·84	14·44	6·32	6·00	7·96	6·93	12·73	13·86	12·67
May . . .	7·75	·10	6·46	·30	·34	·73	·66	·68	·46	1·19	·89
June . . .	3·44	3·71	4·00	5·35	4·59	3·52	3·92	2·94	5·06	4·98	4·07
July . . .	15·88	15·70	16·00	21·10	20·30	14·71	15·97	8·93	18·73	23·05	19·67
August . . .	7·20	8·00	7·70	10·35		8·70	8·00	7·09	10·58	11·77	10·47
September . .	5·19	6·00	6·46	8·47		6·42	8·07	5·62	10·21	11·55	10·42
October . . .	6·61	9·25	7·11	14·00	37·69	7·61	9·11	8·36	12·76	14·60	13·25
November . .	3·20	5·75	3·40	6·11	*	6·68	7·56	5·01	10·14	10·05	9·47
December . .	·85	·55	·41	·47		1·03	1·25	1·15	1·22	1·29	1·23
1853.	56·17	71·06	59·72	94·59	92·65	73·40	83·39	63·07	111·45	124·91	113·69
1852.	81·30	100·22	85·75	134·79	124·19	98·24	109·58	88·85	156·59	167·73	156·74
1851.	71·29	96·29	81·23	125·29	100·16	89·51	97·94	78·58	141·42	169·62	139·60
†1850.	80·31	92·50	87·28	127·80	115·53	91·10	101·50	76·01	138·84	174·33	136·62
‡1849.	83·21	91·90	84·92	121·10	105·15	87·30	114·48	76·90	108·97		128·03
§1848.	94·73		91·32	148·59	138·72	109·19	127·47	95·71	139·48		177·55
‖1847. 1846.	128·15		136·98	207·91	185·74		170·55		180·23		223·64

* The Stye Gauge was injured in August, and it was not subsequently adjusted. The depth of rain, from August to December inclusive, is calculated by differentiation with Sprinkling Tarn in the corresponding months of 1852.

† For 11 months. ‡ and § For 13 months each.

‖ For 21 months, from March 1846 to November 1847 inclusive.

January 31. The Wastdale Mountain Gauges were not read off, on account of the illness of the Registrar.

February 28. The Mountain Gauges were all frozen up, and covered with snow.

April 2. The Gabel and Sprinkling Tarn Gauges were partially frozen. Sca Fell Pike could not be found, on account of the dense mist.

May 2. Some ice left in Sca Fell and Gabel Gauges.

November 30. A little ice left in Sca Fell receiver.

December 31. The Mountain Gauges (all frozen) were brought down to the valley, and the experiments closed.

Table XXV.—For the Summer Months.

No.	XXI.	XXI.[2]	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XXVII.	XIX.
1853.	Sca Fell.						The Valley.		Borrowdale.		
	Pike, 3166 feet above the Sea.	Lingmell, 1778 feet above the Sea.	Great Gabel, 2925 feet above the Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1448 feet above the Sea.	Brant Rigg, 924 feet above the Sea.	To the West, Wastdale, 247 feet above Sea.	To the SE., Eskdale, height unknown.	Seatollar Common, 1338 feet above the Sea.	The Stye, 948 feet above the Sea.	The Valley, Seathwaite, 368 feet above Sea.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
May	·40	·10	·46	·30	·34	·73	·66	·68	·46	1·19	·89
June	3·44	3·71	4·00	5·35	4·59	3·52	3·92	2·94	5·06	4·98	4·07
July	15·88	15·70	16·00	21·10	20·30	14·71	15·97	8·93	18·73	23·05	19·67
August . . .	7·20	8·00	7·70	10·35	}	{ 8·70	8·00	7·09	10·58	11·77	10·47
September . .	5·19	6·00	6·46	8·47	31·54	6·42	8·07	5·62	10·21	11·55	10·42
October . . .	6·61	9·25	7·11	14·00	}	{ 7·61	9·11	8·36	12·76	14·60	13·25
1853.	38·72	42·76	41·73	59·57	56·77	41·69	45·73	33·62	57·80	67·14	58·77
1852.	41·51	43·97	44·16	58·41	51·97	45·72	43·91	31·91	55·40	63·87	57·02
1851.	45 96	51·40	45·77	70·61	55·70	47·35	51·69	41·82	72·73	83·07	68·47
1850.	44·37	45·82	45·29	63·97	59·63	43·83	47·31	35·36	62·62	77·37	60·18
1849.	46·68	45·90	46·47	58·53	51·29	40·63	49·89	34·98	*50·01	...	55·73
1848.	49·46	...	46·81	70·95	60·35	43·18	50·16	37·69	57·97	...	68·96
1847.	43·98	...	43·30	62·30	56·98	43·93	48·98	38·95	56·80	...	62·95
1846.	45·45	...	52·39	70·83	58·12	...	54·30	...	56·12	...	72·23

* On the 31st August 1839, the Seatollar Gauge was removed 90 yards to the South Westward, and about 5 feet lower down the Mountain.

Table XXVI.—For the Winter Months.

1853.	Sca Fell.		Great Gabel.	Sprinkling Tarn.	Stye Head.	Brant Rigg.	The Valley.		Borrowdale.		
	The Pike.	Lingmell.					To the W., Wastdale.	To the SE., Eskdale.	Seatollar Common.	On the Stye.	The Valley, Seathwaite.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . . .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	13·71	10·43	23·02	24·20	23·12
February . .	Fr.	Fr.	Fr.	Fr.	Fr.	Fr.	3·61	2·75	Fr.	Fr.	3·84
March . . .	Fr.	17·70	Fr.	14·00	23·41	18·00	3·57	3·18	6·54	8·37	4·59
April	13·40	4·30	14·18	14·44	6·32	6·00	7·96	6·93	12·73	13·86	12·67
November . .	3·20	5·75	3·40	6·11	} 6·15	{ 6·68	7·56	5·01	10·14	10·05	9·47
December . .	·85	·55	·41	·47	}	{ 1·03	1·25	1·15	1·22	1·29	1·23
1853.	17.45	28·30	17·99	35·02	35·88	31·71	37·66	29·45	53·65	57·77	54·92
1852.	39·79	56·25	41·59	76·38	69·22	52·52	65·67	53·94	101·19	103·86	99·72
1851.	25·33	44·89	35·46	54·68	44·46	42·16	46·25	36·76	68·69	86·55	71·13
1850.	35·94	46·68	41·99	63·83	55·90	47·27	54·19	40·65	76·22	96·96	76·44
1849.	36·53	46·00	38·45	62·57	50·86	46·67	64·59	41·92	58·93	...	72·30
1848.	45·27	...	44·51	77·64	78·37	66·01	77·31	58·02	81·51	...	108·59
*{ 1847. 1846. }	38·72	...	41·29	74·78	70·34	{ 42·72 For 7 Months }	67·27	{ 40·29 For 7 Months }	67·31	...	88·46

* For Nine Months only.

TABLE XXVII.—MONTHLY EVAPORATION at WHITEHAVEN, CUMBERLAND, in each of the Twelve Years ending with 1853.

Months.	1842.*	1843.†	1844.	1845.	1846.‡	1847.	1848.	1849.	1850.	1851.	1852.	1853.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January .	0·890	·785	0·940	0·935	1·015	0·860	0·743	0·909	0·693	0·991	1·284	1·197
February .	1·376	1·178	1·190	·905	1·335	·843	·792	1·024	·823	·929	1·194	1·083
March . .	2·542	1·620	1·835	1·862	2·085	1·821	1·397	1·558	1·690	1·544	2·197	1·749
April . .	4·187	1·718	2·610	3·400	2·575	2·181	2·728	2·620	2·295	2·481	3·940	2·239
May . .	4·552	3·045	6·280	3·645	4·375	2·950	4·580	3·886	3·505	3·308	3·570	4·999
June . .	5·349	4·690	3·820	3·760	6·645	4·506	3·749	5·076	4·290	3·658	3·172	3·845
July . .	4·888	3·125	4·495	5·455	3·450	4·726	3·935	4·156	4·278	3·415	4·672	2·696
August . .	2·900	3·305	2·520	3·250	3·875	3·751	3·686	2·657	3·381	3·063	3·551	3·126
September .	4·342	3·745	3·405	3·225	2·980	2·793	2·896	3·337	2·664	2·631	3·041	2·628
October .	2·814	1·940	2·270	2·360	1·780	1·688	1·549	1·723	1·558	1·554	1·510	1·607
November	1·625	1·030	1·554	1·760	1·360	1·107	1·129	·960	1·181	1·029	1·143	1·240
December	1·371	·800	·800	1·875	1·025	1·005	1·019	·793	·991	·737	1·074	·925
Evaporation.	36·836	26·981	31·719	32·432	32·500	28·231	28·203	28·699	27·349	25·340	30·348	27·334
Rain at Whitehaven.	34·639	46·206	36·723	49·207	49·134	42·921	47·342	38·999	40·473	43·120	50·030	37·404
Rain at Seathwaite.	…	…	…	151·87	143·51	129·24	160·89	125·47	143·96	139·60	156·74	113·690

* The Evaporation for 1842 is computed from the evaporation force, in proportion to the respective values of the estimated and measured evaporation in the corresponding months of the years 1843 and 1844. The result, calculated for the mean of the *whole* period (1843–1846) during which the evaporating force and the spontaneous evaporation were registered daily together, is 39·870 inches:—

	Grains.		Inches.	::	Grains.		Inches.
Thus, if	19·99	:	30·908	::	25·8	:	39·87

† The evaporation in 1843 is somewhat too small in amount, from the gauge not being sufficiently exposed to wind and sun during the first half of the year.

‡ Till the close of 1846, the dish was placed on a stool 8 inches above the ground; since that period, it has been placed on a stand 4 feet 4 inches in height, and just large enough to hold it.

TABLE XXVIII.—TEMPERATURE at WHITEHAVEN, and at SEATHWAITE, BORROWDALE, in the year 1853.

1853.	SEATHWAITE, BORROWDALE.							WHITEHAVEN.					
	ABSOLUTE.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Mean at 9 A.M.	Prevailing Winds.	ABSOLUTE.		Mean of Maximum.	Mean of Minimum.	Approximate Mean Temperature.	Prevailing Winds.
	Maximum.	Minimum.						Maximum.	Minimum.				
January .	51°	27°	42·05	36·79	39·42	39·22	SW.	51°	27·5	44·30	37·91	41·10	SW.
February .	41	20	35·71	26·60	31·15	31·42	NE.	43	20	38·09	30·50	34·29	NE.
March . .	50·5	22	41·64	32·48	37·06	36·17	SE.	53·5	22·5	44·55	34·10	39·32	E.
April . .	53·5	33	48·03	39·81	43·02	42·65	NW.	57	33·5	51·20	40·93	46·06	W.
May . .	69·5	36	58·92	45·14	52·03	50·08	SE.	75	35	61·03	44·04	52·53	NE.
June . .	73	47	63·21	52·46	57·83	57·41	W.	75·5	44	64·33	50·96	57·64	SW.
July . .	66	50	59·40	53·59	56·49	56·43	SW.	70	50·5	63·82	54·01	58·91	SW.
August .	68·5	47·5	60·51	52·87	56·69	56·12	NW.	70·5	46·5	64·10	53·74	58·92	W.
September	64	42·5	56·53	49·10	52·81	51·76	NW.	68	42	60·82	50·51	55·66	SW.
October .	58	35	51·95	45·80	48·87	48·25	SW. var.	59·5	37·5	55·69	47·73	51·71	SW.
November .	55	31	46·05	39·85	42·95	42·60	SW.	56·5	31	47·70	41·65	44·67	SW.
December .	51	19	38·05	31·22	34·63	33·93	NE. & SE.	52	23	39·72	33·37	36·54	E.
1853.	58·4	34·2	50·17	42·14	46·15	45·50	SW. var.	60·9	34·4	52·94	43·28	48·11	SW.
Average of 8 Years, 1846–1852.	61·2	32·3	51·97	42·69	47·32	46·73	SW.	62·8	34·3	54·10	44·26	49·18	SW.

TABLE XXIX.—MINIMUM TEMPERATURE of each MONTH on Sca Fell Pike and the Gabel, and at Sprinkling Tarn, in the Year 1853.

1853.	Sprinkling Tarn.	Great Gabel.	Sca Fell.	Seathwaite.	CHARACTER OF THE MONTH.
January .	21°	10°	10°	27°	A mild, wet month. Minima taken February 16.
February .	20	10	8	20	A dry, frosty month; frequent snow showers.
March . .	23	13	11	22	A fine, dry, cold month; frequent snow showers.
April . .	25	19	11	33	A fine seasonable month.
May . . .	26	21	20	36	A very fine and exceedingly dry month. On the 9th, the valleys were covered with snow.
June . . .	41	36	35	47	A fine, but rather cold month.
July . .	41	38	37	50	Cold and damp.
August . . .	39	34	35	47·5	Fine and dry.
September	32	30	29	42·5	Do. do.
October .	30	27·5	27	35	Mild and wet. On the 2d, Great End was covered with snow.
November	27	23·5	23	31	A mild month. Snow on Fells.
December	16	13	12	19	A very dry, frosty month. Frequent snow showers in the valleys.
1853.	28·4	22·9	21·5	34·2	
1852.	28·0	23·6	22·0	35·8	

February.—At the middle and end of this month, the depth of snow covering the Sprinkling Tarn thermometer was estimated at 2 feet. On Sca Fell and Gabel, the depth was probably about 18 inches.

March.—The high reading of the thermometer at Sprinkling Tarn in the first three months is owing to the greater depth of snow covering the instrument. The thermometer was found to be in good working order.

December 31.—The thermometer box at Sprinkling Tarn was under 18 inches, and those on Gabel and Sca Fell under about 15 inches of snow.

TABLE XXX.—MONTHLY HYGROMETRICAL OBSERVATIONS taken at the MOUNTAIN

DATE.	WASTDALE HEAD. (A)								BRANT RIGG. (B)			
1853.	On leaving.				On returning.				Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.				
				h. m.				h. m.				h. m.
Feb. 16.	29°	26°5	16°5	8·15 a.m.	33°8	29°6	22°7	3·30 p.m.	26°6	24°6	14°5	10·0 a.m.
„ 28.	28·6	27·	20·4	8·45 a.m.	35·8	33·2	29·3	5·45 p.m.	33·8	31·5	27·8	3·15 p.m.
April 2.	42·5	40·3	37·6	8·0 a.m.	47·3	44·3	41·	4·45 p.m.	38·9	38·2	37·1	9·45 a.m.
May 1.	59·7	50·5	44·	1·30 p.m.	58·2	49·4	43·3	7·30 p.m.	54·6	47·4	41·6	3·15 p.m.
„ 31.	47·3	44·3	41·	6·0 a.m.	66·9	57·7	52·1	3·0 p.m.	65·9	56·7	51·1	1·0 p.m.
June 30.	58·7	53·6	50·	4·0 p.m.	54·6	40·4	45·2	9·45 p.m.	49·3	46·4	43·3	9·0 p.m.
August 2.	58·7	55·6	53·4	8·45 a.m.	60·7	55·6	52·	6·0 p.m.	57·7	53·6	50·7	4·0 p.m.
„ 31.	55·1	52·1	49·9	3·30 p.m.	50·8	48·4	46·	8·15 p.m.	49·3	48·4	47·4	7·40 p.m.
Sept. 30.	48·8	45·8	42·5	8·0 a.m.	51·6	46·9	42·2	4·0 p.m.	48·3	45·4	42·2	2·30 p.m.
Oct. 31.	54·3	49·4	45·	9·45 a.m.	55·6	50·4	46·8	5·30 p.m.	51·4	47·4	43·4	4·0 p.m.
Nov. 30.	45·	43·3	41·4	8·30 a.m.	43·5	41·3	38·6	4·0 p.m.	41	39·2	37·	2·45 p.m.
Dec. 31.	30·6	27·5	18·	9·30 a.m.	27·5	25·5	15·8	4·45 p.m.	28·	25·1	11·6	10·0 a.m.
1853.	46·5	43·0	38·3	10·2 a.m.	48·8	44·3	39·6	5·34 p.m.	45·4	42·0	37·3	2·36 p.m.
1852.	46·5	43·7	40·6	7·42 a.m.	48·1	44·8	41·3	4·50 p.m.	45·8	43·2	40·6	2·25 p.m.

1853.	SPRINKLING TARN. (D)				GREAT GABEL. (E)				LINGMELL. (F)			
	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	Dry Bulb.	Wet Bulb.	Dew Point.	Hour.
				h. m.				h. m.				h. m.
Feb. 16.	23°5	22°5	15°9	11·0 p.m.	19°3	19°5	19°3	1·0 p.m.	24°6	24°5	23°8	10·15 a.m.
„ 28.	25·6	25·2	24·2	0·45 p.m.	23·5	23·5	23·5	2·30 p.m.	26·	25·	19·9	10·15 a.m.
April 2.	36·9	36·2	35·1	0·15 p.m.	30·3	30·2	29·2	11·15 a.m.	37·9	37·2	35·9	3·40 p.m.
May 1.	50·9	44·5	38·1	4·0 p.m.	46·	40·3	34·	5.30 p.m.	52·	46·	40·	8·0 a.m.
„ 31.	54·6	49·4	45·2	10·45 p.m.	56·1	50·4	46·5	0·15 p.m.	54·6	49·4	45·2	8·15 a.m.
June 30.	45·	43·3	41·4	7·0 p.m.	50·5	39·7	38·7	8·20 p.m.	47·	45·	42·8	5·20 p.m.
August 2.	52·6	49·4	46·2	1·0 p.m.	50·3	48·4	46·5	3·0 p.m.	57·7	54·6	52·4	10·0 a.m.
„ 31.	46·3	45·4	44·4	6·45 p.m.	42·	39·8	37·1	6·30 p.m.	48·3	46·4	44·3	4·45 p.m.
Sept. 30.	41·	39·2	36·2	11·30 a.m.	38·6	37·2	35·1	1.30 p.m.	41·	39·7	35·1	9·30 a.m.
Oct. 31.	43·	41·8	40·3	2·0 p.m.	25·	43·8	39·1	3.15 p.m.	47·3	44·3	41·	11.45 a.m.
Nov. 30.	36·9	35·7	33·8	0·30 p.m.	33·3	33·2	32·9	2·1 p.m.	38·	37·2	36·	10·15 a.m.
Dec. 31.	23·	21·4	9·4	1·0 p.m.	17·	16·1	9·3	3·0 p.m.	20·5	19·6	12·8	10·15 a.m.
1853.	39·9	37·8	34·2	1·42 p.m.	36·8	35·2	32·6	3·0 p.m.	41·2	39·1	35·8	11·31 a.m.
1852.	40·3	38·8	36·6	11·30 a.m.	36·5	35·8	34·0	1·37 p.m.	41·4	39·9	37·8	9·15 a.m.

STATIONS adjacent to the Vale of Wastdale, for the Year 1853.

STYE HEAD. ()				STATE OF THE WEATHER AT THE DIFFERENT STATIONS.
Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	
			h. m.	1853.
25°·8	24°·5	17°·8	11.30 a.m.	Feb. 16. Dense mist on Sca Fell; no mist or sun at any of the other stations, except at Wastdale Head on returning, when the sun was shining pretty brightly. Mountain tops thickly covered with snow.
31·8	30·	26·1	1·20 p.m.	
37·9	37·2	35·9	0·0 p.m.	,, 28. A, fine. Dense mist on Gabel; light do. on the Pike; fine and gleamy at the other stations. Mountain tops thickly covered with snow.
53·6	45·9	38·2	4·30 p.m.	
60·7	52·6	46·9	11·15 a.m.	
48·3	45·	41·4	7·30 p.m.	
55·6	51·6	48·8	1·30 p.m.	
47·3	46·4	45.4	7·0 p.m.	
43·	41·3	38·	0·0 p.m.	April 2. A, *on leaving*, dark and misty; *on returning*, fair and mild. B, wet, but below the mist. C, fair, below the mist. D, misty, with clear intervals. E and F, thick mist, but fair.
45·	43·3	40·2	2·30 p.m.	
41·	38·7	35·9	1·0 p.m.	
26·	23·6	11·6	1·30 p.m.	
43·0	40·0	35·5	2·8 p.m.	May 1. Fine and sunny; no mist at any of the stations.
42·7	40·9	38·5	0·16 p.m.	,, 31. Fine and clear throughout.
SCA FELL PIKE. (G)				June 30. Misty at intervals on Gabel and the Pike; clear at the other stations.
Dry Bulb.	Wet Bulb.	Dew Point.	Hour.	
			h. m.	August 2. Fine and gleamy; no mist at any of the stations.
19°·	19°·1	19°·	0·0 p.m.	,, 31. Thick mist with small misty rain on the Pike; below the mist at the other stations.
19·9	20·	19·9	11·45 a.m.	Sept. 30. Very misty with small misty rain on the Pike; clear at the other stations.
28·	27·9	27·5	3·0 p.m.	
43·	38·5	33 3	11·30 a.m.	Oct. 31. Misty on the Pike; clear at the other stations.
49·3	44·3	39·	9.15 a.m.	
39·9	39·2	38·1	6·15 p.m.	Nov. 30. Misty on Lingmell, Gabel, and the Pike; clear at the other stations.
47·3	45·9	44·4	11·30 a.m.	
41·	40·8	40·5	5·45 p.m.	
35·4	35·2	34·9	10·30 a.m.	Dec. 31. Fine and gleamy at all the stations.
39·9	39·2	38·3	1·0 p.m.	
29·8	29·8	29·8	11·15 a.m.	
16·5	15·6	8·8	11·45 a.m.	
34·1	32·9	31·1	0·48 p.m.	
34·6	33·8	31·5	10·30 a.m.	

TABLE XXXI.—DEDUCTIONS relative to the HUMIDITY of the ATMOSPHERE at the MOUNTAIN STATIONS, in the year 1853.

STATION.	Dry Bulb.	Wet Bulb.	Dew Point.	WEIGHT OF VAPOUR. In a cubic foot of Air.	WEIGHT OF VAPOUR. Required for Saturation of a cubic ft. of Air.	Degree of Humidity, (complete Saturation 1·000).
	°	°	°	Grains.	Grains.	
Wastdale Head, (10·2 a.m.) 247 above the Sea,	46·5	43·0	38·3	2·96	0·86	0·775
Do. do., (5·34 p.m.) „ „	48·8	44·3	39·6	2·98	1·14	·724
Brant Rigg, . . 924 „ „	45·4	42·0	37·3	2·87	0·81	·779
Stye Head, . . 1448 „ „	43·0	40·0	35·5	2·71	0·70	·795
Lingmell, . . 1778 „ „	41·2	39·1	35·8	2·74	0·47	·854
Sprinkling Tarn, . 1900 „ „	39·9	37·8	34·2	2·61	0·47	·849
Great Gabel, . . 2925 „ „	36·8	35·2	32·6	2·42	0·36	·872
Sca Fell Pike, . . 3166 „ „	34·1	32·9	31·1	2·28	0·25	·898

REMARKS.

1851.—The almost incredible quantity of 38·86 inches of rain precipitated on the "Stye" or shoulder of Sprinkling Fell in a single month (January) is, I believe, without a parallel in the temperate zone, or even in tropical latitudes, except in some of the mountainous regions of India during the prevalence of the monsoon.

Respecting the comparatively small amount of rain (14·47 inches) registered at Wastdale Head in January, the registrar says, "There were many wet days in the month of January, but few heavy falls of rain; you will perceive we had much heavier falls in the month of February." In other parts of the Lake District, it required no instrumental means to impress upon the oldest residents the conviction, that this was one of the wettest months within their recollection.

1852.—This year is distinguished by several striking peculiarities and abnormal conditions of climate, of which the most prominent are,—the large amount of rain, and its very unequal distribution over the different seasons, and the enormous and unprecedented downfall in the first two and last two months of the year. As regards the Lake District generally, the year 1852 exhibits by much the largest quantity of rain recorded in any annual period since the experiments were commenced in 1844; though the fall at Wastdale Head and Seathwaite was exceeded, in 1848, by 5·74 and 4·15 inches, respectively. At the coast, the depth in 1852 was exceeded in only three of the last 20 years—viz., in 1835, 1836, and 1841, in which the atmospheric precipitation reached 54·13, 58·97, and 55·97

inches, respectively. It may be observed, that the fall of rain in 1852 has been relatively much greater in the Westmoreland than in the Cumberland portion of the district.

In January and February, the fall at Seathwaite was 47·70 inches, and, in November and December, it amounted to 50·30 inches; so that, of 156·74 inches precipitated at the head of Borrowdale in 1852, exactly 98 inches descended in four months, whilst 58·74 inches were distributed over the remaining eight months of the year.

On the 11th and 12th of December, the quantity of rain measured at Stonethwaite (for 48 hours) was 9·11 inches; on five days in this month, the fall amounted to 16·36 inches; and, on eight days, to 20·97 inches!!

1853.—Among several anomalous and opposite characteristics presented by the years 1852 and 1853, the departure from the average in the rain fall is the most obvious and remarkable. While the former is the *wettest*, the latter is the *driest* year within the period comprehended by the Lake District observations. In 1852, the depth of water deposited by the atmosphere at Seathwaite was equivalent to 156·74 inches; and, in 1853, to 113·69 inches; a difference of 43 inches—nearly corresponding to the average annual fall at Whitehaven in the last ten years.

Notwithstanding the great deficit in the *quantity* of rain, the wet days at Seathwaite are two more than the average number; and, at Whitehaven, they amount to four, and, at the Flosh, to eight *more* than the number in the preceding memorably wet year. In both years, the Springs were unusually dry, and the fall of rain in the first six months was below an average quantity. The depth of rain measured at Seathwaite in December, was 1¼ inch; in the corresponding month of 1852, the fall amounted to 32·83 inches; and, at Stonethwaite, to 33·03 inches.

The Table (No. XXII.) exhibiting the rain fall in the Lake District Valleys during the last ten years, requires very little comment.

The greatest annual fall at Seathwaite was 160·9 inches, in 1844; the least, 113·7 inches, in 1853. The greatest monthly fall was 32·83 inches, in December, 1852. The greatest depth measured in 24 hours was 6·62 inches, in November, 1846; and, in 48 consecutive hours, 9·62 inches on the 25th and 26th of November, 1845, and 9·74 inches, on the 8th and 9th of October, 1846.

The Mountain Gauges.—The following tables shew the excess or deficiency per cent. of the principal Mountain Gauges over or under the quantity of rain received by the adjacent valleys, both in the summer and winter months, in each year since the instruments were erected in 1846. The positive sign signifies that the quantity is greater, and the negative sign that it is less, than the fall in the valley in the same period.

SUMMER MONTHS.

Year.	Wastdale Head.						Borrowdale.	
	Sca Fell.		Gt. Gabel, 2925 feet above Sea.	Sprinkling Tarn, 1900 feet above Sea.	Stye Head, 1443 feet above Sea.	Brant Rigg, 924 feet above Sea.	Seatollar Common, 1338 feet above Sea.	The Stye, 948 feet above Sea.
	The Pike, 3166 feet above Sea.	Lingmell, 1778 feet above Sea.						
1846.* . .	−13·5	...	− 7·5	+ 29·5	+ 12·0	−10·3	...	...
1847. . .	−13·5	...	− 7·5	+ 29·5	+ 12·0	−10·3	...	...
1848. . .	− 1·0	...	− 6·0	+ 41·5	+ 20·5	−14·0	...	...
1849. . .	− 6·5	− 8·0	− 7·0	+ 17·3	+ 9·0	−18·5	...	...
1850. . .	− 6·2	− 3·2	− 4·3	+ 35·3	+ 26·1	− 7·3	+4·0	+28·5
1851. . .	−11·0	− 0·6	−11·5	+ 36·6	+ 7·6	− 8·0	+6·3	+21·4
1852. . .	− 5·5	− 0·0	+ 0·5	+ 33·0	+ 25·3	+ 4·0	−3·0	+12·0
1853. . .	−15·3	− 6·5	− 8·8	+ 30·3	+ 24·2	− 8·9	−1·7	+14·2
Algebraical Sums.	−72·5	−18·3	−52·1	+253·0	+136·7	−73·3	+5·6	+76·1
Means. . . .	− 9·0	− 3·6	− 6·5	+ 31·6	+ 17·1	− 9·1	+1·4	+19·0

* The percentages in 1846 and 1847 shew the mean of the two years, which were tabulated together.

WINTER MONTHS.

Year.	Wastdale Head.						Borrowdale.	
	Sca Fell.		Great Gabel.	Sprinkling Tarn.	Stye Head.	Brant Rigg.	Seatollar Common.	The Stye.
	The Pike.	Lingmell.						
1846. . .	− 42·5	...	− 38·5	+11·3	+ 4·5	− 15·2	...	...
1847. . .	− 42·5	...	− 38·5	+11·3	+ 4·5	− 15·2	...	...
1848. . .	Leaked.	...	− 42·5	+ 1·5	+ 0·5	− 14·6	...	...
1849. . .	− 43·5	−28·8	− 40·5	− 3·2	−21·2	− 27·8	...	...
1850. . .	− 33·7	−13·9	− 22·5	+17·8	+ 3·2	− 12·8	−0·3	+26·8
1851. . .	− 45·1	− 3·0	− 23·3	+18·3	− 3·8*	− 10·0	−3·4	+21·7
1852. . .	− 39·5	−14·3	− 36·6	+16·3	+ 5·4	− 20·0	+1·5	+ 4·2
1853. . .	− 53·7	−24·9	− 52·2	− 7·0	− 4·7	− 15·8	−2·3	+ 5·2
Algebraical Sums.	−300·5	−84·9	−294·6	+66·3	−11·6	−131·4	−4·5	+57·9
Means. . . .	− 42·9	−16·9	− 36·8	+ 8·3	− 1·4	− 16·4	−1·1	+14·4

* From the falling off in the relative amount of rain at Stye Head in the winter months of 1851, it is suspected that some of the water may have been lost by leakage.—See *Introductory Remarks.*

The relative quantity of rain received by the gauges on the Stye or Sprinkling Fell, and on Seatollar Common, in Borrowdale, seems to be very variable. In the Summer months of 1850 and 1851, Seatollar Common, 1338 feet above the Sea, received 4 per cent. and 6·3 per cent. respectively *more* rain than the dale

at Seathwaite, and, in the Winter months of those years, 0·3 per cent. and 3·4 per cent. respectively *less* rain fell on the mountains than in the valley. But, in 1852, 3 per cent. *less* rain fell in the Summer and 1·5 per cent. *more* in the Winter months, than descended in the subjacent valley; and, in 1853, the fall is less than in the valley, both in the Summer and the Winter months. In the years 1850 and 1851, the relative excess of rain on the Stye, 948 feet above the Sea, was 27·6 and 21·5 per cent., but in 1852 and 1853, only 8·1 and 9·7 per cent. respectively; hence, in the memorably wet year 1852, this station received nearly 2 inches less rain than in 1851, and 21 inches less than in 1850.

The above tables shew that on the mountains, the greatest depth of rain invariably obtains at Sparkling Tarn, 1900 feet above the sea level. The current which brings our principal supply of rain is the South-west. It is characterized by a high temperature—is generally at or near the point of saturation—and, in most heavy and continuous rains, the depth of the stratum of vapour is considerable, extending from a thousand feet or less above the sea to probably 4000 or 5000 feet above it. In passing over the comparatively level tract of country between the coast and the mountain ranges, rain is deposited, but with little diminution in the temperature of the gases or vapour. The current is at length arrested in its progress by the hills—the vapour in contact with the bases of the mountains is subjected to more rapid condensation, during which it gives out a portion of its latent heat in a sensible form, whereby the temperature of the surrounding mass of air and vapour is increased, and, by virtue of its increased elasticity, it rises to a greater height; the diminution of temperature due to the increased elevation causes fresh deposition—the surrounding medium is again warmed—the vapour ascends still higher—is farther cooled, and more water forced from it; and thus, the same operation is continued and repeated, so long as an adequate supply of vapour is furnished from beneath. Hence, in the upper regions of the atmosphere, there is a vertical as well as a lateral current of vapour constantly rushing in to supply the loss by precipitation.

In ascending from the valley, the amount of vapour which the atmosphere is capable of supporting or containing in mechanical combination is found to diminish, while the difference between the air and dew point temperatures also gradually decreases. There must therefore be a point of elevation, where the quantity of vapour and the degree of humidity will combine to produce a maximum deposit of rain in a given time; and this plane of greatest condensation is found, in the English Lake District, at or about 2000 feet above the sea-level.

It does not follow, from what has just been stated, that the same law would hold good in the open atmosphere, (supposing it were possible to plant a gauge therein at an altitude of 2000 feet) because the rate of cooling upwards is much more sudden in ascending *on* the surface, than obtains in rising abruptly *from* the surface, as in a balloon; consequently, the temperature at any given elevation on

a mountain, as 2000 feet, is lower, and the condensation and precipitation of the warm oceanic vapour will be more rapid and copious, than at an equal height in the surrounding atmosphere.

On the other hand, when a Pluviometer is merely removed from a lower to a higher position *in the atmosphere*, as from the bottom to the top of a building, the quantity of rain is found to diminish with the elevation. Thus, the gauge on the tower of St James's Church, Whitehaven, on an average of 10 years, has received 12·1 inches, or 28 per cent. less rain than a precisely similar instrument stationed in a garden, near to and on the same level with the base of the building.

The explanation seems to be, that as in most heavy and continuous rains, the whole atmosphere up to a great height is charged with and precipitates vapour, the drops are enlarged by accretion after leaving the summit of the tower. It is only during heavy showers, when the drops are formed at a great altitude, that the upper gauge is in excess. In this case, the drops have probably been subjected to evaporation in passing through the comparatively warm and dry stratum of air intervening between the two instruments.

Among the mountain chains of the Indian Peninsula, Colonel Sykes finds the maximum fall of rain at 4500 feet, and that above this level the supply is diminished. The following tables and remarks are extracted from Colonel Sykes's valuable "Discussion of Meteorological Observations taken in India," published in the Philosophical Transactions, Part ii., for 1850.

Fall of Rain at various Heights in India. (*Western Coast.*)

		Inches.
Mean of Seven Stations on Western Coast, at Sea level,	. .	81·70
At 150 feet, Rutnagherry, in the Konkun,	. . . .	114·55
... 900 ...	Dapoolee, Southern Konkun, ...	134·96
... 1740 ...	Kundalla, the Pass from Bombay to Poona, . .	141·59
... 4500 ...	Mahabuleshwur, mean of 15 years, . . .	254·05
... 4500 ...	Mercara, in Coorg, mean of 3 years, . . .	143·35
... 4500 ...	Uttray Mullay, Travancore, mean of 2 years, .	263·21
... 6100 ...	Kotergherry, on the Neelgherries, 1 year, . .	81·71
... 8640 ...	Dodabetta, highest point of Western India, 1 year, .	101·24

Uttray Mullay Range—for 1849.

		Inches.
At 500 feet,	Base of range, ...	99
... 2200 ...	Attagherry, ...	170
... 4500 ...	Uttray Mullay, ...	250
... 6200 ...	Agusta Peak	194

Shewing at 6200 feet, 46 inches less rain than at 4500 feet.*

[The greatest depth at Mahabuleshwur in 21 years, was 338·38 inches, in 1849;

* From returns recently published by Dr Buist of Bombay, it appears that in Eastern India also, the maximum deposit of rain is found at 4500 feet, at which elevation, the annual quantity amounts (at Cheerapoong) to no less than 610 inches!! At Sylhet, 5000 feet above sea level, the fall is 209 inches; and, at Darjeeling, at 7000 feet, it is 125 inches. At Bombay, on an average of 30 years, the annual rain fall is 76·08 inches, and at Calcutta and Madras, for 8 years, it is 66·59 and 52·27 inches, respectively.

the least, 180·18 inches, in 1838; and nearly the whole of this enormous quantity falls in the four months of June, July, August, and September, during the prevalence of the South-west monsoon. The greatest *daily* fall was 13·06 inches; and the greatest *monthly* fall, 134·42 inches, in July, 1840.]

"Mr MILLER, in his 'Meteorology of the English Lake District,' has adduced sufficient evidence to prove that the same law, if it be a law, obtains in England in mountainous districts, but Mr MILLER's elevation of maximum fall is about 2000 feet instead of 4500, as in India. The difference no doubt results from the differences of latitude and consequent mean temperature, and would indicate that the stratum of vapour supplying the maximum quantity of rain floats at a less height beyond the tropics than within them."

In commenting upon the difference in the receipts of two Pluviometers—one placed near the ground, and the other above the dome of the Observatory at Bombay—Colonel SYKES further remarks:—

"These results therefore are in accordance with Professor PHILLIPS's and Mr MILLER's observations, taken at limited heights, but entirely antagonistic to Mr MILLER's own observations and those I have supplied in this paper from India, for heights exceeding a few hundred feet. The supposed law may hold good for small differences in elevation on the plains, but that law is reversed in mountainous districts."

On comparing the above Tables with those for the English Lake District, it will be perceived that the downfall of rain at Seathwaite and its adjacent mountain stations* exceeds the annual receipts at most of the stations in the peninsula of India, both in the plain, and at moderate and extreme elevations on the mountain ranges, the excess in the tropical region being chiefly at Mahabuleshwur, Mercara, and Uttray Mullay, which, although differing greatly in latitude, lie nearly on the same meridian, and are all the same elevation of 4500 feet above the sea.

In the mountain valleys of our Lake District, the greatest deposit of rain is always found at the head or Eastern extremity of the dale, because the vapour on arriving there is further obstructed and confined by the high mountains surrounding it, and, being dashed against their cold rocky sides, increased decomposition ensues; and, as the remaining vapour can only escape by slowly climbing over the tops, ere the transition or deportation is effected, fresh vapour rushes in to supply the vacuum produced by condensation; and the precipitation is not only rapid but continuous, so long as the warm saturated current continues to flow up or into the valley from the Western Ocean.† In like manner, more rain is

* The average fall of rain at Seathwaite for 9 years, is 144 inches, and on the Stye or Sprinkling Fell for 4 years, 159 inches.

† Seathwaite, from its position at the head or terminus of the Southern fork of Borrowdale, which is environed by the lofty mountains, Great Gabel, Glaramara, and Sprinkling Fell, is very favourably situated for the retention and exhaustion of the Rain-cloud. But, it is probably to the

impounded on the mountain passes, and in the hollows and basin-shaped cavities on the mountains, than on their summits. In passing over the tops, the vesicles are cooled and rendered heavier, and volumes of vapour flow down into the hollows and gorges, where they are hemmed in as in a cul de sac, and are converted into water. As decomposition proceeds, fresh masses of mist roll down the slopes, which in their turn are condensed and precipitated by the surrounding cold high land, and, as in the former case, the fall is both copious and uninterrupted, while the vapour in its swift transit over the adjacent peaks is but partially metamorphosed into drops of rain.

I also find that a gauge moderately sheltered receives more rain than one near it fully exposed to the weather. At Seathwaite, there are two rain gauges within a very short distance of each other,—the one at 10 inches, the other at 22 inches above the surface. The former is placed in a small garden surrounded by low walls; the latter is planted in an adjoining large field. On an average of 6 years, the garden gauge received 3·61 inches annually more rain than the field gauge. That the difference in the receipts of the two instruments is entirely due to the degree of exposure, and not to the difference in the heights of the receiving surfaces above the soil, is clearly shewn by the fact—that when both the gauges were located in the garden side by side, (as they were from June to December 1846) the *higher* impounded rather *more* rain than the lower.

In a former paper, I alluded to the rapid increment in the fall of rain in approaching the head or terminal point of a valley, and it was shewn numerically, that the effect of such approach was appreciable at intervals of one or two hundred yards. The difference in the quantity of water deposited at places closely contiguous to each other *on the mountains* is also sometimes surprisingly great. In the four years 1846-49, Seatollar Common, at 1338 feet, received 19 per cent. *less* rain annually than the valley at Seathwaite. In August 1849, the first mentioned gauge was removed 90 yards to the south-westward of its then position, and about five feet lower down the mountain. In the four following years, 1850-53, the excess was *in favour* of the "Common" to the extent of 0·5 per cent. The only circumstance attending the change which can be supposed materially to have affected the rain fall, is that of the new locality being somewhat more sheltered than the old one.

In a previous paper, published in the Philosophical Transactions, (Part i. for 1849) I have endeavoured to account for the great difference in the percentage of rain between the summer and winter half-year on the mountains, (particularly on Sca Fell and Gabel) which I attribute conjointly to the loss sustained by the gauge when the precipitation is in the form of snow, and to the lower altitude of the principal plane of condensation in the colder months.

copious supply of vapour poured into this narrow valley from the "Stye" pass—which trends nearly in the direction of the prevailing aerial current—that Seathwaite is chiefly indebted for the great excess of its rain-fall over every other locality in the Lake District.

Evaporation.—It may not be uninteresting to learn the annual amount of Evaporation in the immediate neighbourhood of a locality distinguished by so prodigious a rain fall. The Tables shew that a comparatively small part of the vapour requisite for the production of the enormous depth of water annually deposited in the Lake District, is formed over or from the terraqueous surface constituting the locality.

The chief supply comes from those tropical regions where an unclouded and almost vertical sun is continually lifting up the waters of the ocean into the atmosphere in an invisible form; the warm vesicular vapour thus generated by evaporation rises to a great height, and is transported by currents into our Northern climate, where it descends as rain, and maintains the existence of our lakes, rivers, and springs—while the large amount of sensible heat liberated during the condensation and conversion of the vapour elevates the temperature of the mountain valleys above that of the adjacent plains in the colder months, and no doubt tends materially to modify the climate at all seasons. A cubic inch of water will form a cubic foot of steam of the same apparent temperature as the water; but during the process of conversion it has absorbed 1000 degrees of heat from the surrounding atmospheric space—of which the thermometer gives no indication till the vapour is reconverted into water, when the latent caloric is slowly evolved in a sensible form. The amount of perceptible heat yielded up annually in the formation of 141 inches of rain at Seathwaite, is almost incredible, and the extent to which the climate of this naturally inhospitable region is ameliorated by this natural provision for equalizing temperature, must be proportionally great. The average depth of water raised by evaporation at Whitehaven in the 12 years between 1842 and 1853, is 29·664 inches, and this quantity may be accepted as the liquid equivalent of the average amount of vapour thrown off by our *lakes* in the course of a year. The real amount yielded by the *entire area* of the district is of course considerably less. The rain fall in the Lake country varies from 67 to 141 inches per annum. Of the 141 inches precipitated at the head of Borrowdale, probably at most 25 inches is supplied on the spot in a vesicular form; so that the mass of vapour annually imported from lower latitudes must be equal to 116 perpendicular inches of water—nearly the whole of which finds its way back again unchanged to its original source—the ocean.

The waters of the ocean cover nearly three-fourths of the surface of the globe; and, of the 38 millions of miles of dry land in existence, 28 millions belong to the Northern hemisphere. It is computed that the depth of water raised by evaporation from the entire surface of the ocean is about 4 feet per annum; while, in tropical seas, the amount converted into vapour cannot be less than a quarter of an inch daily all the year round, or say 90 inches annually. In the month of May, the liquid equivalent of evaporation from our lakes occasionally amounts to 0·25 inch per diem for several days together, and in extremely dry

periods, it sometimes, though very rarely, reaches $\frac{4}{10}$ths of an inch in 24 hours. On the 22d of May, 1844, 0·430 inch was measured, and on a freezing mixture being applied to DANIELL's hygrometer, the dew-point was found at 24° below the temperature of the air, which was 63°.* The evaporation for the month was 6·280 inches, with only a quarter of an inch of rain.

Surprise is often expressed that vegetation is enabled to retain its vitality, and even to come forth unscathed from the scorching ordeal to which it is occasionally subjected, in the continuous absence of rain for weeks, and even for months, as was the case in the spring of 1852, when the entire atmospheric precipitation in 70 days only amounted to $\frac{3}{10}$ths of an inch of water. But Nature has provided a check, which prevents the extreme heating and aridity from which the ground would otherwise suffer under such circumstances. In summer, the earth's surface is not unfrequently heated by the sun's rays to 100° or upwards, even in this latitude; but as the soil is a very bad conductor of heat, this temperature does not penetrate more than a very few inches downwards; and, at a moderate depth, the day and night temperatures are nearly identical. When the soil has once become thoroughly desiccated, it loses that capillary action by which excess of water is ordinarily withdrawn from it; in other words, evaporation ceases, and the subjacent moisture is thenceforth stored up for the special uses of the vegetable kingdom: it is absorbed by the roots of trees, shrubs, plants, and grasses, contributing to their growth and sustentation, as it slowly passes through their vascular structure into the atmosphere. When rain at length visits the thirsty soil, it does not recover its absorptive powers all at once; in the meantime, slight showers are forthwith transformed into vapour as they descend on the heated ground, while heavy rains flow off from the baked and indurated surface into the adjacent hollows, drains, and water-courses.

Evaporation is scarcely ever *entirely* suspended, either during the heaviest rains, or when the air is apparently saturated with vapour; at least, I have met with very few instances in which some daily loss was not appreciable to a finely graduated instrument. This important natural process is also active at very low temperatures of the air; and it goes on freely from the surface of frozen water, even when the whole mass is converted into a solid block of ice. From the 11th to the 16th of December, 1846, during which a brisk breeze prevailed, the loss from the frozen contents of my evaporation gauge was 0·450 inch, or ·075 per diem, the average for the month being ·033 per diem. In twelve days of frost in February, 1847, the evaporation from the ice was 0·552 inch, or ·046 per day, the average daily quantity for the month being ·030 inch. During ten days of keen frost between

* Whilst I am revising this paper, (April 21, 1854) Evaporation is unusually active for the season. The loss from the gauge in the 24 hours preceding 9 A.M., was 0·30 inch, which is the greatest daily quantity I have recorded in the month of April. During the last 3 days, the maximum temperature has varied from 65° to 73°, and the complement of the dew-point has ranged from 23°·3 to 25°·5—approaching to the extreme of hygroscopic dryness in this climate.

the 20th and 30th January, 1848, the ice had parted with 0·324 inch, or ·032 per diem, the average daily loss for the month being only ·024 inch. For five days of December, 1848, the loss was ·037 per diem, the daily average for the month being .032; and, from the 1st to the 8th of January, 1849, the depth evaporated was equivalent to ·017 per diem, the average daily loss during the month being ·029 inch. Lastly, in the first six days of January, 1854, the loss from the frozen contents of the gauge was 0·219 inch, or ·036 per diem, which is identical with the average daily evaporation in that month. Hence it appears that, notwithstanding the large amount of heat required not only to liquefy but to vaporize frozen water, the vapour thrown off by ice in an invisible form exceeds in amount the average daily evaporation from an equal surface of water, in the winter season. This apparently anomalous circumstance arises from the extreme dryness of the atmosphere, and its consequently increased capacity for vapour, in severe frosts; whereas, at other times during the winter, the air is very moist near the sea, being generally not more than 2° or 3° above the point of saturation.

At Whitehaven, the average amount of Evaporation for the twelve years ending with 1853, is 29·664 inches, and the fall of rain for the same period is 43·02 inches,* so that the depth of water precipitated exceeds that taken up by evaporation at the coast, in latitude 54°·30, by 13·357 inches. In the almost tropically fine and dry year 1842, the evaporation (36·83 inches) *exceeded* the rain-fall by 2·143 inches. The evaporation is not unfrequently in excess in the months of March, April, May, and September; and, in 1853, the amount of vapour absorbed by the atmosphere equalled or exceeded that which was restored to the ground in a liquid form, in *seven* months of the year.

The evaporation force at the altitudes of our highest mountains appears to be very feeble, notwithstanding the greatly diminished pressure of the air. At the summit of Great Gabel, (2925 feet above the sea) there is a vertical cavity in the rock, which, owing to the almost continuous presence of clouds, the high degree of humidity, and consequent slight evaporation at this elevation, always contains water, except in the very driest seasons. It is commonly believed that this "atmospheric spring" or well, as it is called, is *never* empty. The traditionary belief is, however, not strictly correct. The well was quite dry in the Spring of 1844, and also in April, 1852.

The Evaporation Gauge is a copper vessel, 8 inches in diameter, and rather more than an inch and a half in depth. Half an inch of water is accurately measured and poured into the dish every morning at 9 o'clock, and the loss is ascertained at the end of 24 hours by means of a carefully-graduated tube, reading to the $\frac{1}{1000}$th part of an inch.

The evaporation dish receives a fair proportion of wind and sun, and is always exposed in the open air during the day, except when rain is falling. At

* The average annual rain-fall for 20 years, at Whitehaven, is 46·59 inches.

night, and in wet weather, it is placed under a capacious shed, 9 feet in height, and open in front. Thus, it is considered that the evaporating surface is freely acted upon by all the circumstances concerned in promoting the vaporization of water.

The evaporation has now been recorded with scrupulous care, day by day for twelve years, at this observatory; and it is believed that the results are the best which have yet been obtained in this country.*

Temperature at the Mountain Stations.—Considerable difficulties have attended my endeavours to secure the monthly minimum temperature at the higher mountain stations. The mountain tops consist of an extremely hard and impenetrable rock,† so that it is impossible to fix in it a pole on which to fasten the box containing the thermometers. Some years ago, a party of Government Surveyors, engaged in triangulating from Sca Fell Pike, erected on the summit a cairn or pile of loose stones, having in its centre a stout pole, which projected about two feet above the apex of the cairn. To this pole the box containing the thermometers was originally attached. The box was freely pierced with circular holes at the sides and bottom, to permit the air to circulate freely through it. The horizontal thermometers were purposely disposed with a slight inclination downwards toward the bulb, to counteract, in some degree, the resistance offered to the contraction of the alcohol by the glass-pin or index; and it was not until the observations had been taken for a considerable time, that it was suspected, from the extreme degree of cold indicated, that strong currents of air passing through the apertures in the wooden case might cause the indices to descend towards the bulb, and so produce erroneous readings.

It was subsequently found that the apprehended source of error was real, and that it must have been continuously in more or less active operation, when the air was in rapid motion.

In the year 1851, I determined to make a renewed attempt to obtain correct thermometrical indications from self-registering instruments; and, that I might not have to depend entirely on one instrument, I also stationed minimum thermometers at Sprinkling Tarn and on the Gabel, at 1900 and 2925 feet respectively above the sea. A rock was selected which stood about four feet above the surface, or pieces of stone and rock were collected and piled up to that height: the thermometer boxes were placed thereon, and built in at the top and sides with loose stones, so as to secure them from being displaced by the wind, and, at the same time, to give the air ready access to the instruments through the interstices of the stones and augur-holes in the cases. So placed, the instruments were also concealed from tourists and other casual mountain visitors.

* In compiling the paragraph on Evaporation, I am indebted for several interesting facts to an elaborate paper "On the Physical Geography of Hindostan," by Dr Buist, LL.D., which appeared in the last (April) number of the Edinburgh New Philosophical Journal.

† An extremely indurated Green slate and Porphyritic slate.

In consequence of unforeseen difficulties, and the fatal occurrence above mentioned, I fear that all the observations obtained prior to July 1851 are open to some degree of suspicion, and it is with no little regret that I feel compelled to reject them *in toto* from the tables.

In the observations made subsequently to July, 1851, every reliance may be placed; indeed, I could not have much greater confidence in their accuracy, if the readings had all been taken by myself, as, about this time, I was fortunate in securing the services of a very careful and efficient registrar, who thoroughly understood the work he had to attend to, and who gave entire satisfaction in the performance of his duties up to the close of the year 1853, when the experiments were discontinued.

The mean difference between the absolute minima temperatures in the valley at Seathwaite and on Sca Fell Pike was, in 1852, 13°·8, and, in 1853, 12°·7, or an average of 13°·2 in 2798 feet. This result, which may be assumed to represent the average of the *extreme* difference at night in each month, is somewhat less than I had anticipated, as it exceeds by a small fraction only the *observed* mean depression during the day, derived from observations taken under ordinary circumstances. In examining the tables of temperature for the Mountain Stations, it must be borne in mind that it is impossible to ascertain the *actual* minimum temperature of the air on the mountains in the winter months, as the boxes containing the instruments (although raised four feet above the surface) are then generally covered more or less thickly with snow. In the winters of 1852 and 1853, the *maximum* depth of snow varied from 12 to 24 inches.

The effect of snow in keeping warm the earth and objects upon it is well known. Its slow conducting power is shown indirectly by the high relative temperature indicated by the thermometer at Sprinkling Tarn, in January, February, March, and April, 1853. The average difference between the thermometers on Sca Fell and at Sprinkling Tarn is 5°; but for the first four months of 1853, the average difference was 12°·2. The quantity of snow deposited at the Tarn (probably from its more sheltered position) is invariably greater than on the Pike; at the middle and end of February, the excess was six inches, and by this *extra* depth the temperature was raised 7 degrees. Mr Glaisher frequently found a thermometer on the grass slightly covered by snow to read 8° or 9° higher than one on grass clear of snow; and, in one instance, when the temperature fell suddenly, the difference (under three inches of snow) was no less than 34°; and when the thermometer on grass clear of snow had risen 30°, (from −6° to +24°) no variation had taken place in that under the snow, which still read 28°. Snow being so perfect a non-conductor of heat, evidently prevents to a high degree the loss of heat by radiation from bodies covered by it; and it also prevents the loss of heat from such bodies by conduction, at times when the temperature of the air is lower than they are. We may therefore fairly assume, that when the self-regis-

tering thermometers on Sca Fell and Gabel, buried under 12 to 18 inches of snow, indicated a minimum of 8°, 9°, or 10°, the *real* temperature of the air at these elevations must have been considerably below the zero point of FAHRENHEIT's scale.

The comparatively high readings of the night thermometers in the *winter months* being thus fully accounted for, I have endeavoured to discover whether there are any circumstances or conditions *ordinarily* present during the night hours, which may tend to modify the temperature of the upper regions of the atmosphere when the sun is below the horizon. The cooling effect produced by terrestrial radiation on the stratum of air in immediate contact with the earth, appears to extend to the ordinary height of a thermometer suspended in the air (4 feet), since we find the coldest nights, both on the surface and at 4 feet above it, are always those in which the principal conditions essential to free radiation are present—a serene and unclouded sky. The atmosphere is usually in more rapid motion on high lying lands and hills, than on the plains. Hence, plants growing on high exposed ground, where the air is more disturbed than in the valleys, suffer less from frost. Now, to whatever extent the temperature at 4 feet above the earth's surface is depressed by radiation, the depression at an equal altitude above the tops of the mountains will in general be much less, inasmuch as a calm state of the air very seldom obtains in these elevated regions.* The lateral atmospheric currents, rarely absent, will supply a large portion of the heat lost by conduction consequent on the radiation from the very limited areas constituting the mountain tops or peaks. Moreover, the large amount of heat thrown off by the earth's crust between sunset and sunrise may tend to keep up the temperature of the upper strata of the air during the night. But for this supply of heat communicated by terrestrial radiation, ice and snow would probably form much earlier in the autumn and perhaps rarely be absent during the summer months, on such elevated peaks as Sca Fell and the Gabel.†

In 1852, the mean of the absolute monthly minimum temperature on Sca Fell at 4 feet above the top, and perfectly protected from radiation, was 2°·6 less than the temperature on the grass at Seathwaite similarily determined.

The mildness and equability of the climate in our sequestered mountain valleys was further exemplified during the periods of extreme cold which prevailed over the country generally, in December and January last. The lowest temperature

* I have only met with one instance of the presence of a strong breeze in the valley, when the air was quite motionless on the top of a mountain. On the 21st of April 1848, I find the following memorandum in the register-book,—"Ascended Sca Fell, &c. There was a fresh breeze and appearance of rain on our leaving the valley at $11^h\ 40^m$ A.M., but before attaining the summit of Lingmell (1778 feet) the air became perfectly calm, and so continued till we had again reached the foot of the mountain. We were surprised to find that a strong breeze had prevailed in the valley during our absence on the Fell; and it continued to blow fresh throughout the evening. The clouds (Cumuli) were evidently electric, and generally below the summit of Sca Fell. We passed through one in descending, and a distant peal of thunder was heard from the top.

† The writer saw a patch of snow on Sca Fell, on the 15th of June, 1843.

at Seathwaite, in December, was 19°, and in January 18°. On December 16th, the thermometer at Manchester and Linslade fell to 6°, and, at Wakefield, to 9°·5; on January 3d, it fell at London to 10°; and, in the Midland Counties, Mr Lowe estimated the extreme of cold at 4° below zero.

The greatest depth of snow on the ground at one time in the vales of Wastdale and Borrowdale was about 3 inches. The simultaneous fall at Liverpool and London was 12 inches. On January 3d, the drifts over England and Wales, according to Mr Glaisher, varied from 3 feet to 10, 12, and 15 feet; they were very deep at Derby and at Grantham, and upon the Norfolk coast.

Hygrometrical Observations at the Mountain Stations.—The hygrometrical observations are so consistent with each other, and the resulting humidity of the atmosphere at the different elevations is so accordant with theoretical reasoning, that they carry with them a conviction of the faithfulness and of the care with which they must have been taken and recorded. I have not attempted to correct the dry and wet bulb observations at Wastdale, for the variation in temperature corresponding to the different hours in the day at which the mountain readings were taken, as the law of the horary fluctuation in the temperatures of the air and of evaporation for this latitude is unknown.

The very valuable tables compiled by Mr Glaisher for the latitude of Greenwich, it is believed would not apply to this district, and any forced or modified application of them to a mountain valley would be almost certain to vitiate the results. The chief points aimed at were, to ascertain the complement of the dew point and the relative degree of humidity at various heights above the sea and valley, and these, by far the most important deductions, will be but slightly if at all affected, by the interim variation of the air and evaporation temperatures in the valley. The observations are therefore tabulated as they were taken on the spot, simply corrected for slight instrumental errors.

In computing the decrease of temperature upwards in the atmosphere, the temperature assumed for the valley is the mean of the two observations taken on leaving and returning to the hamlet of Wastdale Head.

The hygrometrical results for the years 1852 and 1853 are in essential accordance with each other, and show *that whilst the absolute quantity of vapour in the atmosphere gradually diminishes, the degree of humidity (or the dampness of the air relative to its temperature) proportionately increases, in ascending above the comparatively level surface of the earth.*

The only exception to the law occurs on Lingmell, where the humidity indicated in both years is slightly greater than at Sprinkling Tarn, about 120 feet above it—the additional quantity of vapour requisite to produce complete saturation being the same at both places. The stations are situated at opposite extremities of the valley, and the difference is no doubt attributable to local causes. In 1852,

the humidity indicated at 3166 feet was rather less than at 2925 feet. This anomaly, though probably accidental, is coincident with the fact, that the average annual fall of rain on Great Gabel exceeds that on Sca Fell by 4·70 inches.

The results enable us to determine approximately the rate of cooling of the atmosphere by expansion, in ascending on the earth's surface above its general level. This may be computed by two independent classes of observations—either by the observed differences in temperature between the valley and Sca Fell and Gabel, or by the monthly minima recorded by self-registering thermometers at Seathwaite and the mountain stations.

In 1852, the observed mean difference in temperature between the Vale of Wastdale and the summit of the Pike was 12°·7, and, in 1853, 13°·5; the biennial mean is 13°·1 in 2919 feet, showing a fall of 1° in every 222·8 feet of elevation. In the same years, the depression between the valley and the top of the Gabel was 11°·6 and 12° respectively, in 2678 feet; or a mean rate of cooling by diminished pressure equivalent to 1° in 226·9 feet, *during the day.*

The difference between the absolute monthly minima at Seathwaite and on the Pike was, in 1852, 13°·8, and, in 1853, 12°·7, in 2798 feet,—the mean result indicating a fall of 1° in every 212 feet, *at night.* A similar comparison with the Gabel gives a depression of 1° in 218·5 feet. The combined results of the day and night observations give a fall of 1 in each 220 feet of elevation.

The depression in the Dew Point between the valley and Sca Fell Pike was, in 1852, 9°·4, and, in 1853, 7°·8—the combination showing a fall of 1° in 339·4 feet, in a perfectly saturated atmosphere. On the Gabel, the descent in those years was 7°·3 and 7°·0 respectively, exhibiting a mean variation in the vapour point of 1 in every 377 feet. The average result is a descent in the dew-point of 1° in 358·1 feet in height, with a simultaneous fall of 1° in every 224·8 feet in the temperature of the mechanically combined and invisible gases and vapour constituting the atmosphere.

The decrease of temperature in ascending appears to be more sudden during the night than the day; and as the mountain thermometers were thickly covered with snow in January, February, March, and December, 1853, the minimum temperature recorded in those months and also the annual mean must be considerably higher than would have been indicated had the instruments been freely exposed to the air. It is therefore concluded that the rate of cooling upwards by expansion is in reality more rapid than is shown by the result eliminated from the mean of the day and night observations, or 1° in every 220 feet of ascent. The variation indicated by the night observations alone (1 in 215 feet) is probably nearer the truth.

The difference in temperature between the valley and the tops of the mountains varies greatly, according to the amount of cloud, the presence or absence of mist on the mountains, and the height of the under surface of the Stratus or

Cirrostratus above the valley. When the mountain is enveloped in mist quite down to its base, particularly if accompanied by deposition, the difference in temperature does not exceed 7 or 8 degrees; while, under a perfectly clear and cloudless sky, it amounts to 18 or 20 degrees. By examining the Hygrometrical Tables in connection with the simultaneous records of the state of the weather, the variation of temperature under various conditions of the atmosphere as to clearness and humidity may be computed.

The effect of dense mist in assimilating the temperature of the different strata of the atmosphere (by emission of its latent heat during condensation) is shown in the observations of September 9th, 1847, taken on Snowdon in North Wales, when the depression in 3571 feet was scarcely 10 degrees.

Dr Buff gives the temperature on the Brocken, in 51°·51′ north, 3500 feet above the sea, 33°·8; at Göttingen, 51·30′ N. 480 feet above the sea 47°—a difference of 13°·2 in 3020 feet; and at Munster, in the same latitude, 190 feet above the sea, 49°·1—a fall of 15°·3 in 3310 feet. In tropical latitudes, the lowering of the temperature is much less sudden. On the table-lands of Mexico, at 6990 feet, the mean temperature, according to Humboldt, is 61°·34; and at Vera Cruz, on the sea-coast, in the same latitude, it is 77 degrees. At Quito, nearly under the equator, (0°·7′ S.) 8970 feet elevation, the temperature is 59·1, and, at the coast, 78·8 degrees.

The conditions of the decrease of temperature in ascending *on* the earth's surface and *from* the earth's surface seem to be very different.

From the observations made during four balloon ascents at Kew in the autumn of 1852, the number of feet of height corresponding to a fall of 1° Fahrenheit was, as under:—

On August 17th, . . .	292·0 Feet.
... 20th, . . .	290·7 ...
On October 21st, . . .	291·4 ...
On November 10th, . . .	312·0 ...

All the ascents, however, show an increasing degree of humidity up to the altitudes of the highest English mountains.

In a former paper, I ventured to remark, "The degree of humidity increases upwards from the earth's surface, and the condition or combination of conditions most favourable for the condensation and precipitation of vapour in the greatest abundance will probably be found somewhere about 2000 feet above the sea-level. It is assumed that the atmosphere is generally near the point of saturation at and above 2000 feet; but as the air temperature decreases with every farther increase of elevation, its capacity for vapour is proportionately diminished, and consequently there will be less to precipitate than at the point where the temperature of the air and that of the dew-point first begin to balance

each other."* This prediction is signally verified by the results of the hygrometrical observations taken on the mountains, in the years 1852 and 1853.

It is proposed to follow up and conclude the series of annual papers on the Meteorology of the Lake District, by an essay "On the Physical Geography of the English Lake and Mountain District," for which I am now engaged in collecting materials. This treatise, in addition to the meteorological facts and results arrived at in the course of this inquiry, and the aerial phenomena common or peculiar to mountain localities, is intended to embrace the subjects or sciences usually included under the head of "Physical Geography,"—as the Geology, Botany, Ornithology, and Entomology of the district, in connection with climate and elevation; and special attention will be directed to the plants and insects (Lepidoptera) indigenous to the mountain regions. As the English Lake District presents a wide and rich field for research in these departments of science, it is hoped that, with competent assistance, this work will not be wholly destitute of interest or importance to those occupied in similar investigations, in other and widely different localities.

* Philosophical Transactions, Part ii., 1849.

Observatory, Whitehaven,
April 15, 1854.

IX.—*On the Dynamical Theory of Heat.* Part V. *Thermo-electric Currents.* By William Thomson, M.A., Professor of Natural Philosophy in the University of Glasgow.

(Read 1st May 1854.)

Preliminary §§ 97–101. *Fundamental Principles of General Thermo-dynamics recapitulated.*

97. Mechanical action may be derived from heat, and heat may be generated by mechanical action, by means of forces either acting between contiguous parts of bodies, or due to electric excitation; but in no other way known, or even conceivable, in the present state of science. Hence Thermo-dynamics falls naturally into two Divisions, of which the subjects are respectively, *the relation of heat to the forces acting between contiguous parts of bodies*, and *the relation of heat to electrical agency*. The investigations of the conditions under which thermo-dynamic effects are produced, in operations of any fluid or fluids, whether gaseous or liquid, or passing from one state to the other, or to or from the solid state, and the establishment of universal relations between the physical properties of all substances in these different states, which have been given in Parts I.–V. of the present series of papers, belong to that first great Division of Thermo-dynamics—to be completed (as is intended for future communications to the Royal Society) by the extension of similar researches to the thermo-elastic properties of solids. The second Division, or Thermo-electricity, which may include many kinds of action as yet undiscovered, has hitherto been investigated only as far as regards the agency of heat in producing electrical effects in non-crystalline metals. In a mechanical Theory of electric currents, communicated to the Royal Society, Dec. 15, 1851,* the application of the General Laws of the Dynamical Theory of Heat to this kind of agency was made, and certain universal relations precisely analogous to the thermo-elastic properties of fluids established in the previous treatment of the First Division of the subject, were established between the thermo-electric properties of non-crystalline metals. The object of the present communication is to extend the theory to the phenomena of thermo-electricity in crystalline metals; but as recent experimental researches on air have pointed out an absolute thermometric scale,† the use of which in express-

* See "Proceedings" of that date, or Philosophical Magazine, 1852, where a sufficiently complete account of the investigations and principal results is given.

† That is a scale defined without reference to effects experienced by any particular kind of matter. Such a scale, founded on general thermo-dynamic relations of heat and matter, and requiring reference to a particular thermometric substance only for defining the unit or degree, was, so far as I know,

ing the general laws of the dynamical theory of heat, both leads to a very concise mode of stating the principles, and shows the most convenient forms of the expressions brought forward in my former communication, the whole subject of thermo-electricity in metals will be included in the theoretical investigations now communicated. I shall take the opportunity of introducing developments and illustrations, which, although communicated at the meeting of the Royal Society along with the original treatment of the subject, did not appear in the printed abstract; and I shall add some experimental conclusions which have since been arrived at, in answer to questions proposed in the former theoretical investigation.

98. Before entering on the treatment of the special subject, it is convenient to recal the fundamental Laws of the Dynamical Theory of Heat, and necessary to explain the thermometric assumption by which temperature is now to be measured.

The conditions under which heat and mechanical work are mutually convertible by means of any material system, subjected to either a continuous uniform action, or a cycle of operations at the end of which the physical conditions of all its parts are the same as at the beginning, are subject to the following laws:—

Law I. The material system must give out exactly as much energy as it takes in, either in heat or mechanical work.

Law II. If every part of the action, and all its effects, be perfectly reversible,

first proposed in a communication to the Cambridge Philosophical Society (Proceedings, May 1848, or Philosophical Magazine, October 1848). The particular thermometric assumption there suggested, was that a thermo-dynamic engine working to perfection, according to Carnot's criterion, would give the same work from the same quantity of heat, with its source and refrigerator differing by one degree of temperature in any part of the scale; the fixed points being taken the same as the 0° and 100° of the centigrade scale. A comparison of temperature, according to this assumption, with temperature by the air thermometer, effected by the only data at that time afforded by experiment, namely, Regnault's observations on the pressure and latent heat of saturated steam at temperatures of from 0° to 230° of the air thermometer, showed, as the nature of the assumption required, very wide discrepance, even inconveniently wide between the fixed points of agreement. A more convenient assumption has since been pointed to by Mr Joule's conjecture, that Carnot's function is equal to the mechanical equivalent of the thermal unit divided by the temperature by the air thermometer from its zero of expansion; an assumption which experiments on the thermal effects of air escaping through a porous plug, undertaken by him in conjunction with myself, for the purpose of testing it, (Philosophical Magazine, Oct. 1852,) have shown to be not rigorously but very approximately true. More extensive and accurate experiments have given us data for a closer test (Phil. Trans., June 1853), and in a joint communication by Mr Joule and myself to the Royal Society of London, to be made during the present session, we propose that the numerical measure of temperature shall be not founded on the expansion of air at a particular pressure, but shall be simply the mechanical equivalent of the thermal unit divided by Carnot's function. We deduce from our experimental results, a comparison between *differences on the new scale from the temperature of freezing water*, and *temperatures centigrade of* Regnault's *standard air thermometer*, which shows no greater discrepance than a few hundredths of a degree, at temperatures between the freezing and boiling points, and, through a range of 300° above the freezing point, so close an agreement that it may be considered as perfect for most practical purposes. The form of assumption given below in the text as the foundation of the new thermometric system, without explicit reference to Carnot's function, is equivalent to that just stated, inasmuch as the formula for the action of a perfect thermo-dynamic engine, investigated in § 25, expresses (§ 42) that the heat used is to the heat rejected in the proportion of the temperature of the source to the temperature of the refrigerator, if Carnot's function have the form there given as a conjecture, and now adopted as the definition of temperature.

and if all the localities of the system by which heat is either emitted or taken in be at one or other of two temperatures the aggregate amount of heat taken in or emitted at the higher temperature, must exceed the amount emitted or taken in at the lower temperature always in the same ratio when these temperatures are the same, whatever be the particular substance or arrangement of the material system, and whatever be the particular nature of the operations to which it is subject.

99. *Definition of Temperature*, and *General Thermometric Assumption.*—If two bodies be put in contact, and neither gives heat to the other, their temperatures are said to be the same; but if one gives heat to the other, its temperature is said to be higher.

The temperatures of two bodies are proportional to the quantities of heat respectively taken in and given out in localities at one temperature and at the other, respectively, by a material system subjected to a complete cycle of perfectly reversible thermo-dynamic operations, and not allowed to part with or take in heat at any other temperature: or, the absolute values of two temperatures are to one another in the proportion of the heat taken in to the heat rejected in a perfect thermo-dynamic engine working with a source and refrigerator at the higher and lower of the temperatures respectively.

100. *Convention for thermometric unit, and determination of absolute temperatures of fixed points in terms of it.*

Two fixed points of temperature being chosen according to Sir Isaac Newton's suggestion, by particular effects on a particular substance or substances, the difference of these temperatures is to be called unity, or any number of units or degrees, as may be found convenient. The particular convention is, that the difference of temperatures between the freezing and boiling points of water under standard atmospheric pressure shall be called 100 degrees. The determination of the absolute temperatures of the fixed points is then to be effected by means of observations indicating the economy of a perfect thermo-dynamic engine, with the higher and the lower respectively as the temperatures of its source and refrigerator. The kind of observation best adapted for this object was originated by Mr Joule, whose work in 1844* laid the foundation of the theory, and opened the experimental investigation; and it has been carried out by him, in conjunction with myself, within the last two years, in accordance with the plan proposed in Part IV.† of the present series. The best results, as regards this determination, which we have yet been able to obtain is, that the temperature of freezing water is 273·7 on the absolute scale; that of the boiling point being consequently 373·7. Farther details regarding the new thermometric system will be found in

* On the Changes of Temperature occasioned by the Rarefaction and Condensation of Air. See Proceedings of the Royal Society, June 1844; or, for the paper in full, Phil. Mag., May 1845.

† On a Method of discovering experimentally the Relation between the Heat Produced and the Work Spent in the Compression of a Gas. Trans. R.S.E., April 1851.

a joint communication to be made by Mr JOULE and myself to the Royal Society of London before the close of the present session.

101. A corollary from the second General Law of the Dynamical Theory stated above in § 98, equivalent to the law itself in generality, is, that if a material system experience a continuous action, or a complete cycle of operations, of a perfectly reversible kind, the quantities of heat which it takes in at different temperatures are subject to a linear equation, of which the coefficients are the corresponding values of an absolute function of the temperature. The thermometric assumption which has been adopted is equivalent to assuming that this absolute function is the reciprocal of the temperature; and the equation consequently takes the form

$$\frac{H_t}{t} + \frac{H_{t'}}{t'} + \frac{H_{t''}}{t''} + \&c. = 0,$$

if t, t', &c., denote the temperatures of the different localities where there is either emission or absorption of heat, and $\pm H_t$, $\pm H_{t'}$, $\pm H_{t''}$, &c., the quantities of heat taken in or given out in those localities respectively. To prove this, conceive an engine emitting a quantity H_t of heat at the temperature t, and taking in the corresponding quantity $\frac{t'}{t} H_t$ at the temperature t'; then an engine emitting the quantity $\frac{t'}{t} H_t + H_{t'}$ at t', and taking in the corresponding quantity $t''\left(\frac{H_t}{t} + \frac{H_{t'}}{t'}\right)$ at the temperature t''; another emitting $t''\left(\frac{H_t}{t} + \frac{H_{t'}}{t'}\right) + H_t$ at t'', and taking in the corresponding quantity $t'''\left(\frac{H_t}{t} + \frac{H_{t'}}{t'} + \frac{H_{t''}}{t''}\right)$ at t'''; and so on. Considering $n-2$ such engines as forming one system, we have a material system causing, by reversible operations, an emission of heat amounting to H_t at the temperature t, $H_{t'}$ at the temperature t',....and $H_{t^{(n-2)}}$ at $t^{(n-2)}$; and taking in $t^{(n-1)}\left(\frac{H_t}{t} + \frac{H_{t'}}{t'} + \ldots + \frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right)$ at the temperature $t^{(n-1)}$. Now this system, along with the given one, constitutes a complex system, causing on the whole neither absorption nor emission of heat at the temperatures t, t', &c., or at any other temperatures than $t^{(n-1)}$, $t^{(n)}$; but giving rise to an absorption or emission equal to $\pm\left[t^{(n-1)}\left(\frac{H_t}{t} + \frac{H_{t'}}{t'} + \ldots + \frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right) + H_{t^{(n-1)}}\right]$ at $t^{(n-1)}$, and an emission or absorption equal to $\pm H_{t^{(n)}}$ at $t^{(n)}$. This complete system fulfils the criterion of reversibility, and, having only two temperatures at localities where heat is taken in or given out, is therefore subject to Law II.; that is, we must have

$$H_{t^{(n)}} = \frac{t^{(n)}}{t^{(n-1)}} - \left[t^{(n-1)}\left(\frac{H_t}{t} + \frac{H_{t'}}{t'} + \ldots + \frac{H_{t^{(n-2)}}}{t^{(n-2)}}\right) + H_{t^{(n-1)}}\right]$$

which is the same as

$$\frac{H_t}{t} + \frac{H_{t'}}{t'} + \ldots + \frac{H_{t^{(n-1)}}}{t^{(n-1)}} + \frac{H_{t^{(n)}}}{t^{(n)}} = 0 \qquad . \quad . \quad . \quad . \quad (1).$$

This equation may be considered as the mathematical expression of the Second fundamental Law of the Dynamical Theory of Heat. The corresponding expression of the First Law is

$$W + J(H_t + H_{t'} + \ldots + H_{t(n-1)} + H_{t(n)}) = 0 \quad . \quad . \quad . \quad . \quad (2),$$

where W denotes the aggregate amount of work spent in producing the operations, and J the mechanical equivalent of the thermal unit.

§§ 102–106. *Initial examination of Thermo-dynamic circumstances regarding Electric Currents in Linear Conductors.*

102. PELTIER's admirable discovery that an electric current in a metallic circuit of antimony and bismuth produces cold where it passes from bismuth to antimony, and heat where it passes from antimony to bismuth, shows how an evolution of mechanical effect, by means of thermo-electric currents, involves transference of heat from a body at a higher temperature to a body at a lower temperature, and how a reverse thermal effect may be produced, by thermo-electric means, from the expenditure of work. For if a galvanic engine be kept in motion doing work, by a thermo-electric battery of bismuth and antimony; the current by means of which this is effected passing, as it does, from bismuth to antimony through the hot junctions, and from antimony to bismuth through the cold junctions, must cause absorption of heat in each of the former, and evolution of heat in each of the latter; and to sustain the difference of temperature required for the excitation of the electro-motive force, even were there no propagation of heat by conduction through the battery, it would be necessary continually, during the existence of the current, to supply heat from a source to the hot junctions, and to draw off heat from the cold junctions by a refrigerator:—Or, if work be spent to turn the engine faster than the rate at which its inductive reaction balances the electro-motive force of the battery, there will be a reverse current sent through the circuit, producing absorption of heat at the cold junctions, and evolution of heat at the hot junctions, and consequently effecting the transference of some heat from the refrigerator to the source.

103. We see then, that in PELTIER's phenomenon we have a reversible thermal agency of exactly the kind supposed in the second Law of the Dynamical Theory of Heat. Before, however, we can apply either this or the first Law, we must consider other thermal actions which are involved in the circumstances of a thermo-electric current; and with reference to the second Law we shall have to examine whether there are any such of an essentially irreversible kind.

104. It is to be remarked, in the first place, that a current cannot pass through a homogeneous conductor without generating heat in overcoming resistance. This effect, which we shall call the *frictional generation of heat*, has been discovered by JOULE to be produced at a rate proportional to the square of the

strength of the current; and taking place equally with the current in one direction or in the contrary, is obviously of an irreversible kind. Any other thermal action that can take place must depend on the heterogeneousness of the circuit, and must be of a kind reversible with the current.

105. Now if in an unbroken circuit with an engine driven by a thermo-electric current, the strength of the current be infinitely small compared with what it would be were the engine held at rest, or, which is the same, if the engine be kept at some such speed that its inductive electro-motive force may fall short of, or may exceed, by only an infinitely small fraction of itself, the amount required to balance the thermal electro-motive force of the battery, there will only an infinitely small fraction of the work done by the current in the former case, or of the work done in turning the engine in the latter, be wasted on the frictional generation of heat through the electric circuit. In these circumstances, it is clear, that whatever mechanical effect would be produced in any time by the engine from a direct current of a certain strength, an equal amount of work would have to be spent in forcing it to move faster and keeping up an equal reverse current for the same length of time; and as the direct and reverse currents would certainly produce equal and opposite thermal effects at the junctions, and elsewhere in all actions depending on heterogeneousness of the circuit, it appears that, were there no propagation of heat through the battery by ordinary conduction, CARNOT'S criterion of a perfect thermo-dynamic engine would be completely fulfilled, and a definite relation, the same as that which has been investigated (§ 25) already by considering expansive engines fulfilling the same criterion, would hold between the operative thermal agency and the mechanical effect produced. It appears extremely probable that this relation does actually subsist between the *part of the thermal agency which is reversed with the current* and the mechanical effect produced by the engine, and that the ordinary conduction of heat through the battery takes place independently of the electrical circumstances. The following proposition is therefore assumed as a fundamental hypothesis in the Theory at present laid before the Royal Society.

106. *The electro-motive forces produced by inequalities of temperature in a circuit of different metals, and the thermal effects of electric currents circulating in it, are subject to the laws which would follow from the general principles of the dynamical theory of heat if there were no conduction of heat from one part of the circuit to another.*

In adopting this hypothesis, it must be distinctly understood that it is only a hypothesis, and that, however probable it may appear, experimental evidence in the special phenomena of thermo-electricity is quite necessary to prove it. Not only are the conditions prescribed in the second Law of the Dynamical Theory not completely fulfilled, but the part of the agency which does fulfil them is in all

known circumstances of thermo-electric currents excessively small in proportion to agency inseparably accompanying it and essentially violating those conditions. Thus, if the current be of the full strength which the thermal electromotor alone can sustain against the resistance in its circuit, the whole mechanical energy of the thermo-electric action is at once spent in generating heat in the conductor;—an essentially irreversible process. The whole thermal agency immediately concerned in the current, even in this case when the current is at the strongest, is (from all we know of the magnitude of the thermo-electric force and absorptions and evolutions of heat,) probably very small in comparison with the transference of heat from hot to cold by ordinary conduction through the metal of the circuit. It might be imagined, that by choosing, for the circuit, materials which are good conductors of electricity and bad conductors of heat, we might diminish indefinitely the effect of conduction in comparison with the thermal effects of the current; but unfortunately we have no such substance as a *non-conductor* of heat. The metals which are the worst conductors of heat, are nearly in the same proportion the worst conductors of electricity; and all other substances appear to be comparatively very much worse conductors of electricity than of heat; stones, glass, dry wood, and so on, being, as compared with metals, nearly perfect non-conductors of electricity, and yet possessing very considerable conducting powers for heat. It is true, we may, as has been shown above, diminish without limit the waste of energy by frictional generation of heat in the circuit, by using an engine to do work and react against the thermal electro-motive force; but, as we have also seen, this can only be done by keeping the strength of the current very small compared with what it would be if allowed to waste all the energy of the electro-motive force on the frictional generation of heat; and it therefore requires a very slow use of the thermo-electric action. At the same time, it does not in any degree restrain the dissipation of energy by conduction, which is always going on, and which will therefore bear an even much greater proportion to the thermal agency electrically spent than in the case in which the latter was supposed to be unrestrained by the operation of the engine. By far the greater part of the heat taken in at all, then, in any thermo-electric arrangement, is essentially dissipated, and there would be no violation of the great natural law expressed in CARNOT's principle, if the small part of the whole action, which is reversible, gave a different, even an enormously different, and either a greater or a less, proportion of heat converted into work to heat taken in, than that law requires in all completely reversible processes. Still, the reversible part of the agency, in the thermo-electric circumstances we have supposed, is in itself so *perfect*, that it appears in the highest degree probable it may be found to fulfil independently the same conditions as the general law would impose on it if it took place unaccompanied by any other thermal or thermo-dynamic process.

§§ 107–111. *Mathematical expression of the Thermo-dynamic circumstances of Currents in Linear Conductors.*

107. In a heterogeneous metallic conductor the whole heat developed in a given time, will consist of a quantity generated *frictionally*, increased or diminished by the quantities produced or absorbed in the different parts by action depending on heterogeneousness of the circuit. The former, according to the law discovered by JOULE, may be represented by a term $B\gamma^2$, in which B denotes a constant depending only on the resistance of the circuit. The latter, being reversible with the current, may be assumed, at least for infinitely feeble currents, to be, in a given conductor, proportional simply to the strength of the current; and hence, the whole quantity of heat evolved in a given time, must be expressible by a term of the form $-A\gamma$, where A, whether it varies with γ or not, has a finite, positive, or negative value, when γ is infinitely small. Hence, the whole heat developed in any portion of a heterogeneous metallic conductor in a unit of time, must be expressible by the formula

$$-A\gamma+B\gamma^2;$$

where B is essentially positive, but A may be positive, negative, or zero, according to the nature of the different parts of the conducting arc. It may be assumed, with great probability, that the quantities A and B are absolutely constant for a given conductor with its different parts at given constant temperatures, and that when the temperatures of the different parts of a conductor are kept as nearly constant as possible with currents of different strengths passing through it, the quantities A and B can only depend on γ, inasmuch as it may be impossible to prevent the interior parts of the conductor from varying in temperature, and so changing in their resistance to conduction of electricity, or in their thermo-electric properties. In the present paper, accordingly, A and B are assumed to depend solely on the nature and thermal circumstances of the conductor, and to be independent of γ; but the investigations and conclusions would be applicable to cases of action with sufficiently feeble currents, probably to all currents due solely to the thermal electromotive force, even if A and B were in reality variable, provided the limiting values of these quantities for infinitely small values of γ be used.

108. Let us consider a conductor of any length and form, but of comparatively small transverse dimensions, composed of various metals, at different temperatures, but having portions at its two extremities homogeneous, and at the same temperature. These terminal portions will be denoted by E and E′, and will be called the *principal electrodes*, or *the electrodes of the principal conductor*; the conductor itself being called the *principal conductor* to distinguish it from others, either joining its extremities or otherwise circumstanced, which we may have to consider again.

Let an electro-motive force be made to act continuously and uniformly between these electrodes; as may be done for instance by means of a metallic disc included in the circuit touched by electrodes at its centre and a point of its circumference, and made to rotate between the poles of a powerful magnet, an arrangement equivalent to the "engine" spoken of above. Let the amount of this electromotive force be denoted by P, to be regarded as positive, when it tends to produce a current from E through the principal conductor, to E′. Let the absolute strength of the current, which, in these circumstances, passes through the principal conductor, be denoted by γ, to be considered as positive, if in the direction of P when positive.

109. Then, $P\gamma$ will be the amount of work done by the electro-motive force in the unit of time. As this work is spent wholly in keeping up a uniform electric current in the principal conductor, it must be equal to the mechanical equivalent of the heat generated, since no other effect is produced by the current. Hence, if $-A\gamma+B\gamma^2$ be, in accordance with the preceding explanations, the expression for the heat developed in the conductor in the unit of time by the current γ, and if J, as formerly, denote the mechanical equivalent of the thermal unit, we have

$$P\gamma=J(-A\gamma+B\gamma^2) \quad . \quad . \quad . \quad . \quad (3),$$

which is the expression for the particular circumstances of the first Fundamental Law of the Dynamical Theory of Heat.

Hence, by dividing by γ, we have

$$P=J(-A+B\gamma) \quad . \quad . \quad . \quad . \quad . \quad (4),$$

from which we deduce

$$\gamma=\frac{P+JA}{JB} \quad . \quad . \quad . \quad . \quad . \quad (5).$$

110. These equations show that, according as P is greater than, equal to, or less than $-JA$, the value of γ is positive, zero, or negative; and that, in any of the circumstances, the strength of the actual current is just the same as that of the current which an electro-motive force equal to $P+JA$ would excite in a homogeneous metallic conductor having JB for the absolute numerical measure of its galvanic resistance. Hence we conclude:—

(1.) That in all cases in which the value of A is finite, there must be an intrinsic electro-motive force in the principal conductor, which would itself produce a current if the electrodes E, E′, were put in contact with one another, and which must be balanced by an equal and opposite force, JA, applied either by means of a perfect non-conductor, or some electromotor, placed between E and E′, in order that there may be electrical equilibrium in the principal conductor;

And (2.) That JB, which cannot vanish in any case, is the absolute numerical measure of the galvanic resistance of the principal conductor itself.

It appears, therefore, that the whole theory of thermo-electric force in linear conductors is reduced to a knowledge of all the circumstances on which the value of

the coefficient A, in the expression $-A\gamma+B\gamma^2$ for the heat developed throughout any given conductor, depends.

110. To express the Second General Law, we must take into account the temperatures of the different localities of the circuit in which heat is evolved or absorbed, when the current is kept so feeble (by the action of the electro-motive force P, against the thermo-electric force of the system), as to render the frictional generation of heat insensible. Denoting then by $a_t\gamma$ the heat absorbed in all parts of the circuit which are at the temperature t, by the action of a current of infinitely small strength γ: so that the term $-A\gamma$, expressing the whole heat generated not frictionally throughout the principal conductor in any case, will be the sum of all such terms with their signs changed, or

$$A\gamma=\Sigma a_t\gamma,$$

which gives

$$\Sigma a_t=A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6);$$

and, if F denote the value of the electro-motive force required to balance the thermo-electric tendency, we have

$$F=J\Sigma a_t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

The Second General Law, as expressed above in equation (1), applied to the present circumstances, gives immediately,

$$\Sigma\frac{a_t\gamma}{t}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or, since γ is the same for all terms of the sum,

$$\Sigma\frac{a_t}{t}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

111. Of these equations, (7), and (3) from which it is derived, involve no hypothesis whatever, but merely express the application of a great natural law,—discovered by JOULE for every case of thermal action whether chemical electrical or mechanical,—to the electrical circumstances of a solid linear conductor, having in any way the property of experiencing reverse thermal effects from infinitely feeble currents in the two directions through it. Equation (9) expresses the hypothetical application of the Second General Law discussed above in § 106. The two equations, (7) and (9), express all the information that can be derived from the General Dynamical Theory of Heat, regarding the special thermal and electrical energies brought into action by inequalities of temperature, or by the independent excitation of a current, in a solid linear conductor whether crystalline or not. The condition that the circuit is to be linear, being merely one of convenience in the initial treatment of the subject, may of course be removed by supposing linear conductors to be put together, so as to represent the circumstances of a solid conductor of electricity, with any distribution of electric currents whatever through it; and we may therefore regard these two equations as the Fundamental Equations of the Mechanical Theory of Thermo-electric Currents. To work out the theory for crystalline or non-crystalline conductors, it is necessary

to consider all the conditions which determine the generation or absorption of heat in different parts of the circuit, whatever be the properties of the metals of which it is formed. This we may now proceed to do; first for non-crystalline, and after that for crystalline metals.

§§ 112–124. *General Equations of Thermo-electric Currents, in non-crystalline Linear Conductors.*

112. The only reversible thermal effect of electric currents, which experiment has yet demonstrated, is that which PELTIER has discovered in the passage of electricity from one metal to another. Besides this, we may conceive that in one homogeneous metal formed into a conductor of varying section, different thermal effects may be produced by a current in any part, according as it passes in the direction in which the section increases, or in the contrary direction; and, with greater probability, we may suppose that a current in a conductor of one metal unequally heated, may produce different thermal effects according as it passes from hot to cold, or from cold to hot. But MAGNUS has shown, by careful experiments, that no application of heat can sustain a current in a circuit of one homogeneous metal, however varying in section; and from this it is easy to conclude, by equations (7) and (9), that there can be no reversible thermal effect due to the passage of a current between parts of a homogeneous metallic conductor having different sections. Now, it is clear that no circumstances, except those which have just been mentioned, can possibly give rise to different thermal effects in any part of a linear conductor of the same or of different metals, uniformly or non-uniformly heated, provided none of them be crystalline; and we have, therefore, at present nothing in the sum Σa_t, besides the terms depending on the passage of electricity from one metal to another, which certainly exist, and terms which may possibly be discovered, depending on its passage from hot to cold, or from cold to hot in the same metal.

113. Let the principal conductor consist of n different metals; in all $n+1$ parts, of which the first and last are of the same metal, and have their terminal portions (which we have called the electrodes E and E′) at the same temperature T_0. Let T_1, T_2, T_3, &c., denote the temperatures of the different junctions in order, and let Π_1, Π_2, Π_3, &c., denote the amounts (positive or negative) of heat absorbed at them respectively by a positive current of unit strength during the unit of time. Let $\gamma\sigma_1 dt$, $\gamma\sigma_2 dt$, $\gamma\sigma_3 dt$, &c., denote the quantities of heat evolved in each of the different metals in the unit of time by a current of infinitely small strength, γ, passing from a locality at temperature $t+dt$ to a locality at temperature t. Without hypothesis, but by an obvious analogy, we may call the elements σ_1, σ_2, &c., the *specific heats of electricity in the different metals*, since they express the quantities of heat absorbed or evolved by the unit of current electricity in passing from cold to hot, or from hot to cold, between localities differing by a degree of temperature

in each metal respectively. It is easily shown (as will be seen by the treatment of the subject to follow immediately) that if the values of σ_1, σ_2, &c., depend either on the *section* of the conductor, or on the rate of variation of temperature along it, or on any other variable differing in different parts of the conductor, except the temperature, a current might be maintained by the application of heat to a homogeneous metallic conductor. We may, therefore, at once assume them to be, if not invariable, absolute functions of the temperature. From this it follows, that if ϕt denote any function of t, the value of the sum, $\int \phi t \, \sigma \, dt$, for any conducting arc of homogeneous metal, depends only on the temperatures of its extremities; and therefore the parts of the sums Σa_t and $\frac{\Sigma a_t}{t}$, corresponding to the successive metals in the principal conductor, are respectively

$$-\int_{T_1}^{T_0} \sigma_1 \, dt, \quad -\int_{T_2}^{T_1} \sigma_2 \, dt, \ldots\ldots\ldots -\int_{T_n}^{T_{n-1}} \sigma_n \, dt, \quad -\int_{T_0}^{T_n} \sigma_1 \, dt,$$

and

$$-\int_{T_1}^{T_0} \frac{\sigma_1}{t} dt, \quad -\int_{T_2}^{T_1} \frac{\sigma_2}{t} dt \ldots\ldots\ldots -\int_{T_n}^{T_{n-1}} \frac{\sigma_n}{t} dt, \quad -\int_{T_0}^{T_n} \frac{\sigma_1}{t} dt.$$

Hence the general equations (7) and (9) become

$$F = J\left\{\Pi_1 + \Pi_2 + \ldots\ldots + \Pi_n - \int_{T_1}^{T_0} \sigma_1 \, dt - \int_{T_2}^{T_1} \sigma_2 \, dt - \ldots - \int_{T_n}^{T_{n-1}} \sigma_n \, dt - \int_{T_0}^{T_n} \sigma_1 \, dt\right\} \quad . \quad (10)$$

$$\frac{\Pi_1}{T_1} + \frac{\Pi_2}{T_2} + \ldots\ldots + \frac{\Pi_n}{T_n} - \int_{T_1}^{T_0} \frac{\sigma_1}{t} dt - \int_{T_2}^{T_1} \frac{\sigma}{t} dt - \ldots\ldots - \int_{T_n}^{T_{n-1}} \frac{\sigma_n}{t} dt - \int_{T_0}^{T_n} \frac{\sigma_1}{t} dt = 0 \quad . \quad (11)$$

which are the fundamental equations of thermo-electricity in non-crystalline conductors. In these, along with the equation

$$\gamma = \frac{P + F}{J B} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

which shows the strength of the current actually sustained in the conductor when an independent electro-motive force, P, is applied between the principal electrodes E, E′, we have a full expression of the most general circumstances of thermo-electric currents in linear conductors of non-crystalline metals.

114. The special qualities of the metals of a thermo-electric circuit must be investigated experimentally before we can fix the values of Π_1, Π_2, &c., and σ_1, σ_2, &c., for any particular case. The relation between these quantities expressed in the general equation (11), having, as we have seen, a very high degree of probability, not merely as an approximate law, but as an essential truth, may be used as a guide, but must be held provisionally until we have sufficient experimental evidence in its favour. The first fundamental equation (10) admits of no doubt whatever in its universal application, and we shall see (§ 123 below) that it leads to most remarkable conclusions from known experimental facts.

The general principles are most conveniently applied by restricting the number of metals referred to in the general equations to two; a case which we accordingly proceed to consider.

115. Let the principal conductor consist of two metals, one constituting the middle, and the other the two terminal portions. Let the junctions of these portions next the terminals E, E′ be denoted by A, A′ respectively, and let their temperatures be T, T′. Let also $\Pi(T), -\Pi(T')$ be the quantities of heat absorbed at them per second by a current of unit strength. We should have

$$\Pi(T)=\Pi(T'),$$

if the temperatures were equal, since the PELTIER phenomenon consists, as we have seen, of equal quantities of heat evolved or absorbed, according to the direction of a current crossing the junction of two different metals; and if these quantities be not actually equal, we may consider them as particular values of a function Π of the temperature, which depends on the particular relative thermo-electric quality of the two metals. Accordingly, the preceding notation is reduced to $n=2$, $T_1=T$, $T_2=T'$, $\Pi_1=\Pi(T)$, $\Pi_2=-\Pi(T')$; and we have

$$\int_{T_1}^{T_0}\sigma_1 dt+\int_{T_2}^{T_1}\sigma_2 dt+\int_{T_0}^{T_2}\sigma_1 dt=\int_{T}^{T'}(\sigma_1-\sigma_2)dt,$$

and similarly for the integral involving $\frac{1}{t}$. Hence the general equations become

$$F=J\left\{\Pi(T)-\Pi(T')+\int_{T'}^{T}(\sigma_1-\sigma_2)dt\right\} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$\frac{\Pi(T)}{T}-\frac{\Pi(T')}{T'}+\int_{T'}^{T}\frac{\sigma_1-\sigma_2}{t}dt=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

If in the latter equation we substitute t for T, and differentiate with reference to this variable, we have, as an equivalent equation,

$$\frac{d\left(\frac{\Pi}{t}\right)}{dt}+\frac{\sigma_1-\sigma_2}{t}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or

$$\sigma_1-\sigma_2=\frac{\Pi}{t}-\frac{d\Pi}{dt} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

This last equation leads to a remarkably simple expression for the electro-motive force of a thermo-electric pair, solely in the terms of the PELTIER evolution of heat at any temperature intermediate between the temperatures of its junctions; for we have only to eliminate by means of it $(\sigma_1-\sigma_2)$ from (13), to find

$$F=J\int_{T'}^{T}\frac{\Pi}{t}dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

116. Let us first apply these equations to the case of a thermo-electric pair,

with the two junctions kept at temperatures differing by an infinitely small amount τ. In this case we have

$$\Pi(T)-\Pi(T')=\frac{d\,\Pi}{dt}\tau,$$

$$\int_{T'}^{T}(\sigma_1-\sigma_2)\,dt=(\sigma_1-\sigma_2)\,\tau;$$

and equation (13) becomes

$$F=J\left\{\frac{d\,\Pi}{dt}+\sigma_1-\sigma_2\right\}\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

If we make use of (16) in this, we have

$$F=J\,\frac{\Pi}{t}\,\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

The first of these expressions for the electro-motive force involves no hypothesis, but only the general principle of equivalence of heat and work. Its agreement with any experimental results is only to be looked on as a verification of the accuracy of the experiments, and can add nothing to the certainty of the part of the theory from which it is deduced. On the other hand, it would be extremely important to test the second expression (18) by direct experiment, and so confirm or correct the only doubtful part of the theory. The way to do so would be to determine, in absolute measure, the electro-motive force, F, due to a small difference of temperature, τ, in any thermo-electric pair, and to determine, in known thermal units, the amount of the PELTIER effect at a junction of the two metals, with a current of strength measured in electro-dynamic units, as we should then, by these determinations, be able to evaluate from direct experiments the values of the two members separately which appear equated in (18). As yet no observations have been made which lead, directly or indirectly, to the evaluation of the second member of (18) in any case; but I hope before long to succeed in carrying out a plan I have formed for this object. Neither have any observations been made yet, which give in any case a determination of the first member; but they may easily be accomplished by any person who possesses a conductor of which the resistance has been determined in absolute measure. Mr JOULE having kindly put me in possession of the silver wire on which his observations of the electrical generation of heat, in 1845, were made with currents measured by a tangent galvanometer used by him about the same time in experimenting on the electrolysis of sulphate of copper and sulphate of zinc, I hope to be able to complete the test of the theoretical result without difficulty, in any case in which I may succeed in determining the amount of the PELTIER thermal effect.

117. In the mean time, it is interesting to form an estimate, however rough, of the absolute values of the thermo-electric elements, in any case in which observations that have been made afford, directly or indirectly, the requisite data. This I have done for copper and bismuth, and copper and iron, in the manner

shown in the following explanation; which was communicated in full to the Royal Society, when the theory was first brought forward in 1851, although only the part inclosed in double quotation marks was printed in the Proceedings.

Example 1. *Copper and Bismuth.*—"'Failing direct data, the absolute value of the electro-motive force in an element of copper and bismuth, with its two junctions kept at the temperatures 0° and 100° Cent., may be estimated indirectly from POUILLET'S comparison of the strength of the current it sends through a copper wire 20 metres long and 1 millimetre in diameter, with the strength of a current decomposing water at an observed rate; by means of the determinations by WEBER, and others, of the specific resistance of copper and the electro-chemical equivalent of water, in absolute units. The specific resistances of different specimens of copper having been found to differ considerably from one another, it is impossible, without experiments on the individual wire used by M. POUILLET, to determine with much accuracy the absolute resistance of his circuit; but the author has estimated it on the hypothesis that the specific resistance of its substance is $2\frac{1}{4}$ British units. Taking ·02 as the electro-chemical equivalent of water in British absolute units, the author has thus found 16,300 as the electro-motive force of an element of copper and bismuth, with the two junctions at 0° and 100° respectively. About 154 of such elements would be required to produce the same electro-motive force as a single cell of DANIELL'S—if, in DANIELL'S battery, the whole chemical action were electrically efficient.* A battery of 1000 copper and bismuth elements, with the two sets of junctions at 0° and 100° Cent., employed to work a galvanic engine, if the resistance in the whole circuit be equivalent to that of a copper wire of about 100 feet long and about one-eighth of an inch in diameter, and if the engine be allowed to move at such a rate as by inductive reaction to diminish the strength of the current to the half of what it is when the engine is at rest, would produce mechanical effect at the rate of about one-fifth of a horse-power. The electro-motive force of a copper and bismuth element, with its two junctions at 0° and 1°, being found by POUILLET to be about $\frac{1}{100}$th of the electro-motive force when the junctions are at 0° and 100, must be about 163. The value of Θ_0'" [*i. e.*, in terms of the notation now used, Π (273·7), or the value of Π (t), for the freezing point] "'for copper and bismuth, or the quantity of heat absorbed in a second of time by a current of unit strength in

* M. JULES REGNAULD has since found experimentally, that 165 copper-bismuth elements balance the electro-motive force of a single cell of DANIELL'S (See Comptes Rendus, Jan. 9, 1854, or Bibliothéque Univ. de Genève, March 1854), a result agreeing with the estimate quoted in the text, more closely than the uncertainty and indirectness of the data on which that estimate was founded would have justified us in expecting. The comparison of course affords no test of the thermo-electric theory; and only shows that, as far as the observations of WEBER, and others alluded to, render POUILLET'S available for determining the absolute electro-motive force of a copper-bismuth element, the absolute electro-motive force of a single cell of DANIELL'S, obtained by multiplying it by the number found by M. REGNAULD, agrees with that which I first gave on the hypothesis of all the chemical action being electrically efficient (Phil. Mag., Dec. 1851), and so confirms this hypothesis.

passing from bismuth to copper, when the temperature is kept at 0° Cent. must therefore be $\frac{163}{160\cdot16}$, or very nearly equal to the quantity required to raise the temperature of a grain of water from 0° to 1° Cent.'"

119. Example 2. *Copper and Iron.*—"By directing the electro-motive force of one copper and bismuth element against that of a thermo-electric battery of a variable number of copper and iron wire elements in one circuit, I have found, by a galvanometer included in the same circuit, that when the range of temperature in all the thermo-electric elements is the same, and not very far at either limit from the freezing point of water, the current passes in the direction of the copper-bismuth agency when only three, and in the contrary direction when four or more, of the copper-iron elements are opposed to it. Hence the electro-motive force of a copper-bismuth element is between three and four times that of a copper-iron element with the same range of temperature, a little above the freezing point of water. The electro-motive force of a copper-iron element, with its two junctions at 0° and 1° Cent. respectively, must therefore be something greater than one-fourth of the number found above for copper-bismuth with the same range of temperature, that is, something more than 40 British absolute units, and we may consequently represent it by $m \times 40$, where $m > 1$. We have then by the equation expressing the application of CARNOT's principle, [equation (19) of § 116.],

$$\Theta_0 \mu = \Theta_0 \frac{J}{273{\cdot}7} = m \times 40,$$

whence* $$\Theta_0 = \tfrac{1}{4} m \text{ nearly} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a).$$

"Now, by the principle of mechanical effect, we have

$$F_0^{280} = J \left(\int_0^{280} \vartheta \, dt - \Theta_0 \right);$$

if F_0^{280} denote the electro-motive force of a copper-iron element, of which the two junctions are respectively 0° and 280° Cent., and $\vartheta \, dt$, the quantity of heat absorbed per second by a current of unit strength, in passing in copper from a locality at temperature t to a locality at $t+dt$, and in iron from a locality at $t+dt$ to a locality at t;† since the PELTIER generation of heat between copper and iron at their neutral point, 280°, vanishes;‡ and therefore the only absorption of heat is that due to the electric convection expressed by $\int \vartheta \, dt$; while there is evolution of

* The value of J now used being $32{\cdot}2 \times 1390 = 44{,}758$, which is the equivalent of the unit of heat in "absolute units" of work. The "absolute unit of force" on which this unit of work is founded, and which is generally used in magnetic and electro-magnetic expressions, is the force which acting on the unit of matter (one grain) during the unit of time (one second), generates a unit of velocity (one foot per second). The "absolute unit of work" is the work done by the absolute unit of force in acting through the unit of space (one foot).

† That is, if ϑ denote the algebraic excess of the specific heat of electricity in copper, above the specific heat of electricity in iron, according to the terms more recently introduced.

‡ See § 123, below. Instead of 240°, conjectured from REGNAULT's observation when these details were first published, 280° is now taken as a closer approximation to the neutral point of copper and iron.

heat amounting to Θ_0 at the cold junction, and of mechanical effect by the current amounting to F units of work. If we estimate the value of F_0^{280} as half what it would be were the electro-motive force the same for all equal differences of temperature as for small differences near the freezing point,* that is, if we take $F_0^{280} = \frac{1}{2} \times 40m \times 280$, the preceding equation becomes

$$140 \times m \times 40 = J\left(\int_0^{280} \vartheta \, dt - \Theta_0\right).$$

But we found $$m \times 40 = \mu \Theta_0.$$

Hence $$\int_0^{280} \vartheta \, dt = \Theta_0 \left(1 + \frac{140\,\mu}{J}\right) = \Theta_0 \left(1 + \frac{140}{272{\cdot}7}\right) = \Theta_0 \times \frac{3}{2} \text{ nearly} \quad . \quad . \quad . (b);$$

or, according to (*a*),

$$\int_0^{280} \vartheta \, dt = m \times \frac{3}{8} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c);$$

results, of which (*b*) shows how the difference of the aggregate amount of the theoretically indicated convective effect in the two metals is related to the PELTIER effect at the cold junction; and (*c*) shows that its absolute value is rather more than one-third of a thermal unit per second per unit strength of current.

120. If the specific heats of current electricity either vanished or were equal in the different metals, we should have, by (15) and (16),

$$\frac{\Pi}{t} = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad (20),$$

and $$F = J \frac{\Pi}{t} (T - T') \quad . \quad . \quad . \quad . \quad . \quad (21),$$

or, the PELTIER thermal effect at a junction of two metals would be proportional to the absolute temperature at which it takes place, and the electro-motive force in a circuit of any two metals would vary in the simple ratio of the difference of temperature on the new absolute scale between their junctions.† Whatever thermometric system be followed, the second of these conclusions would require the same law of variation of electro-motive force with the temperatures of the junctions, in every pair of metals used as a thermo-electric element.

121. Before the existence of a convective effect of electricity in an unequally heated metal had even been conjectured, I arrived at the preceding conclusions by a theory in which the PELTIER effect was taken as the only thermal effect reversible with the current in a thermo-electric circuit, and found them at variance with

* See § 122, below.

† When the Theory was first communicated to the Royal Society, I stated these conclusions with reference to temperature by the air thermometer, and therefore in terms of CARNOT's absolute function of the temperature, not simply as now in terms of absolute temperature. At the same time, I gave as consequences of MAYER's hypothesis, the same statement in terms of air thermometer temperatures, as is now made absolutely. See Proceedings, Dec. 15, 1851; or Philosophical Magazine, June 1852, p. 532.

known facts which show remarkably different laws of electro-motive force in thermo-electric pairs of different metals. I therefore inferred, that besides the PELTIER effect there must be other reversible thermal effects; and I showed that these can be due to no other cause than the inequalities of temperature in single metals in the circuit. A convective effect of electricity in an unequally heated conductor of one metal was thus first demonstrated by theoretical reasoning; but only the difference of the amount of this effect produced by currents of equal strength in different metals, not its quality or its absolute value in any one metal, could be inferred from the data of thermo-electric force alone. The case of a thermo-electric circuit of copper and iron, being that which first forced on me the conclusion that an electric current must produce different effects according as it passes from hot to cold, or from cold to hot, in an unequally heated metal, was taken as an example in my first communication of the Theory to this Society;* and the two metals, copper and iron, were made the subjects of a consequent experimental investigation, to ascertain the quality of the anticipated property in each of them separately. The application of the general reasoning to this particular case, and the answers which I have derived by experiment to the question which it raises, are described in the following extract of a Report communicated to the Royal Society of London, March 31, and published in the Proceedings, May, of the present year :—

122. "BECQUEREL discovered that if one junction of copper and iron, in a circuit of the two metals, be kept at an ordinary atmospheric temperature, while the other is raised gradually, a current first sets from copper to iron through the hot junction, increasing in strength only as long as the temperature is below about 300° Cent.; and becoming feebler with farther elevation of temperature until it ceases, and a current actually sets in the contrary direction when a high red heat is attained.† Many experimenters have professed themselves unable to verify this extraordinary discovery; but the description which M. BECQUEREL gives of his experiments leaves no room for the doubts which some have thrown upon his conclusion, and establishes the thermo-electric inversion between iron and copper, not as a singular case (extraordinary and unexpected as it appeared), but as a phenomenon to be looked for between any two metals, when tried through a sufficient range of temperatures. M. REGNAULT has verified M. BECQUEREL'S conclusion so far, in finding that the strength of a current in a circuit of copper and iron wire did not increase sensibly for elevations of temperature above 240° Cent., and began to diminish when the temperature considerably exceeded this limit;

* See Proceedings, R. S. E., Dec. 15, 1851.

† Since this was written, I have found that thermo-electric inversions between copper and an alloy of antimony and bismuth, and between silver and the same alloy, precisely analogous to that between copper and iron more recently discovered by M. BECQUEREL, were discovered as early as 1823, by Professor CUMMING of Cambridge, shortly after the thermo-electricity of metals was first brought to light by SEEBECK. These, with other experiments, leading to important results, especially as to the order of metals and metallic compounds in the thermo-electric series, are described in the Cambridge Transactions for 1823, and in Professor CUMMING'S Treatise on Electro-dynamics.

but the actual inversion observed by M. BECQUEREL is required to show that the diminution of strength in the current is due to a real falling off in the electromotive force, and not to the increased resistance known to be produced by an elevation of temperature.

123. "From BECQUEREL'S discovery it follows that, for temperatures below a certain limit, which, for particular specimens of copper and iron wire, I have ascertained, by a mode of experimenting described below, to be 280° Cent., copper is on the negative side of iron in the thermo-electric series; on the positive side for higher temperatures; and at the limiting temperature these two metals are thermo-electrically neutral to one another. It follows, according to the general mechanical theory* of thermo-electric currents, referred to above, that electricity passing from copper to iron causes the absorption or the evolution of heat according as the temperature of the metals is below or above the neutral point; but neither absorption nor evolution of heat, if the temperature be precisely that of neutrality; (a conclusion which I have already partially verified by experiment). Hence, if in a circuit of copper and iron, one junction be kept about 280°, that is, at the neutral temperature, and the other at any lower temperature, a thermo-electric current will set from copper to iron through the hot, and from iron to copper through the cold, junction; causing the evolution of heat in the latter, and the raising of weights, too, if it be employed to work an electro-magnetic engine, but not causing the absorption of any heat at the hot junction. Hence there must be an absorption of heat at some part or parts of the circuit consisting solely of one metal or of the other, to an amount equivalent to the heat evolved at the cold junction, together with the thermal value of any mechanical effects produced on other parts of the circuit. The locality of this absorption can only be where the temperatures of the single metals are non-uniform, since the thermal effect of a current in any homogeneous uniformly-heated conductor is always an evolution of

* This is the only part of the theoretical reasoning as first given, which depended on the application of CARNOT'S principle, and consequently, is the only part capable of being objected to as uncertain. All doubt would be removed by an experimental verification of the stated PELTIER effects for copper and iron, at the different temperatures, such as I hope very soon to have completed. In the meantime, instead of the theoretical reasoning, we may, if it is preferred, use an ample foundation of analogy to conclude that heat is absorbed at the hotter junction, and evolved at the colder, by the actual thermo-electric current in every case of a circuit of two metals, with their junctions differing but little in temperature. For it was found by PELTIER himself, that currents from bismuth to copper, from copper to antimony, from zinc to iron, from copper to iron, and from platinum to iron, cause absorption, and the reverse current in each case, evolution of heat; experimental conclusions, with which I was not acquainted when I first published the Theory. Very soon after I found, myself, by experiment, that copper and iron at ordinary atmospheric temperatures, exhibit the anticipated thermal phenomenon; and corresponding experimental results have been obtained still more recently in the cases of bismuth and copper, copper and antimony, copper and iron, German silver and iron, by FRANKENHEIM. (POGGENDORFF'S *Annalen*, Feb. 1854); in every case, the current which would be produced by heating one junction a little, being that which in the same junction causes an absorption of heat. If we consider the induction sufficient to establish this as a universal law in thermo-electricity, the reasoning in the text becomes independent of any hypothesis to which objections can possibly be raised.

heat. Hence there must be on the whole absorption of heat caused by the current passing from cold to hot in copper, and from hot to cold in iron. When a current is forced through the circuit against the thermo-electric force, the same reasoning establishes an evolution of heat to an amount equivalent to the sum of the heat that would be then taken in at the cold junction, and the value in heat of the energy spent by the agency (chemical, or of any other kind) by which the electromotive force is applied. The aggregate reversible thermal effect, thus demonstrated to exist in the unequally-heated portions of the two metals, might be produced in one of the metals alone, or (as appears more natural to suppose) it may be the sum or difference of effects experienced by each. Adopting, as a matter of form, the latter supposition, without excluding the former possibility, we may assert that either there is absorption of heat by the current passing from hot to cold in the copper, and evolution to a less extent, in the iron of the same circuit; or there is absorption of heat produced by the current from hot to cold in the iron, and evolution of heat to a less amount in the copper; or there must be absorption of heat in each metal: with the reverse effect in each case, when the current is reversed. The reversible effect in a single metal of non-uniform temperature may be called a convection of heat; and, to avoid circumlocution, I shall express it, that the vitreous electricity carries heat with it, or that the specific heat of vitreous electricity is positive, when this convection is in the nominal 'direction of the current;' and I shall apply the same expressions to 'resinous electricity,' when the convection is against the nominal direction of the current. It is established, then, that one or other of the following three hypotheses must be true:—

124. "Vitreous electricity carries heat with it in an unequally heated conductor, whether of copper or iron; but more in copper than in iron:

"Or, resinous electricity carries heat with it in an unequally heated conductor, whether of copper or iron; but more in iron than in copper:

"Or, vitreous electricity carries heat with it in an unequally heated conductor of copper, and resinous electricity carries heat with it in an unequally heated conductor of iron.

125. "Immediately after communicating this theory to the Royal Society of Edinburgh, I commenced trying to ascertain by experiment which of the three hypotheses is the truth, as Theory, with only thermo-electric data, could not decide between them. I had a slight bias in favour of the first rather than the second, in consequence of the positiveness which, after Franklin, we habitually attribute to the vitreous electricity, and a very strong feeling of the improbability of the third. With the able and persevering exertions of my assistant, Mr M'Farlane, applied to the construction of various forms of apparatus, and to assist me in conducting experiments, the research has been carried on with little intermission for more than two years. Mr Robert Davidson, Mr Charles A. Smith, and

other friends, have also given much valuable assistance during a great part of this time, in the different experimental investigations, of which the results are now laid before the Royal Society.

126. "Only nugatory results were obtained until recently, from multiplied and varied experiments both on copper and iron conductors; but the theoretical anticipation was of such a nature, that no want of experimental evidence could influence my conviction of its truth. About four months ago, by means of a new form of apparatus, I ascertained that *resinous electricity carries heat with it in an unequally heated iron conductor*. A similar equally sensitive arrangement showed no result for copper. The second hypothesis might then have been expected to hold; but to ascertain the truth with certainty, I have continued ever since getting an experiment on copper nearly every week, with more and more sensitive arrangements; and at last, in two experiments, I have made out with certainty, that *vitreous electricity carries heat with it in an unequally heated copper conductor*.

"The third hypothesis is thus established; a most unexpected conclusion, I am willing to confess.

"I intend to continue the research; and hope not only to ascertain the nature of the thermal effects in other metals, but to determine its amount in absolute measure in the most important cases, and to find how it varies, if at all, with the temperature; that is, to determine the character (positive or negative) and the value of the specific heat, (varying or not with the temperature,) of the unit of current electricity in various metals."

127. The relations

$$\sigma_1 - \sigma_2 = \frac{\Pi}{t} - \frac{d\Pi}{dt} \quad . \quad . \quad . \quad (16) \qquad \text{and } F = J\int_{T'}^{T} \frac{\Pi}{t} dt \quad . \quad . \quad . \quad . \quad . \quad (17)$$

established above, "show how important it is towards the special object of determining the specific heats of electricity in metals, to investigate the law of electro-motive force in various cases, and to determine the thermal effect of electricity in passing from one metal to another at various temperatures. Both of these objects of research are therefore included in the general investigation of the subject.

128. "The only progress I have as yet made in the last-mentioned branch of the inquiry, has been to demonstrate experimentally, that there is a cooling or heating effect produced by a current between copper and iron, at an ordinary atmospheric temperature, according as it passes from copper to iron, or from iron to copper, in verification of a theoretical conclusion mentioned above; but I intend shortly to extend the verification of theory to a demonstration, that reverse effects take place between those metals, at any temperature above their neutral point of about 280° Cent.; and I hope also to be able to make determinations in absolute

measure of the amount of the PELTIER effect for a given strength of current between various pairs of metals.

129. "With reference to laws of electro-motive force in various cases, I have commenced by determining the order of several specimens of metals in the thermo-electric series, and have ascertained some very curious facts regarding varieties in this series, which exist at different temperatures. In this I have only followed BECQUEREL'S remarkable discovery, from which I had been led to the reasoning and experimental investigation regarding copper and iron described above. My way of experimenting has been, to raise the temperature first of one junction as far as the circumstances admit, keeping the other cold, and then to raise the temperature of the other gradually, and watch the indications of a galvanometer during the whole process. When an inversion of the current is noticed, the changing temperature is brought back till the galvanometer shows no current; and then (by a process quite analogous to that followed by Mr JOULE, and Dr LYON PLAYFAIR, in ascertaining the temperature at which water is of maximum density), the temperatures of the two junctions are approximated, the galvanometer always being kept as near zero as possible. When the difference between any two temperatures on each side of the neutral point which give no current is not very great, their arithmetical mean will be the neutral temperature. A regular deviation of the mean temperature from the true neutral temperature is to be looked for with wide ranges, and a determination of it would show the law according to which the difference of the specific heat of electricity in the two metals varies with the temperatures; but I have not even as yet ascertained with certainty the existence of such a deviation in any particular case. The following is a summary of the principal results I have already obtained in this department of the subject.

130. "The metals tried being—three platinum wires (P_1 the thickest, P_2 the thinnest, and P_3 of intermediate thickness), brass wires (B), a lead wire (L'), slips of sheet-lead (L), copper wires (C), and iron-wire (I); I find that the specimens experimented on stand thermo-electrically, at different temperatures, in the orders shown in the following table, and explained in the heading by reference to bismuth and antimony, or to the terms "negative" and "positive," as often used:—

Temperature Centigrade.	Bismuth "Negative."					Antimony "Positive."
−20°	. . . P_3 . . . c . . .	P_2	P_1	I		
0	. . . P_3 . . . l' . . .	P_2 . . . C . . .	P_1	I		
37	. . . P_3 . . . . . b .	$\{L'P_2\}$ . . . . . C .	P_1	I		
64	. . . P_3	$\overrightarrow{P_2}$ . . . b . l' . .	$\{CP_1\}$	I		
130	. . . P_3	P_2	$\{BP_1\}$. L . . C . .	I		
140	. . . P_3	P_2	P_1 . . $\{BL\}$. C .	I		
280	. . . P_3	P_2	P_1 . . . . b . .	$\{CI\}$		
300	. . . P_3	P_2	P_1 . . . . . b .	I . . . C . .		

It must be added, by way of explanation, that the bracket enclosing the symbols of any two of the metallic specimens indicates that they are neutral to one another at the corresponding temperature; and the arrow-head below one of them shows the direction in which it is changing its place with reference to the other, in the series, as the temperature is raised. When there is any doubt as to a position as shown in the table, the symbol of the metal is a small letter instead of a capital.

131. "The rapidity with which copper changes its place among some of the metals (the platinums and iron) is very remarkable. Brass also changes its place in the same direction, possibly no less rapidly than copper; and lead changes its place also in the same direction, but certainly less rapidly than brass, which, after passing the thick platinum wire P_1 at 130° Cent., passes the lead at 140°, the lead itself having probably passed the thick platinum at some temperature a little below 130° [at 121, as I afterwards found]. The conclusion, as regards specific heats of electricity in the different metals, from the equation expressing thermo-electric force given above, is,—that the specific heat of vitreous electricity is greater in each metal passing another from left to right in the series, as the temperature rises, than in the metal it passes; thus in particular —

132. "*The specific heat of vitreous electricity is greater in copper than in platinum or in iron; greater in brass than in platinum or in lead; and greater in lead than in platinum.*

133. "It is probable enough, from the results regarding iron and copper mentioned above, that the specific heat of vitreous electricity is positive in brass; and very small positive or else negative in platinum, perhaps about the same as in iron. It will not be difficult to test these speculations either by direct experiments on the convective effects of electric currents in the different metals, or by comparative measurements of thermo-electric forces for various temperatures in circuits of the metals, and I trust to be able to do so before long."

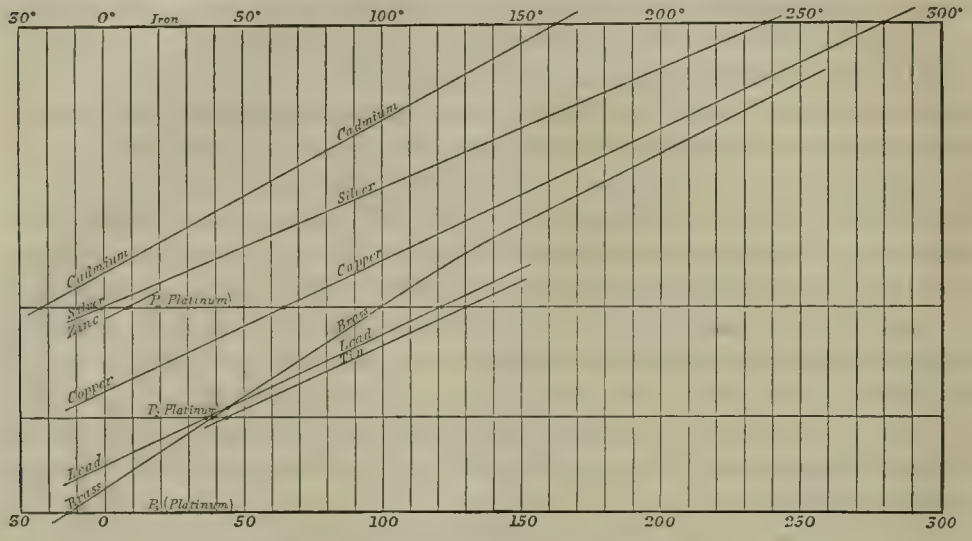
§§ 134, 135. *Inserted September* 15, 1854.

134. A continuation of the experiments has shown many remarkable variations of order in the thermo-electric series. The following table exhibits the results of observations to determine neutral points for different pairs of metals: the number at the head of each column being the temperature centigrade at which the two metals written below it are thermo-electrically neutral to one another; and the lower metal in each column being that which *passes the other from bismuth towards antimony*, as the temperature rises.

−14° Cent.	−12·°2	−1·°5	8·°2	36°	38°	44°	44°	64°	99°	121°	130°	162·°5	237°	280°
P_3	P_1	P_1	P_1	P_2	P_2	P_2 ·	Lead	P_1	P_1	P_1	P_1	Iron	Iron	Iron
Brass.	Cadmium.	Silver.	Zinc.	Lead.	Brass.	Tin.	Brass.	Copper	Brass.	Lead.	Tin.	Cadmium.	Silver.	Copper

I also found that brass becomes neutral to copper, and copper becomes neutral to silver, at some high temperatures, estimated at from 800° to 1400° Cent., in the

former case, and from 700° to 1000° in the latter, being a little below the melting point of silver. The following diagram exhibits the results graphically, constructed on the principle of drawing a line through the letters corresponding to any one of the metallic specimens in a table such as that of § 130, and arranging the spaces so that each line shall be as nearly straight as possible, if not exactly so.



Explanation of Thermo-electric Diagram.

The orders of the metals in the thermo-electric series, at different temperatures, are shown by the points in which the vertical lines through the numbers expressed by the temperatures centigrade are cut by the horizontal and oblique lines named for the different metallic specimens.

The object to be aimed at in perfecting a thermo-electric diagram, and perhaps approximately attained to (conjecturally) in the preceding, is to make the ordinates of the lines (which will, in general, be curves) corresponding to the different metallic specimens, be exactly proportional to their *thermo-electric powers*,* with reference to a standard metal (P_3 in the actual diagram).

135. Judging by the eye from the diagram, as regards the convective agency of electricity in unequally heated conductors, I infer that the different metals are probably to be ranked as follows, in order of the values of the specific heat of electricity in them.

Specific Heat of Vitreous Electricity:—

In	Cadmium, ...	Positive.
„	Brass, ...	...
„	Copper, ...	...
„	Lead, Tin, Silver, ...	Positive, zero, or negative.
„	Platinum, ...	Probably negative.
„	Iron, ...	Negative.

Zinc probably stands high, certainly above platinum.

* See § 140, below.

136. A very close analogy subsists between the thermo-dynamical circumstances of an electrical current in a circuit of two metals, and those of a fluid circulating in a closed rectangular tube, consisting of two vertical branches connected by two horizontal branches. Thus if, by the application of electro-motive force in one case, or by the action of pistons in the other, a current be instituted, and if, at the same time, the temperature be kept uniform throughout the circuit, heat will be evolved and absorbed at the two junctions respectively in the former case, and heat will be evolved in one and absorbed in the other of vertical branches of the tube in the latter case, in consequence of the variations of pressure experienced by the fluid in moving through those parts of the circuit. If the temperature of one junction of the electrical circuit be raised above that of the other, and if the temperature of one vertical branch of the tube containing fluid be raised above that of the other, a current will in each case be occasioned, without any other motive appliance. If the current be directed to do work with all its energy, by means of an engine in each case, there will be a conversion of heat into mechanical effect, with perfectly analogous relations as to absorption and evolution of heat in different parts of the circuit, provided the engine worked by the fluid current be arranged to pass the fluid through it without variation of temperature from or to either of the vertical branches of the tube. If σ_1 and σ_2 denote the specific heat of unity of mass of the fluid, under the constant pressures at which it exists in the lower and upper horizontal branches of the tube in the second case; $\Pi(T)$, $\Pi(T')$ the quantities of heat evolved and absorbed respectively by the passage of a unit mass of fluid through the two vertical branches kept at the respective temperatures T, T'; and if F denote the work done by a unit mass of the fluid in passing through the engine; the fundamental equations obtained above, with reference to the thermo-electric circumstances, may be at once written down for the case of the ordinary fluid, as the expression of the two fundamental laws of the Dynamical Theory of Heat, both of which are applicable to this case, without any uncertainty such as that shown to be conceivable as regards the application of the second law to the case of a thermo-electric current. The two equations thus obtained are equivalent to the two general equations given, in §§ 20 and 21 of the First Part of this series of papers, as the expressions of the fundamental laws of the Dynamical Theory of Heat applied to the elasticity and expansive properties of fluids. In fact, when we suppose the ranges of both temperature and pressure in the circulating fluid to be infinitely small, the equation $F = J\int_{T'}^{T} \frac{\Pi}{t}\,dt$, reduced to the notation formerly used, and modified by changing the independent variables from (t, p) to (t, v), becomes

$$M = \frac{t}{J}\frac{dp}{dt},$$

which is the same as (3) of § 21; and a combination of this with $\frac{d}{dt}\left(\frac{\Pi}{t}\right)=\frac{\sigma_2-\sigma_1}{t}$, gives

$$\frac{dM}{dt}-\frac{dN}{dv}=\frac{1}{J}\frac{dp}{dt},$$

which is identical with (2) of § 20. It appears, then, that the consideration of the case of fluid motion here brought forward as analogous to thermo-electric currents in non-crystalline linear conductors, is sufficient for establishing the general thermo-dynamical equations of fluids, and consequently the universal relations among specific heats, elasticities, and thermal effects of condensation or rarefaction, derived from them in Part III., are all included in the investigation at present indicated. Not going into the details of this investigation, because the former investigation, which is on the whole more convenient, is fully given in Parts I. and III., I shall merely point out a special application of it to the case of a liquid which has a temperature of maximum density, as for instance water.

137. In the first place, it is to be remarked, that if the two vertical branches be kept at temperatures a little above and below the point of maximum density, no current will be produced; and therefore if T_0 denote this temperature, the equation $F=\int\frac{\Pi}{t}\,dt$ gives $\pi(T_0)=0$. Again, if one of the vertical branches be kept at T_0, and the other be kept at a temperature either higher or lower, a current will set, and always in the same direction. Hence $\int_{T_0}^{T}\frac{\Pi}{t}\,dt$ has the same sign, whether T be greater or less than T_0, and consequently $\pi(t)$ must have contrary signs for values of t above and below T_0: which, by attending to the signs in the general formulæ, we see must be such as to express evolution of heat by the actual current in the second vertical branch, when its temperature is below, and absorption when above, T_0. As the current in each case ascends in this vertical branch, we conclude that a slight diminution of pressure causes evolution or absorption of heat, in water, according as its temperature is below or above that of maximum density; or conversely,—That when water is suddenly compressed, it becomes colder if initially below, or warmer if initially above, its temperature of maximum density. This conclusion from general thermo-dynamic principles was first, so far as I know, mentioned along with the description of an experiment to prove the lowering of the freezing point of water by pressure, communicated to the Royal Society in January, 1850.* The quantitative expression for the effect, which was given in § 50 of Part III., may be derived with ease from the considerations now brought forward. The other thermo-dynamic equation $\frac{\sigma_2-\sigma_1}{t}=\frac{d\left(\frac{\Pi}{t}\right)}{dt}$ shows that the specific heat of the water must be greater in the upper horizontal branch

* See Proceedings of that date, or Philosophical Magazine, 1850.

than in the lower; or that the specific heat of water under constant pressure is increased by a diminution of the pressure. The same conclusion, and the amount of the effect, are also implied in equations (18) and (19) of Part III. We may arrive at it without referring to any of the mathematical formulæ, merely by an application of the general principle of mechanical effect, when once the conclusion regarding the thermal effects of condensation or rarefaction is established; exactly as the conclusion regarding the specific heats of electricity in copper and in iron was first arrived at.* For if we suppose one vertical branch to be kept at the temperature of maximum density (corresponding to the neutral point of the metals in the corresponding thermo-electric case), and the other at some lower temperature, a current will set downwards through the former branch, and upwards through the latter. This current will cause evolution of heat, in consequence of the expansion of the fluid, in the branch through which it rises, but will cause neither absorption nor evolution in the other vertical branch, since in it the temperature is that of the maximum density. There will also be heat generated in various parts by fluid friction. There must then be, on the whole, absorption of heat in the horizontal branches; because otherwise there would be no source of energy for the heat constantly evolved to be drawn from. But heat will be evolved by the fluid in passing in the lower horizontal branch from hot to cold; and therefore, exactly to the extent of the heat otherwise evolved, this must be over-compensated by the heat absorbed in the upper horizontal branch by the fluid passing from cold to hot. On the other hand, if one of the vertical branches be kept above the temperature of maximum density, and the other at this point, the fluid will sink in the latter, causing neither absorption nor evolution of heat, and rise in the former, causing absorption; and therefore more heat must be evolved by the fluid passing from hot to cold in the upper horizontal branch than is absorbed by it in passing from cold to hot in the lower. From either case, we infer that the specific heat of the water is greater in the upper than in the lower branch. The analogy with the thermo-electric circumstances of two metals which have a neutral point, is perfect algebraically in all particulars. The proposition just enunciated corresponds exactly to the conclusion arrived at formerly, that if one metal passes another in the direction from bismuth towards antimony in the thermo-electric scale, the specific heat of electricity is greater in the former metal than in the latter; this statement holding algebraically, even in such a case as that of copper and iron, where the specific heats are of contrary origin in the two metals, although the existence of such contrary effects is enough to show how difficult it is to conceive the physical circumstances of an electric current as physically analogous to those of a current of fluid in one direction.

* Proceedings R. S. E., Dec. 15, 1851, or extract of Proceedings R. S., May 1854, quoted above, § 124.

§§ 138–140. *General Lemma, regarding relative thermo-electric properties of Metals, and multiple combinations in a Linear Circuit.*

138. The general equation (11), investigated above, shows that *the aggregate amount of all the thermal effects produced by a current, or by any system of currents, in any solid conductor or combination of solid conductors must be zero, if all the localities in which they are produced are kept at the same temperature.*

Cor. 1. If in any circuit of solid conductors the temperature be uniform from a point P through all the conducting matter to a point Q, both the aggregate thermal actions, and the electro-motive force are totally independent of this intermediate matter, whether it be homogeneous or heterogeneous, crystalline or non-crystalline, linear or solid, and is the same as if P and Q were put in contact. [The importance of this simple and elementary truth in thermo-electric experiments of various kinds is very obvious. It appears to have been overlooked by many experimenters who have scrupulously avoided introducing extraneous matter (as solder) in making thermo-electric junctions, and who have attempted to explain away Cumming's and Becquerel's remarkable discovery of thermo-electric inversions, by referring the phenomena observed to coatings of oxide formed on the metals at their surfaces of contact.]

Cor. 2. If $\Pi(A, B)$, $\Pi(B, C)$, $\Pi(C, D)$, ... $\Pi(Z, A)$ denote the amounts of the Peltier absorption of heat per unit strength of current per unit of time, at the successive junctions of a circuit of metals A, B, C, ... Z, A, we must have,

$$\Pi(A, B) + \Pi(B, C) + \ldots\ldots\ldots + \Pi(Z, A) = 0.$$

Thus if the circuit consist of three metals,

$$\Pi(A, B) + \Pi(B, C) + \Pi(C, A) = 0;$$

from which, since $\Pi(C, A) = -\Pi(A, C)$, we derive

$$\Pi(B, C) = \Pi(A, C) - \Pi(A, B).$$

139. Now, by (19) above, the electro-motive force in an element of the two metals (A, B), tending from B to A through the hot junction, for an infinitely small difference of temperature τ, and a mean absolute temperature t, is $\frac{J\,\Pi(A, B)}{t}\tau$, and so for every other pair of metals. Hence, if $\phi(A, B)$, $\phi(B, C)$, &c., denote the quantities by which the infinitely small range τ must be multiplied to get the electro-motive forces of elements composed of successive pairs of the metals in the same thermal circumstances, we have

$$\phi(A, B) + \phi(B, C) + \ldots\ldots + \phi(Z, A) = 0;$$

and, for the case of three metals,

$$\phi(B, C) = \phi(A, C) - \phi(A, B).$$

Since the thermo-electric force for any range of temperature is the sum of the thermo-electric forces for all the infinitely small ranges into which we may divide

the whole range (being, as proved above, equal to $\int_{T}^{T'} \phi \, dt$), in the case of each element, the theorem expressed by these equations is true of the thermo-electric forces in the single elements for *all ranges* of temperature, provided the absolute temperatures of the hot and cold junctions be the same in the different elements. The second equation, by successive applications of which the first may be derived, is the simplest expression of a theorem which was, I believe, first pointed out and experimentally verified by BECQUEREL in researches described in the second volume of his Traité d'Electricité.

140. For brevity, we shall call what has been denoted by ϕ (B, C) *the thermo-electric relation* of the metal B to the metal C; we shall call a certain metal (perhaps copper or silver) the standard metal; and if A be the standard metal, we shall call ϕ (A, B) the *thermo-electric power of the metal B.* The theorem expressed by the last equation may now be stated thus: *The thermo-electric relation between two metals is equal to the difference of their thermo-electric powers;* which is nearly identical with BECQUEREL'S own statement of his theorem.

§§ 141–146. *Elementary Explanations in Electro-cinematics and Electro-mechanics.*

141. When we confined our attention to electric currents flowing along linear conductors, it was only necessary to consider in each case, the *whole strength of the current*, and the longitudinal electro-motive force in any part of the circuit, without taking into account any of the transverse dimensions of the conducting channel. In what follows, it will be frequently necessary to consider distributions of currents in various directions through solid conductors, and it is therefore convenient at present to notice some elementary properties, and to define various terms, adapted for specifications of systems of electric currents and electro-motive forces, distributed in any manner whatever throughout a solid.

142. It is to be remarked, in the first place, that any portion of a solid traversed by current electricity may be divided, by tubular surfaces coinciding with lines of electric motion, into an infinite number of channels or conducting arcs, each containing an independent linear current. The *strength* of a linear current being, as before, defined to denote the quantity of electricity flowing across any section in the unit of time, we may now define the *intensity of the current*, at any point of a conductor, as the strength of a linear current of infinitely small transverse dimensions through this point, divided by the area of a normal section of its channel. The elementary proposition of the composition of motions, common to the cinematics of ordinary fluids and of electricity, shows that the superposition of two systems of currents in a body gives a resultant system, of which the intensity and direction at any point are represented by the diagonal of a parallelogram described upon lines representing the intensity and direction of the component systems

respectively. Hence we may define the components, along three lines at right angles to one another, of the intensity of electric current through any point of a body, as the products of the intensity of the current at that point into the cosines of the inclination of its direction to those three lines respectively; and we may regard the specification of a distribution of currents through a body as complete, when the components, parallel to three fixed rectangular axes of reference, of the intensity of the current at every point are specified.

143. The term electro-motive force has been applied, in what precedes, consistently with the ordinary usage, to the whole force urging electricity through a linear conducting arc. When a current is sustained through a conducting arc, by energy proceeding from sources belonging entirely to the remainder of the circuit, the electro-motive force may be considered as applied from without to its extremities; and in all such cases it may be measured—electro-statically, by determining in any way the difference of potential between two conducting bodies, insulated from one another and put in metallic communication with the extremities of the conducting arc;—or electro-dynamically, by applying to these points the extremities of another linear conductor, of infinitely greater resistance (practically, for instance, a long fine wire used as a galvanometer coil), and determining the strength of the current which it conveys when so applied. These tests may, of course, be regarded as giving either the amount of the electro-motive force with which the remainder of the circuit acts on, or the whole of the electro-motive force efficient in, the passive conducting arc first considered. On the other hand, the electro-motive force acting in the portion from which the energy proceeds is not itself determined by such tests, but is equal to the whole electro-motive force of the sources contained in it, diminished by the reaction of the force which is measured in the manner just explained. The same tests applied to any two points whatever of a complete conducting circuit, however the sources of energy are distributed through it, show simply the electro-motive force acting and reacting between the two parts into which the circuit might be separated by breaking it at these points. In some cases, for instance some of thermo-electric action which we shall have to consider, these tests would give a zero indication to whatever two points of a circuit through which a current is actually passing they are applied, and would therefore show that there is no electric action and reaction between different parts of the circuit, but that each part contains intrinsically the electro-motive force required to sustain the current through it at the existing rate. An actual test of the electro-motive force of sources contained in any part of a linear conductor is defined, with especial reference to the circumstances of thermo-electricity, in the following statement:—

144. Def. The actual intrinsic electro-motive force of any part of a linear conducting circuit is the difference of potential which it produces in two insulated conductors of a standard metal at one temperature, when its extremities are

connected with them by conducting arcs of the same metal, and insulated from the remainder of the circuit.

The electro-motive force so defined may be determined either by determining, by some electro-statical method, the difference of potentials in the two conductors of standard metal mentioned in the definition; or by measuring the strength of the current produced in a conducting arc of the standard metal of infinitely greater resistance than the given conducting arc, applied to connect its extremities, when insulated from the remainder of its own circuit.

145. With reference to the distribution of electro-motive force through a solid, the following definitions are laid down:—

Def. 1. The intrinsic electro-motive force of a linear conductor at any point is the actual intrinsic electro-motive force in an infinitely small arc through this point, divided by its length.

Def. 2. The efficient electro-motive force at any point of a linear conducting circuit is the sum of the actual intrinsic electro-motive force in an infinitely small arc, and the electro-motive force produced by the remainder of the circuit on its extremities, divided by its length.

Def. 3. The intrinsic electro-motive force at any point in a solid, in any direction, is the electro-motive force that would be experienced by an infinitely thin conducting arc of standard metal, applied with its extremities to two points in a line with this direction, in an infinitely small portion insulated all round from the rest of the solid, divided by the distance between these points.

Def. 4. The electro-motive force efficient at any point of a solid, in any direction, is the difference of the electro-motive forces that would be experienced by an infinitely thin conducting arc of standard metal, with its extremities applied to two points infinitely near one another in this direction, divided by the distance between the points, in the two cases separately of the solid being left unchanged, and of an infinitely small portion of it containing these points being insulated from the remainder.

146. Principle of the superposition of thermo-electric action. It may be assumed as an axiom, that each of any number of co-existing systems of electric currents produces the same reversible thermal effect in any locality as if it existed alone.

§§ 147–155. *On Thermo-electric Currents in Linear Conductors of Crystalline Substance.*

147. The general characteristic of crystalline matter is that physical agencies, having particular directions in the space through which they act, and depending on particular qualities of the substance occupying that space, take place with different intensities in different directions, if the substance be crystalline. Substances not naturally crystalline may have the crystalline characteristic induced in them by the action of some directional agency, such as mechanical strain or

magnetization; and may be said to be inductively crystalline. Or again, minute fragments of non-crystalline substances may be put together, so as to constitute solids, which, on a large scale, possess the general characteristic of homogeneous crystalline substances; and such bodies may be said to possess the crystalline characteristic by structure, or to be structurally crystalline.

148. As regards thermo-electric currents, the characteristic of crystalline substance must be, that bars cut from it in different directions would, when treated thermo-electrically as linear conductors, be found in different positions in the thermo-electric series; or that two bars cut from different directions in the substance would be thermo-electrically related to one another like different metals. This property has been experimentally demonstrated by Svanberg, for crystals of bismuth and antimony; and there can be no doubt but that other natural metallic crystals will be found to possess it. I have myself observed, that the thermo-electric properties of copper and iron wires are affected by alternate tension and relaxation in such a manner, as to leave no doubt but that a mass of either metal, when compressed or extended in one direction, possesses different thermo-electric relations in different directions. Fragments of different metals may be put together so as to form solids, possessing by structure the thermo-electric characteristic of a crystal, in an infinite variety of ways. Thus, a structure consisting of thin layers alternately of two different metals, possesses obviously the thermo-electric qualities of a crystal with an axis of symmetry. I have investigated the thermo-electric properties in all directions of such a structure, in terms of the conducting powers for heat and electricity, and the thermo-electric powers, of the two metals of which it is composed; and bars made up of alternate layers of copper and iron, one with the layers perpendicular, another with the layers oblique, and a third with the layers parallel, to the length, illustrating the theoretical results, which were communicated along with this paper, were exhibited to the Royal Society. The principal advantage of considering metallic structures with reference to the theory of thermo-electricity is, as will be seen below, that we are so enabled to demonstrate the possibility of crystalline thermo-electric qualities of the most general conceivable type, and are shown how to construct solids (whether or not natural crystals may be ever found) actually possessing them.

149. The following two propositions with reference to thermo-electric effects in a particular case of crystalline matter are premised to the unrestricted treatment of the subject, because they will serve to guide us as to the nature of the agencies for which the general mathematical expressions are to be investigated.

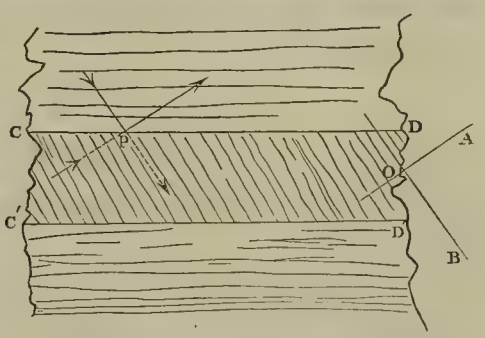
Prop. I. If a bar of crystalline substance, possessing an axis of thermo-electric symmetry, has its length oblique to this axis, a current of electricity sustained in it longitudinally will cause evolution of heat at one side, and absorption of heat at the opposite side, all along the bar, when the whole substance is kept at the same temperature.

PROP. II. If the two sides of such a bar be kept at different temperatures, and a homogeneous conducting arc be applied to points of the ends which are at the same temperature, a current will be produced along the bar, and through the arc completing the circuit.

150. For proving these propositions, it will be convenient to investigate fully the thermo-electric agency experienced by a bar cut obliquely from a crystalline substance possessing an axis of symmetry, when placed longitudinally in a circuit of which the remainder is composed of the standard metal, and kept with either its sides or its ends unequally heated. Let θ and ϕ denote the thermo-electric powers of two bars cut from the given substance in directions parallel and perpendicular to its axis of symmetry respectively. Let us suppose the actual bar to be of rectangular section with two of its opposite sides perpendicular to the plane of its length, and the axis of symmetry of its substance. Let a longitudinal section in this plane be represented by the accompanying diagram; let O A or any line parallel to it be the direction of the axis of symmetry through any point; and let ω denote the inclination of this line to the length of the bar. Let the breadth of the two opposite sides of the bar perpendicular to the plane of the diagram be denoted by a, and in the plane of the diagram, b. The area of the transverse section of the bar will be ab; and therefore if γ denote the strength, and i the intensity of the current in it, we have,—

$$i = \frac{\gamma}{ab}.$$

151. We may suppose the current, itself parallel to the length of the bar and in the direction from left to right of the diagram, to be resolved, at any point P at the side of the bar, into two components in directions parallel and perpendicular to O A, of which the intensities will be $i \cos \omega$, and $i \sin \omega$, respectively. The former of these components may be supposed to belong to a system of currents crossing the bar in lines parallel to O A and passing out of it, across the side C D, into a conductor of the standard metal: and the latter, to a system of currents entering the bar across C D, from the same conductor of standard metal, and crossing it in lines perpendicular to O A. The resultant current in the supposed standard metal beside the bar will clearly be parallel to the length, and can therefore (this metal being non-crystalline) produce no effect influencing the thermal agency at the side of the bar or within it. The inclinations of the currents to a perpendicular to the separating plane of the two metals being respectively $90° - \omega$ and ω, their strength per unit of area of this



plane, obtained by multiplying their intensities by the cosine of those angles respectively, will be each equal to

$$i \cos \omega \sin \omega .$$

Hence the absorptions of heat which they will produce at the surface of separation of the metals per unit of area per second will be,

$$-\frac{1}{\mathrm{J}} i \cos \omega \sin \omega \, t \, \theta , \quad \text{and} \quad \frac{1}{\mathrm{J}} i \cos \omega \sin \omega \, t \, \phi ,$$

respectively. According to the general principle of the superposition of thermo-electric actions stated above, the sum of these is the rate of absorption of heat per unit of surface, when the two systems of currents coexist. But the resultant of these systems is simply the given longitudinal current in the bar, with no flow, either out of it or into it, across any of its sides. Hence, a simple current of intensity i, parallel to the sides of the bar, causes absorption of heat at the side C D, amounting to

$$\frac{1}{\mathrm{J}} i \cos \omega \sin \omega \, t \, (\phi - \theta),$$

per unit of area per second; and the same demonstration shows that an equal amount of evolution must be produced at the opposite side C′ D′. These effects take place quite independently of the matter round the bar, since the metal carrying electric currents which we supposed to exist at the sides of the bar in the course of the demonstration, can exercise no influence on the phenomena.

152. If l denotes the length of the bar, the area of each of the sides perpendicular to the plane of the diagram will be $l \, a$; and therefore, the absorption over the whole of the side C D, and the evolution over the whole of the other side C′ D′, per second will be

$$\frac{1}{\mathrm{J}} i \, l \, a \cos \omega \sin \omega \, t \, (\phi - \theta),$$

or

$$\frac{1}{\mathrm{J}} \gamma \frac{l}{b} \cos \omega \sin \omega \, t \, (\phi - \theta).$$

It is obvious, that there can be neither evolution nor absorption of heat at the two other sides.

153. An investigation, similar to that which has just been completed, shows that if the actual current enter from a conductor of the standard metal at one end of the bar, and leave it by a conductor of the same metal at its other end, the absorption and evolution of heat at these ends respectively will amount to

$$\frac{1}{\mathrm{J}} \gamma \, (t \, \theta \cos^2 \omega + t \phi \sin^2 \omega)$$

per second.

154. Let us now suppose the two sides C D, C′ D′ to be kept at uniform temperatures, T, T′, and the two ends to be kept with equal and similar distributions of temperatures, whether a current is crossing them or not. Then if a current of strength γ be sent through the bar from left to right of the diagram, in a circuit

of which the remainder is the standard metal, there will be reversible thermal action, consisting of the following parts, each stated per unit of time.

(1.) Absorption amounting to $\Omega(\mathrm{T})\frac{l}{b}\gamma$, in a locality at the temperature T.

(2.) Evolution amounting to $\Omega(\mathrm{T}')\frac{l}{b}\gamma$, in a locality at the temperature T',

(3.) Absorption amounting to $\Pi\gamma$ at one end, (that beyond C C',)

and (4.) Evolution amounting to $\Pi\gamma$ at the other end;

where, for brevity, $\Omega(\mathrm{T})$ and $\Omega(\mathrm{T}')$ are assumed to denote the values of $\int_{\mathrm{J}}^{t}(\phi-\theta)\sin\omega\cos\omega$, at the temperatures T and T'; and Π the mean value of $\int_{\mathrm{J}}^{t}(\theta\cos^2\omega+\phi\sin^2\omega)$ for either end of the bar. The contributions towards the sums appearing in the general thermo-dynamic equations which are due to these items of thermal agency, are as follows:—

$$\left[\Omega(\mathrm{T})-\Omega(\mathrm{T}')\right]\frac{l}{b}\gamma \qquad \text{towards } \Sigma\mathrm{H}_t,$$

and

$$\left[\frac{\Omega(\mathrm{T})}{\mathrm{T}}-\frac{\Omega(\mathrm{T}')}{\mathrm{T}'}\right]\frac{l}{b}\gamma \qquad \text{towards } \Sigma\frac{\mathrm{H}_t}{t};$$

the thermal agencies at the ends disappearing from each sum, in consequence of their being mutually equal and opposite, and being similarly distributed through localities equally heated. Now when every reversible thermal effect is included, the value of $\Sigma\frac{\mathrm{H}_t}{t}$ must be zero, according to the second general law. Hence either $\frac{\Omega(\mathrm{T})}{\mathrm{T}}-\frac{\Omega(\mathrm{T}')}{\mathrm{T}'}$ must vanish, or there must be a reversible thermal agency not yet taken into account. But probably $\frac{\Omega(\mathrm{T})}{\mathrm{T}}-\frac{\Omega(\mathrm{T}')}{\mathrm{T}'}$ may not vanish, that is, $\frac{\Omega}{t}$ may vary with the temperature, for natural crystals, and it certainly does vary with the temperature for metallic combinations structurally crystalline: (for a bar cut obliquely from a solid consisting of alternate layers of copper and iron, for instance, the value of Ω decreases to zero, as the temperature is raised from an ordinary atmospheric temperature up to about 280°, and has a contrary sign for higher temperatures.) Hence, in general, there must be another reversible thermal agency, besides the agencies at the ends and at the sides of the bar which we have investigated. This agency must be in the interior; and since the substance is homogeneous, and uniformly affected by the current, the new agency must be uniformly distributed through the length, as different points of the same cross section can only differ in virtue of their different circumstances as to temperature. If there were no variation of temperature, there could be no such effect anywhere in the interior of the bar; and therefore, if dt denote the variation of tempera-

ture in an infinitely small space dx across the bar in the plane of the diagram, and χ an unknown element, constant or a function of the temperature, depending on the nature of the substance, we may assume

$$i\,\chi\frac{dt}{dx}$$

as the amount of absorption, per unit of the volume of the bar, due to a current of intensity i, by means of the new agency. The whole amount in a lamina of thickness dx, length l, and breadth a perpendicular to the plane of the diagram, is therefore

$$i\,\chi\,\frac{dt}{dx}\,a\,l\,dx,$$

or

$$\gamma\,\frac{l}{b}\,\chi\,dt.$$

As there cannot possibly be any other reversible thermal agency to be taken into account, we may now assume

$$\Sigma\,\mathrm{H}_t = \gamma\,\frac{l}{b}\left\{\left[\Omega(\mathrm{T}) - \Omega(\mathrm{T}')\right] + \int_{\mathrm{T}'}^{\mathrm{T}}\chi\,dt\right\} \quad . \quad . \quad (22),$$

$$\Sigma\,\frac{\mathrm{H}_t}{t} = \gamma\,\frac{l}{b}\left\{\frac{\Omega(\mathrm{T})}{\mathrm{T}} - \frac{\Omega(\mathrm{T}')}{\mathrm{T}'} + \int_{\mathrm{T}'}^{\mathrm{T}}\frac{\chi}{t}\,dt\right\} \quad . \quad . \quad . \quad (23).$$

The second General Law showing that $\Sigma\,\frac{\mathrm{H}_t}{t}$ must vanish, gives, by the second of these equations,

$$\frac{\Omega(\mathrm{T})}{\mathrm{T}} - \frac{\Omega(\mathrm{T}')}{\mathrm{T}'} + \int_{\mathrm{T}'}^{\mathrm{T}}\frac{\chi}{t}\,dt = 0 \quad . \quad . \quad . \quad (24).$$

Substituting, in place of T, t, and differentiating with reference to this variable, we have, as an equivalent equation,

$$\frac{\chi}{t} = -\frac{d\,\frac{\Omega}{t}}{dt} - \frac{d\,\frac{\Omega}{t}}{dt} \quad . \quad . \quad . \quad . \quad (25);$$

and using this in (22), we have

$$\Sigma\,\mathrm{H}_t = \gamma\,\frac{l}{b}\int_{\mathrm{T}'}^{\mathrm{T}}\frac{\Omega}{t}\,dt \quad . \quad . \quad . \quad . \quad . \quad (26).$$

This expresses the full amount of heat taken in through the agency of the current γ; of which the mechanical equivalent is therefore the work done by the current. Hence (according to principles fully explained above) the thermal circumstances actually cause an electro-motive force F, of which the amount is given by the equation

$$\mathrm{F} = \mathrm{J}\,\frac{l}{b}\int_{\mathrm{T}'}^{\mathrm{T}}\frac{\Omega}{t}\,dt \quad . \quad . \quad . \quad . \quad . \quad (27),$$

to act along the bar from left to right of the diagram; which will produce a current, unless balanced by an equal and contrary reaction. This result both esta-

blishes Proposition II., enunciated above in § 149, and shows the amount of the electro-motive force producing the stated effect, in terms of T and T′, the temperatures of the two sides of the bar, the obliquity of the bar to the crystalline axis of symmetry, and the thermo-electric properties of the substance; since, if θ and ϕ denote its thermo-electric powers, along the axis of symmetry, and along lines perpendicular to this axis, at the temperature t, and ω the inclination of this axis to the length of the bar when the substance is at the temperature t, we have

$$\Omega = \frac{t}{J}(\phi - \theta) \sin\omega \cos\omega \quad . \quad . \quad . \quad . \quad . \quad (28).$$

155. By an investigation exactly similar to that of § 115 which had reference to non-crystalline linear conductors, we deduce the following expression for the electro-motive force, when the ends of the bar are kept at temperatures T, T′, from the terminal thermal agency Π, of a current investigated in § 153.

$$F = J\int_{T'}^{T} \frac{\Pi}{t}\, dt \quad . \quad . \quad . \quad . \quad . \quad (29),$$

where

$$\Pi = \frac{t}{J}(\theta \cos^2\omega + \phi \sin^2\omega) \quad . \quad . \quad . \quad . \quad (30).$$

§§ 156–170. *On the Thermal Effects and the Thermo-electric Excitation of Electrical Currents in Homogeneous Crystalline Solids.*

156. The Propositions I. and II., investigated above, suggest the kind of assumptions to be made regarding the reversible thermal effects of currents in uniformly heated crystalline solids, and the electro-motive forces induced by any thermal circumstances which cause inequalities of temperature in different parts. The formulæ expressing these agencies in the particular case which we have now investigated, guide us to the precise forms required to express those assumptions in the most general possible manner.

157. Let us first suppose a rectangular parallelepiped (a, b, c) of homogeneous crystalline conducting matter, completely surrounded by continuous metal of the standard thermo-electric quality touching it on all sides, to be traversed in any direction by a uniform electric current, of which the intensity components parallel to the three edges of the parallelepiped are h, i, j, and to be kept in all points at a uniform temperature t. Then taking ϕ, θ, ψ, to denote the thermo-electric powers of bars of the substance cut from directions parallel to the edges of the parallelepiped, quantities which would be equal to one another in whatever directions those edges are if the substance were non-crystalline; and θ' θ'', ϕ', ϕ'', ψ', ψ'', other elements depending on the nature of the substance with reference to the directions of the sides of the parallelepiped, to which the name of thermo-electric obliquities may be given, and which must vanish for every system of rectangular

planes through the substance, if it be non-crystalline; we may assume the following expression for the reversible thermal effects of the current:—

$$\left.\begin{aligned} Q_{(b\,c)} &= b\,c\,\frac{t}{J}\,(h\,\theta + i\,\phi'' + j\,\psi') \\ Q_{(c\,a)} &= c\,a\,\frac{t}{J}\,(h\,\theta' + i\,\phi + j\,\psi'') \\ Q_{(a\,b)} &= a\,b\,\frac{t}{J}\,(h\,\theta'' + i\,\phi' + j\,\psi) \end{aligned}\right\} \quad . \quad . \quad (31),$$

where $Q_{(b\,c)}$, $Q_{(c\,a)}$, $Q_{(a\,b)}$, denote quantities of heat absorbed per second at the sides by which positive current components enter, and quantities evolved in the same time at the opposite sides. Hence, if the opposite sides be kept at different temperatures, currents will pass, unless prevented by the resistance of surrounding matter; and the electro-motive forces by which these currents are urged, in directions parallel to the three edges of the parallelepiped, have the following expressions, in which $u\,a$, $v\,b$, and $w\,c$ denote the difference of temperature between corresponding points in the pairs of sides $b\,c$, $c\,a$, and $a\,b$, respectively reckoned positive, when the temperature increases in the direction of positive components of current;

$$\left.\begin{aligned} E &= -a\,(u\,\theta + v\,\theta' + w\,\theta'') \\ F &= -b\,(u\,\phi'' + v\,\phi + w\,\phi') \\ G &= -c\,(u\,\psi' + v\,\psi'' + w\,\psi) \end{aligned}\right\} \quad . \quad . \quad . \quad (32).$$

The negative signs are prefixed, in order that positive values of the electro-motive components may correspond to forces in the direction assumed for positive components of current.

158. The most general conceivable elementary type of crystalline thermo-electric properties is expressed in the last equations, along with the equations (31) by which we arrived at them, and we shall see that every possible case of thermo-electric action in solids of whatever kind may be investigated by using them with values, and variations it may be, of the coefficients ϕ, θ, &c., suitable to the circumstances. It might be doubted, indeed, whether these nine coefficients can be perfectly independent of one another; and indeed it might appear very probable that they are essentially reducible to six independent coefficients, from the extraordinary nature of certain conclusions which we shall show can only be obviated by supposing

$$\theta' = \phi'', \quad \theta'' = \psi', \quad \text{and} \quad \phi' = \psi''.$$

Before going on to investigate any consequences from the unrestricted fundamental equations, I shall prove that it is worth while to do so, by demonstrating that a metallic structure may be actually made, which, when treated on a large scale as a continuous solid, according to the electric and thermal condi-

tions specified for the substance with reference to which the equations (31) and (32) have been applied, shall exhibit the precise electric and thermal properties respectively expressed by those sets of equations with nine arbitrarily prescribed values for the coefficients θ, ϕ, &c.

159. Let two zigzag linear conductors of equal dimensions, each consisting of infinitely short equal lengths of infinitely fine straight wire alternately of two different metals, forming right angles at the successive junctions, be placed in perpendicular planes, and touching one another at any point, but with a common straight line joining the points of bisection of the small straight parts of each

conductor. Let an insulating substance be moulded round them, so as to form a solid bar of square section, just containing the two zigzags imbedded in it in planes parallel to its sides. Although this substance is a non-conductor of electricity, we may suppose it to have enough of conducting power for heat, or the wires of the electric conductors to be fine enough, that the conduction of heat through the bar when it is unequally heated may be sensibly the same as if its substance were homogeneous throughout, and, consequently, that the electric conductors take at every point the temperatures which the bar would have at the same point if they were removed. Let an infinite number of such bars, equal and similar, and of the same substance, be constructed; and let a second system of equal and similar bars be constructed with zigzag conductors of different metals from the former; and a third with other different metals: the sole condition imposed on the different zigzag conductors being that the two in each bar, and those in the bars of different systems, exercise the same resistance against electric conduction. Let an infinite number of bars of the first set be laid on a plane, parallel to one another, with intervals between every two in order, equal to the breadth of each. Lay perpendicularly across them an infinite number of bars of the second system similarly disposed relatively to one another; place on these again bars of the first system, constituting another layer similar and parallel to the first; on this, again, a layer similar and parallel to the second; and so on, till the thickness of the superimposed layers is equal to the length of each bar. Then let an infinite number of the bars of the third system be taken and pushed into the square prismatic apertures perpendicular to the plane of the layers; the cubical hollows which are left (not visible in the

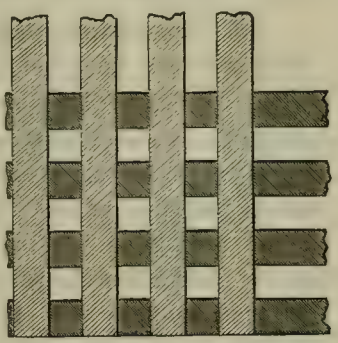


diagram) being previously filled up with insulating matter, such as that used in the composition of the bars. Let the complex solid cube thus formed be coated round its sides with infinitely thin connected sheets of the standard metal, so thin that the resistance to the conduction of electricity along them is infinitely great, compared to the resistance to conduction experienced by a current traversing the interior of the cube by the zigzag linear conductors imbedded in it. (For instance, we may suppose the resistance of four parallel sides of the cube to be as great as, or greater than, the resistance of each one of the zigzag linear conductors.) Let an infinite number of such cubes be built together, with their structural directions preserved parallel, so as to form a solid, which, taken on a large scale, shall be homogeneous. A rectangular parallelepiped, abc, of such a solid, with its sides parallel to the sides of the elementary cubes, will present exactly the thermo-electric phenomena expressed above by the equations (31) and (32) provided the thermo-electric powers ϖ_1, ϖ_1', ϖ_1'', ϖ_1''', ϖ_2, ϖ_2', ϖ_2'', ϖ_2''', and ϖ_3, ϖ_3', ϖ_3'', ϖ_3''', of the metals used in the three systems, fulfil the following conditions:—

$$\left.\begin{array}{ll}
\tfrac{1}{4}(\varpi_1 + \varpi_1' + \varpi_1'' + \varpi_1''') = \theta, & \\
\tfrac{1}{4}(\varpi_1 - \varpi_1') = \theta', & \tfrac{1}{4}(\varpi_1'' - \varpi_1''') = \theta'', \\
\tfrac{1}{4}(\varpi_2 + \varpi_2' + \varpi_2'' + \varpi_2''') = \phi, & \\
\tfrac{1}{4}(\varpi_2 - \varpi_2') = \phi', & \tfrac{1}{4}(\varpi_2'' - \varpi_2''') = \phi'', \\
\tfrac{1}{4}(\varpi_3 + \varpi_3' + \varpi_3'' + \varpi_3''') = \psi, & \\
\tfrac{1}{4}(\varpi_3 - \varpi_3') = \psi', & \tfrac{1}{4}(\varpi_3'' - \varpi_3''') = \psi''.
\end{array}\right\} \quad . \quad . \quad . \quad (33).$$

160. To prove this, let us first consider the condition of a bar of any of the three systems, taken alone, and put in the same thermal circumstances as those in which each bar of the same system exists in the compound mass. If, for instance, we take a bar of the first system, we must suppose the temperature to vary at the rate u per unit of space along its length; at the rate v across it, perpendicularly to two of its sides; and at the rate w across it, perpendicular to its other two sides. If l be its length, and e the breadth of each side, its ends will differ in temperature by $u\,l$; corresponding points in one pair of its sides by $v\,e$, and corresponding points in the other pair of sides, by $w\,e$. Now, it is easily proved that the longitudinal electro-motive force (that is, according to the definition, the electro-motive force between conductors of the standard metal) would, with no difference of temperatures between its sides, and the actual difference $u\,l$ between its ends, be equal to $\frac{1}{2}(\varpi_1 + \varpi_1')\,u\,l$, if only the first of the zigzag conductors existed imbedded in the bar, or equal to $\frac{1}{2}(\varpi_1'' + \varpi_1''')\,u\,l$, if only the second; and, since the two have equal resistances to conduction, and are connected by a little square disc of the standard metal, it follows that the longitudinal electro-motive force of the actual bar, with only the longitudinal variation of temperature, is

$$\tfrac{1}{4}(\varpi_1 + \varpi_1' + \varpi_1'' + \varpi_1''')\,u\,l.$$

Again, with only the lateral variation $v\,e$, we have in one of the zigzags a little thermo-electric battery, of a number of elements amounting to the greatest integer in $\frac{l}{2\,e}$, which is sensibly equal to $\frac{l}{2\,e}$, since the value of this is infinitely great; the electro-motive force of each element is $(\varpi_1 - \varpi_1')\,v\,e$; and, therefore, the whole electro-motive force of the zigzag is $\frac{l}{2\,e} \times (\varpi - \varpi_1')\,v\,e$, or $\frac{1}{2}\,l \times (\varpi_1 - \varpi_1')\,v$. This battery is part of a complete circuit with the little terminal squares and the other zigzag, and therefore its electro-motive force will sustain a current in one direction through itself, and in the contrary through the second zigzag; but since the resistances are equal in the two zigzags, and those of the terminal connections may be neglected, just half the electro-motive force of the first zigzag, being equal to the action and reaction between the two parts of the circuit, must remain ready to act between conductors applied to the terminal discs of the standard metal. In the circumstances now supposed, the second zigzag is throughout at one temperature, and therefore has no intrinsic electro-motive force; and the resultant intrinsic electro-motive force of the bar is therefore

$$\tfrac{1}{4}\,l\,(\varpi_1 - \varpi_1')\,v\,.$$

Similarly, if there were only the lateral variation $w\,e$ of temperature in the bar, we should find a resultant longitudinal electro-motive force equal to

$$\tfrac{1}{4}\,l\,(\varpi_1'' - \varpi_1''')\,w\,.$$

If all the three variations of temperature are maintained simultaneously, each will produce its own electro-motive force, as if the others did not exist, and the resultant electro-motive force due to them all will therefore be,—

$$\frac{l}{4}\left\{(\varpi_1 + \varpi_1' + \varpi_1'' + \varpi_1''')\,u + (\varpi_1 - \varpi_1')\,v + (\varpi_1'' - \varpi_1''')\,w\right\}.$$

This being the electro-motive force of each bar of the first system in any of the cubes composing the actual solid, must be the component electro-motive force of each cube in the direction to which they are parallel; and, therefore,

$$a\tfrac{1}{4}\left\{(\varpi_1 + \varpi_1' + \varpi_1'' + \varpi_1''')\,u + (\varpi_1 - \varpi_1')\,v + (\varpi_1'' - \varpi_1''')\,w\right\}$$

must be the component electro-motive force of the entire parallelepiped in the same direction. Similar expressions give the component electro-motive forces parallel to the edges b and c of the solid, which are similarly produced by the bars of the second and third systems, and we infer the proposition which was to be proved.

161. Cor. By choosing metals of which the thermo-electric relations, both to the standard metal and to one another, vary, we may not only make the nine coefficients have any arbitrarily given values for a particular temperature, but we may make them each vary to any extent with a given change of temperature.

162. For the sake of convenience in comparing the actual phenomena of thermo-electric force in different directions presented by an unequally heated crystal-

line solid; let us now, instead of a parallelepiped imbedded in the standard metal, consider an insulated sphere of the crystalline substance, with sources of heat and cold applied at its surface, so as to maintain a uniform variation of temperature in all lines perpendicular to the parallel isothermal planes. Let the rate of variation of temperature per unit of length, perpendicular to the isothermal surfaces, be q, and let the cosines of the inclinations of this direction to the three rectangular directions in the substance to which the edges of the parallelepiped first considered were parallel, and which we shall now call the lines of reference, be l, m, n, respectively. Then if we take

$$q\,l = u, \quad q\,m = v, \quad q\,n = w,$$

the substance of the sphere will be in exactly the same thermal condition as an equal spherical portion of the parallelepiped; and it is clear that the preceding expressions for the component electro-motive forces of the parallelepiped will give the electro-motive forces of the sphere between the pairs of points at the extremities of diameters coinciding with the rectangular lines of reference, if we take each of the three quantities, a, b, c, equal to the diameter of the sphere. Calling this unity, then we have

$$\left.\begin{aligned} -\mathrm{E} &= u\,\theta + v\,\theta' + w\,\theta'' \\ -\mathrm{F} &= u\,\phi'' + v\,\phi + w\,\phi' \\ -\mathrm{G} &= u\,\psi' + v\,\psi'' + w\,\psi \end{aligned}\right\} \quad . \quad . \quad . \quad (34).$$

According to the definition given above (§ 144, Def. 3), it appears that these quantities, E, F, G, are the three components of *the intrinsic electro-motive force at any point in the substance*, whether the portion of it we are considering be limited and spherical, or rectangular, or of any other shape, or be continued to any indefinite extent by homogeneous or heterogeneous solid conducting matter with any distribution of temperature through it. The component electro-motive force P along a diameter of the sphere inclined to the rectangular lines of reference at angles whose cosines are l, m, n, is of course given by the equation

$$\mathrm{P} = \mathrm{E}\,l + \mathrm{F}\,m + \mathrm{G}\,n \quad . \quad . \quad . \quad . \quad . \quad (35),$$

which may also be employed to transform the general expressions for the components of the electro-motive force to any other lines of reference.

163. A question now naturally presents itself, Are there three principal axes at right angles to one another in the substance possessing properties of symmetry, with reference to the thermo-electric qualities, analogous to those which have been established for the dynamical phenomena of a solid rotating about a fixed point, and for electro-statical and for magnetic forces, in natural crystals or in substances structurally crystalline as regards electric or magnetic induction? The following transformation, suggested by Mr Stokes' paper on the Conduction of Heat in Crystals,* in which a perfectly analogous transformation is applied to the

* Cambridge and Dublin Mathematical Journal.

most general conceivable equations expressing flux of heat in terms of variations of temperature along rectangular lines of reference in a solid, will show the nature of the answer.

164. The direction cosines of the line of greatest thermal variation, or the perpendicular to the isothermal planes, are $\frac{u}{q}, \frac{v}{q}, \frac{w}{q}$, where q, denoting the rate of variation of temperature in the direction of that line, is given by the equation

$$q = (u^2 + v^2 + w^2) \quad . \quad . \quad . \quad . \quad . \quad (36).$$

Taking these values for l, m, n, in the preceding general expression for the electromotive force in any direction, we find

$$\mathrm{P} = \frac{1}{q} \left\{ \theta u^2 + \phi v^2 + \psi w^2 + (\phi' + \psi'') v w + (\psi' + \theta'') w u + (\theta' + \phi'') u v \right\}$$

the negative sign being omitted on the understanding that P shall be considered positive when the electro-motive force is from hot to cold in the substance. This formula suggests the following changes in the notation expressing the general thermo-electric coefficients:—

$$\left.\begin{array}{llll} \phi' + \psi'' = 2\theta_1, & \psi' + \theta'' = 2\phi_1, & \theta' + \phi'' = 2\psi_1 \\ -\phi' + \psi'' = 2\zeta, & -\psi' + \theta'' = 2\eta, & -\theta' + \phi'' = 2\vartheta \end{array}\right\} \quad . \quad . \quad . \quad . \quad (37),$$

which reduce the general equations, and the formula itself which suggests them, to—

$$\left.\begin{array}{l} -\mathrm{E} = \theta u + \psi_1 v + \phi_1 w + (\eta w - \vartheta v) \\ -\mathrm{F} = \psi_1 u + \phi v + \theta_1 w + (\vartheta u - \zeta w) \\ -\mathrm{G} = \phi_1 u + \theta_1 v + \psi w + (\zeta v - \eta u) \end{array}\right\} \quad . \quad . \quad (38),$$

$$\mathrm{P} = \frac{1}{q} \left(\theta u^2 + \phi v^2 + \psi w^2 + 2\theta_1 v w + 2\phi_1 w u + 2\psi_1 u v \right) \quad . \quad . \quad (39).$$

165. The well-known process of the reduction of the general equation of the second degree shows that three rectangular axes may be determined for which the coefficients θ_1, ϕ_1, ψ_1, in these expressions vanish, and for which, consequently, the equations become

$$\left.\begin{array}{l} -\mathrm{E} = \theta u + (\eta w - \vartheta v) \\ -\mathrm{F} = \phi v + (\vartheta u - \zeta w) \\ -\mathrm{G} = \psi w + (\zeta v - \eta u) \end{array}\right\} \quad . \quad . \quad . \quad . \quad (40),$$

$$\mathrm{P} = \frac{1}{q} \left(\theta u^2 + \phi v^2 + \psi w^2 \right) \quad . \quad . \quad . \quad (41).$$

166. The law of transformation of the binomial terms $(\eta w - \vartheta v)$, &c., in these expressions is clearly, that if ϱ denote a quantity independent of the lines of reference, and expressing a specific thermo-electric quality of the substance, which I shall call its thermo-electric rotatory power, and if λ, μ, ν denote the inclinations of a cer-

tain axis fixed in the substance, which I shall call its axis of thermo-electric rotation to any three rectangular lines of reference, then the values of ζ, η, ϑ for these lines of reference are as follows:—

$$\zeta = \rho \cos \lambda, \ \eta = \rho \cos \mu, \ \vartheta = \rho \cos \nu.$$

If i denote the inclination of the direction $\left(\frac{u}{q}, \frac{v}{q}, \frac{w}{q}\right)$, in which the temperature varies most rapidly, to the axis of thermo-electric rotation, and if α, β, γ denote the angles at which a line perpendicular to the plane of this angle i is inclined to the axes of reference, we have

$$\left.\begin{aligned} \eta w - \vartheta v &= \rho q \sin i \cos \alpha \\ \vartheta u - \zeta w &= \rho q \sin i \cos \beta \\ \zeta v - \eta u &= \rho q \sin i \cos \gamma \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (42).$$

Hence we see that the last terms of the general formula for the component electro-motive forces along the lines of reference express the components of an electro-motive force acting along a line perpendicular both to the axis of thermo-electric rotation, and to the direct line from hot to cold in the substance, and equal in magnitude to the greatest rate of variation of temperature perpendicular to that axis, multiplied by the coefficient ρ.

167. Or again, if we consider a uniform circular ring, of rectangular section, cut from any plane of the substance inclined at an angle λ to a plane perpendicular to the axis of thermo-electric rotation, and if the temperature of the outer and inner cylindrical surfaces of this ring be kept each uniform, but different from one another, so that there may be a constant rate of variation, q, of temperature in the radial direction, but no variation either tangentially or in the transverse direction perpendicular to the plane of the ring, we find immediately, from (42), that the last terms of the general expressions indicate a tangential electro-motive force, equal in value to $\rho q \cos \lambda$, acting uniformly all round the ring. This tangential force vanishes if the plane of the ring contain the axis of thermo-electric rotation, and is greatest when the ring is in a plane perpendicular to the same axis.

168. The peculiar quality of a solid expressed by these terms would be destroyed by cutting it into an infinite number of plates of equal infinitely small thickness, inverting every second plate, and putting them all together again into a continuous solid; a process which would clearly not in any way affect the thermo-electric relations expressed by the first term of the general expressions for the components of electro-motive force; and it is therefore of a type, to which also belongs the rotatory property with reference to light discovered by FARADAY as induced by magnetization in transparent solids, which I shall call dipolar, to distinguish it from such a rotatory property with reference to light as that which is naturally possessed by many transparent liquids and solids, and which may be called an

isotropic rotatory property. The axis of thermo-electric rotation, since the agency distinguishing it as a line, also distinguishes between the two directions in it, may be called a dipolar axis; so may the axis of rotation of a rotating rigid body,* or the direction of magnetization of a magnetized element of matter; and its general type is obviously different from that of a principal axis of inertia of a rigid body, or a principal axis of magnetic inductive capacity in a crystal, or a line of mechanical tension in a solid; any of which may be called an isotropic axis.

169. The general directional properties expressed by the first terms of the second members of (40) are perfectly symmetrical regarding the three rectangular lines of reference, and are of a type so familiar that they require no explanation here. We conclude that every substance has three principal isotropic axes of maximum and minimum properties regarding thermo-electric power, which are at right angles to one another; but that it is only for a particular class of conceivable substances that the thermo-electric properties are entirely symmetrical with reference to these axes; all substances from which the rotatory power, ϱ, does not vanish, having besides a dipolar axis of thermo-electric rotation which may be inclined in any way to them.

170. These principal isotropic axes lose distinction from all other directions in the solid, when the thermo-electric powers along them (the values of the coefficients θ, ϕ, ψ) are equal; but a rotatory property, distinguishing a certain line as a dipolar axis, may still exist. By § 159, we see how metallic structures possessing any of these properties (for instance having equal thermo-electric power in all directions, and possessing a given rotatory power, ϱ, in a given direction about a given system of parallel lines), may be actually made.

171. [*Added, July* 1854.] It is far from improbable that a piece of iron in a state of magnetization, which I have, since § 147 was written, ascertained to possess different thermo-electric properties in different directions, may also possess rotatory thermo-electric power,† distinguishing its axis of magnetization, which is essentially, in its magnetic character, dipolar, as thermo-electrically dipolar also.

§§ 172–181.—*On the general equations of Thermo-Electric Action in any homogeneous or heterogeneous crystallized or non-crystallized solid.*

172. Let t denote the absolute temperature at any point, x, y, z, of a solid. Let $\theta, \phi, \psi, \theta', \phi', \psi', \theta'', \phi'', \psi''$, be the values of the nine thermo-electric coefficients,

* [*Added, Liverpool, Sept.*, 27, 1854.]—As is perfectly illustrated by M. Foucault's beautiful experiment of a rotating solid, placing its axis parallel to that of the earth's, and so turned that it may itself be rotating in the same direction as the earth; which the meeting of the British Association just concluded has given me an opportunity of witnessing.

† [*Added, Sept.* 13, 1854.]—By an experiment made to test its existence, which has given only negative results, I have ascertained that this "rotatory power" if it exists in inductively magnetized iron at all, must be very small in comparison with the amount by which the thermo-electric power, in the direction of magnetization, differs from the thermo-electric power of the same metal not magnetized.

for the substance at this point, quantities which may vary from point to point, either by heterogeneousness of the solid, or in virtue of non-uniformity of its temperature. Let h, i, j be the components of the intensity of electric current through the same point (x, y, z).

173. Then, applying equations (31) of § 157 to infinitely small contiguous rectangular parallelepipeds in the neighbourhood of the point (x, y, z), and denoting by $\mathrm{H}\,dx\,dy\,dz$ the resultant reversible absorption of heat occasioned by the electric current across the infinitely small element $dx\,dy\,dz$, we find

$$\mathrm{H}=\frac{t}{\mathrm{J}}\left\{\frac{d}{dx}(h\theta+i\phi''+j\psi')+\frac{d}{dy}(h\theta'+i\phi+j\psi'')+\frac{d}{dz}(h\theta''+i\phi'+j\psi)\right\} \quad . \quad . \quad (43).$$

174. By the analysis of discontinuous functions this expression may be applied not only to homogeneous or to continuously varying heterogeneous substances, but to abrupt transitions from one kind of substance to another. Still it may be convenient to have formulæ immediately applicable to such cases, and therefore I add the following expression for the reversible thermal effect in any part of the bounding surface separating the given solid from a solid of the standard metal in contact with it.

$$\mathrm{Q}=\frac{t}{\mathrm{J}}\left\{p(h\theta+i\phi''+j\psi')+q(h\theta'+i\phi+j\psi'')+r(h\theta''+i\phi'+j\psi)\right\} \quad . \quad . \quad (44),$$

where Q denotes the quantity of heat absorbed per second per unit of surface at a point of the bounding surface, and (p, q, r) the direction cosines of a normal at the point.

175. Equations (34) give explicitly the intrinsic electro-motive force at any point of the solid, when the distribution of temperature is given; but we must take into account also the reaction proceeding from the surrounding matter, to get the efficient electro-motive force determining the current through any part of the body. This reaction will be the electro-statical resultant force due to accumulations of electricity at the bounding surface and in the interior of the conducting mass throughout which the electrical circuits are completed. Hence if V denote the electrical potential at (x, y, z) due to these accumulations, the components of the reactional electro-motive force are—

$$-\frac{d\mathrm{V}}{dx}, \quad -\frac{d\mathrm{V}}{dy}, \quad -\frac{d\mathrm{V}}{dz};$$

and the components of the efficient electro-motive force in the solid, are therefore—

$$\mathrm{E}-\frac{d\mathrm{V}}{dx}, \quad \mathrm{F}-\frac{d\mathrm{V}}{dy}, \quad \mathrm{G}-\frac{d\mathrm{V}}{dz},$$

where E, F, G are given by the following equations, derived from (34) by substituting for u, v, w, their values $\frac{dt}{dx}$, $\frac{dt}{dy}$, $\frac{dt}{dz}$, in terms of the notation now introduced:—

$$\left.\begin{aligned} -\mathrm{E} &= \frac{dt}{dx}\theta + \frac{dt}{dy}\theta' + \frac{dt}{dz}\theta'' \\ -\mathrm{F} &= \frac{dt}{dx}\phi'' + \frac{dt}{dy}\phi + \frac{dt}{dz}\phi' \\ -\mathrm{G} &= \frac{dt}{dx}\psi' + \frac{dt}{dy}\psi'' + \frac{dt}{dz}\psi \end{aligned}\right\} \quad . \quad . \quad . \quad (45).$$

176. The body, being crystalline, probably possesses different electrical conductivities in different directions, and the relation between current and electro-motive force cannot, without hypothesis, be expressed with less than nine coefficients. These, which we shall call the coefficients of electric conductivity, we shall denote by κ, λ, &c.; and we have the following equations, expressing by means of them the components of the intensity of electric current in terms of the efficient electro-motive force at any point of the solid:—

$$\left.\begin{aligned} h &= \kappa\left(\mathrm{E} - \frac{d\mathrm{V}}{dx}\right) + \kappa'\left(\mathrm{F} - \frac{d\mathrm{V}}{dy}\right) + \kappa''\left(\mathrm{G} - \frac{d\mathrm{V}}{dz}\right) \\ i &= \lambda''\left(\mathrm{E} - \frac{d\mathrm{V}}{dx}\right) + \lambda\left(\mathrm{F} - \frac{d\mathrm{V}}{dy}\right) + \lambda'\left(\mathrm{G} - \frac{d\mathrm{V}}{dz}\right) \\ j &= \mu'\left(\mathrm{E} - \frac{d\mathrm{V}}{dx}\right) + \mu''\left(\mathrm{F} - \frac{d\mathrm{V}}{dy}\right) + \mu\left(\mathrm{G} - \frac{d\mathrm{V}}{dz}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (46).$$

These equations (45) and (46), with

$$\frac{dh}{dx} + \frac{di}{dy} + \frac{dj}{dz} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (47),$$

which expresses that as much electricity flows out of any portion of the solid as into it, in any time, (in all seven equations,) are sufficient to determine the seven functions E, F, G, V, h, i, j, for every point of the solid, subject to whatever conditions may be prescribed for the bounding surface, and so to complete the problem of finding the motion of electricity across the body in its actual circumstances; provided the values of $\frac{dt}{dx}$, $\frac{dt}{dy}$, $\frac{dt}{dz}$ are known, as they will be when the distribution of temperature is given. We may certainly, in an electrical problem such as this, suppose the temperature actually given at every point of the solid considered, since we may conceive thermal sources distributed through its interior to make the temperature have an arbitrary value at every point.

177. Yet practically the temperature will, in all ordinary cases, follow by conduction from given thermal circumstances at the surface. The equations of motion of heat, by which, along with those of thermo-electric force, such problems may be solved, are as follows:—(1) Three equations,

$$\left.\begin{aligned} \zeta &= -\left(k\ \frac{dt}{dx} + k'\frac{dt}{dy} + k''\frac{dt}{dz}\right) \\ \eta &= -\left(l''\frac{dt}{dx} + l\ \frac{dt}{dy} + l'\frac{dt}{dz}\right) \\ \vartheta &= -\left(m'\frac{dt}{dx} + m''\frac{dt}{dy} + m\frac{dt}{dz}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (48).$$

to express the components ζ, η, ϑ of the "flux of heat" at any point of the solid, in terms of the variations of temperature $\left(\frac{dt}{dx}, \frac{dt}{dy}, \frac{dt}{dz}\right)$ multiplied by coefficients k, l, m, k', &c., which may be called the nine coefficients of thermal conductivity of the substance;—and (2) the single equation,

$$\frac{d\zeta}{dx}+\frac{d\eta}{dy}+\frac{d\vartheta}{dz} = -\frac{t}{\mathrm{J}}\left\{\frac{d}{dx}(h\theta+i\phi''+j\psi')+\frac{d}{dy}(h\theta'+i\phi+j\psi'')+\frac{d}{dz}(h\theta''+i\phi'+j\psi)\right\}$$
$$+\frac{1}{\mathrm{J}}\left\{h\left(\mathrm{E}-\frac{d\mathrm{V}}{dx}\right)+i\left(\mathrm{F}-\frac{d\mathrm{V}}{dy}\right)+j\left(\mathrm{G}-\frac{d\mathrm{V}}{dz}\right)\right\} \quad . \quad . \quad . \quad (49)$$

of which the first member expresses the rate at which heat flows out of any part of the solid per unit of volume, and the second member, to which it is equated, the resultant thermal agency (positive when there is on the whole evolution at $x\,y\,z$ produced by the electric currents.

178. The general treatment of these eleven equations (45), (46), (47), (48), (49), leads to two non-linear partial differential equations of the second order and degree for the determination of the functions t and V.

179. It may be remarked, however, that the second term of the second member of (49), when the prefixed negative sign is removed, expresses the frictional generation of heat by currents through the solid, and will, therefore, when the electro-motive forces in action are solely thermo-electric, be very small, even in comparison with the reversible generation and absorption of heat in various parts of the circuit, provided the differences of temperature between these different localities are small fractions of the temperature, on the absolute scale from its zero. Excepting then cases in which there are wide ranges (for instance, of 50° Cent. or more) of temperature, the second principal term of the second member of (49) may be neglected, and the partial differential equations to which t and V are subject will become linear; so that one of the unknown functions may be readily eliminated, and a linear equation of the fourth order obtained for the determination of the other.

180. Farther, it may be remarked that probably in most, if not in all known cases, the reversible as well as the frictional thermal action of the currents, when excited by thermo-electric force alone, is very small in comparison with that of conduction, perhaps quite insensible. [See above, § 106.] Hence, except when more powerful electro-motive forces than the thermo-electric forces of the solid itself and of its relation to the conductors touching it at any part of its surface,

act to drive currents through it, we may, possibly in all, certainly in many cases, neglect the entire second member of (49) without sensible loss of accuracy; and we then have a differential equation of the second order for the determination of the temperature in the interior of the body, simply from ordinary conduction, according to the conditions imposed on its surface. To express these last conditions generally, a superficial application of the three equations (48) with their nine independent coefficients is required.

181. When t is either given or determined in any way, the solution of the purely electrical problem is, as was remarked above, to be had from the seven equations (45), (46), and (47). These lead to a single partial differential equation of the second order for the determination of V through the interior, subject to conditions as to electro-motive force and electrical currents across the surface, for the expression of which superficial applications of (45) and (46) will be required. When V is determined, the solution of the problem is given by (45) and (46), expressing respectively the electro-motive force and the motion of electricity through the solid.

[*Additional Note Regarding the Discovery of Thermo-electric Inversions.*]

In a foot-note on the passage quoted above from the Proceedings of the Royal Society of London, I referred to phenomena observed in the use of certain alloys of bismuth and antimony in thermo-electric circuits completed by copper and by silver, as constituting the first discovery of thermo-electric inversions, having been described by Professor Cumming, in a paper published as early as 1823 in the Transactions of the Cambridge Philosophical Society. On becoming farther acquainted with the experimental results contained in that important paper, I find that they include inversions, not only in cases like those first mentioned, which might be regarded as anomalies dependent on singular properties of strange alloys, but between pure metals, in various cases; and that the actual phenomenon in the case of copper and iron, the observation of which several years later by M. Becquerel had been very generally regarded as the first discovery of thermo-electric inversion, is there described; as the following extracts show:—

"If silver and iron wires be heated in connection, the deviation attains a maximum; diminishes on increasing the heat, and again attains the former maximum on cooling."—Camb. Phil. Trans., 1823; Note on p. 61.

"Addition to p. 61" [occurring in a page of additions at the end of the paper]. "If gold, silver, copper, brass, or zinc wires be heated in connection with iron, the deviation, which is at first positive, becomes negative at a red heat."

X.—*An Investigation into the Structure of the Torbanehill Mineral, and of various kinds of Coal.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine in the University of Edinburgh. (With Two Plates.)

(Read 6th February 1854.)

The investigation of which I am now about to give an account, was undertaken with the view of determining whether the structure of the Torbanehill mineral was similar to or unlike that of coal. I was aware that the subject would be brought before a court of law, and that many scientific persons of great eminence had already spent much time in the inquiry. With the understanding, therefore, that my evidence, should it be required, was to be limited to the structure of coal and of the mineral in question, I gave directions to Mr BRYSON, the optician, of this city, to make thin sections of attested specimens of various coals and of the mineral, conceiving that a careful examination of them would easily determine the point. It was soon apparent, however, that a far more extended series of researches was necessary than I at first anticipated; but as it was also evident, from the marked structural differences which were observed in the sections, that the investigation would not be destitute of positive results, I determined on pursuing it to a conclusion.

The plan adopted was, in the first instance, to make myself familiar with the structure of the ordinary household coals used in this city, of which those called the Zetland and the Dalkeith or Buccleuch coals may be considered as the types. I then examined the structure of the Wallsend, Newcastle, and various other kinds of household coal, in every case observing, with magnifying powers of various diameters, thin sections made horizontally and longitudinally with the line of stratification. I next examined similarly made thin sections of the Torbanehill mineral, and was struck with the remarkable dissimilarity which existed between them. I now had numerous sections prepared of various cannel coals, and having previously determined the appearances presented by true coal and by the mineral, I was readily enabled to distinguish the various shades of differences between them. I saw that although the cannel coals, and especially one of them, the Brown Methil, approached in structural character to that of the Torbanehill mineral, it could still be distinguished from it by a practised eye; and that although gradations existed between these different substances, there was at least one element which served readily to characterize all the different kinds of coal I had hitherto examined, and which was not present in the mineral. I

now went over the sections of coal in the rich collection of Mr Alexander Bryson of this city, and subsequently carefully examined the numerous sections made by Dr Adams of Glasgow. Before the trial of Gillespie *versus* Russel came on, Dr Adams, Mr Quekett, and myself, spent nearly an entire day together, examining each other's specimens, and carefully re-investigating the whole subject. It was then that the character of the ashes in the various substances we had examined was pointed out to me by Dr Adams, who, in my opinion, is entitled to the greatest credit for the laborious, skilful, and successful efforts he has made in determining the structure of numerous coals, and pointing out the differences they exhibited, when compared with the Torbanehill mineral. At this meeting, also, we compared the structure of coal with various kinds of recent woods, we incinerated the mineral and certain coals, and carefully examined the ashes; and there was established, as the result of this conjoined investigation, as well as from the independent researches made by Dr Adams in Glasgow, by Mr Quekett in London, and by myself in Edinburgh, the most perfect accord with regard to all the facts which had been elicited during the inquiry.

At the commencement of the present session, I brought the subject under the notice of the Physiological Society of this city, who appointed a committee, composed of four gentlemen in addition to myself, all of whom had long been accustomed to the use of the microscope, and were familiar with vegetable and animal structures. Three of these gentlemen, viz., Dr Cobbold, and Messrs Barlow and Kirk, made farther inquiries and researches, which served to elicit additional facts, and to demonstrate, in the language of their report, that "the Torbanehill mineral is widely different from every kind of coal." Lastly, with a view of meeting certain theoretical objections which have been advanced, I have carefully examined the structure of various kinds of peat, as well as the stems of recent ferns and several fossil plants, which have only served to establish the entire absence of connection between these substances and the Torbanehill mineral.

In now endeavouring to place in a condensed form the results of this extended investigation before the Society, I propose, in the first place, to describe the facts, as they may be easily demonstrated in the field of the microscope: Secondly, to deduce from these facts the structural element which distinguishes every kind of coal from the Torbanehill mineral, and explain the cause of the differences which are recorded in the proceedings of the recent trial: Lastly, to offer a few speculations as to the nature of this mineral, as distinguished from various kinds of household and cannel coals.

I. When we examine a piece of undoubted coal, such as of the Zetland or Buccleuch coals, it presents to the naked eye a fibrous structure, and has a black shining streak. It has been found difficult to make thin sections of it, as in the grinding process it readily crumbles down. But when a tolerably thin slice, made

in the direction of the fibres, is with great pains obtained, and examined with a magnifying power of 200 diameters linear, it is then also seen to possess a fibrous structure. (Plate I., fig. 3.) These fibres may be observed to be composed of a reddish-brown coloured substance, in the centre of which is sometimes a dark streak. Oval and elongated transparent masses of a light yellow or reddish-brown colour may also be seen running parallel with the fibres, and here and there are colourless spaces, which strongly reflect light, and which are evidently filled with a crystalline mineral substance.

On examining a section horizontal to the former one, parallel with the plane of stratification, a bistre-brown or blackish opaque mass is seen, containing a number of rings of a transparent yellowish or reddish colour, with an opaque centre. These rings are from the 1000th to the 1500th of an inch in diameter, and resemble the transverse sections of tubes running at right angles to the fibres of the coal. (Plate I., figs. 1 and 2.) There may also be observed larger masses of a reddish-brown transparent material, varying in size from the $\frac{1}{6}$th to the $\frac{1}{200}$th of an inch in diameter. There are also visible, circles or rings of a rich golden yellow matter, much larger, and varying in size from the 50th to the 6th of an inch, which have been described by some as seeds or spore cases. (Plate I., fig. 1. Plate II., figs. 13 and 14.)

Similar appearances may be observed in the Wallsend, Newcastle, and all the other household coals I have examined, although in some of them, especially Newcastle coal, this structure is more obscured than in the Scotch coal, by dense black opaque matter. Here and there, however, in the Newcastle as well as in the Hamilton and some other coals, it may be found to present a highly fibrous fracture, minute chips of which exhibit at their edges distinctly dotted or porous ducts. (Plate II., figs. 5, 6, 7, 8, and 9.)

On examining the Torbanehill mineral with the naked eye, it is destitute of a fibrous structure, and presents a homogeneous appearance in whatever way it is fractured or cut. It is tough and hard to break, when compared with coal, has a dull brown streak, and is readily ground down into thin slices of any degree of tenuity. Some specimens are of a dark, and others of a light brown colour. The section of a dark specimen seen under a magnifying power of 200 diameters, presents, first, a number of yellowish and reddish-brown transparent masses, of a rounded form with an irregular outline, varying in size, from the $\frac{1}{4000}$th to the $\frac{1}{200}$th of an inch in diameter (Plate I., fig. 10). These are surrounded by a dark opaque substance, in which they appear to be imbedded, and in which no trace of structure can be detected. These light and dark substances vary in relative amount in different specimens of the mineral, and according to the thickness of the section. In some specimens, the rounded transparent masses are more widely separated, by the opaque substance, but in others, they are often so close, that a very thin section presents a homogeneous appearance of yellowish or reddish-

yellow matter, resembling bees-wax, with only a few irregular spots of the black matter. In some sections, especially of the light-brown specimens, the rounded masses, as they are ground thinner, may be seen, as it were, to melt into one another (Plate I., fig. 11). In such sections, no difference whatever can be made out, whether they be made in a longitudinal or in a horizontal direction. But in certain sections, the yellow masses assume an elongated shape, so as to resemble the appearance represented, Plate I., fig. 9.

In some thin sections these rounded transparent bodies can be separated from one another, and be distinctly seen to possess a radiated crystalline appearance, strongly reminding one of the crystals of carbonate of lime which occur in urine. (Plate II., fig. 1.) At certain angles, also, a few of them refract light, and become strongly tinted with the orange ray when polarized,—a circumstance perhaps dependent on the admixture of mineral matter. When a section of the mineral, presenting both the substances described is held over the flame of a lamp, the yellow matter evaporates in the form of thick smoke, leaving the black matter unaffected, with large holes or loculi in it. It must be clear from this experiment that the yellow matter is some bituminous or resinous substance, easily decomposed by the heat of a lamp, and that the black matter is an earthy material, which resists the same amount of heat. We can have no doubt, therefore, that an easily volatilized and highly inflammable matter has concreted in the form of rounded masses, and constitutes the light-coloured portion of the mineral formerly described. Whether this be chemically the same as, or only allied to bitumen, resin, or amber, I leave to be determined by chemists. But we may at least correctly denominate it a *Bituminoid* substance, that is, one which closely resembles, even should it turn out not to be identical with, bitumen. The matter in which this is imbedded seems for the most part to be composed of clay, or earthy matter which leaves a white ash, altogether destitute of structural traces, and is equally amorphous in whatever direction the section of the mineral is examined.

Some portion of the Torbanehill mineral, however, has a tendency to split up into thin laminæ, and presents smooth or irregular depressions, dependent on the presence of Stigmaria or other fossil plants, which, in these places, come in contact with, or are imbedded in, the substance of the mineral. Thin sections of such portions exhibit masses of a rich brown colour, composed of scalariform ducts in great numbers, and occasionally the woody fibres and rings of coal. These latter are most common where the mineral forms a junction with coal, and where the one is more or less mingled, or alternates with the other. In these places the great difference in structure between them is easily recognized both by the naked eye, and by microscopic demonstration. By the naked eye, the black shining layers of coal are easily distinguished from the brown dull appearance of the mineral, and wherever such coal exists, the streak is dark and lustrous; wherever

the Torbanehill mineral is pure, and unmixed with vegetable matter, it exhibits the dull brown streak. In such places, the mineral is characterized, under the microscope, by its yellow masses and black basis; the coal, by its rich brown fibrous structure. (Plate I., fig. 12, and Plate II., fig. 2.) Occasionally sections at the point of junction, prove that the scalariform tissue, like the substance of coal, is very friable and easily broken down. This fact which was pointed out to me by Mr Kirk, induced him to think that the amorphous basis might be composed of such tissue disintegrated, a supposition negatived by the absence of all trace of structure through the mineral generally.

From what has been said it must be evident, that there is a wide distinction between all kinds of household coal and the Torbanehill mineral, and the correct discrimination between the fibrous, woody texture of the one, and the granular bituminoid, and earthy substance of the latter, will enable us to understand the more confused texture presented in certain cannel coals, which it has been contended are identical in structure with the mineral.

I have examined a large number of cannel coals, and in every case have been enabled to recognise the fibrous structure of the longitudinal section, and the appearance of rings in the transverse sections, as they are seen in household coal. They contain, however, a greater or less number of the bituminiod masses, identical with those which constitute the principal substance of the Torbanehill mineral.* (Plate I., figs. 4 to 9.)

The Capeldrae and brown Methil coals are especially rich in these bituminoid bodies, and in consequence have been regarded as identical in structure with the mineral. In some sections of the latter coal, they are almost as numerous as those in the dark specimens of the Torbanehill mineral; but a careful examination will show that it also possesses the same organic structure as coal, and may be at once distinguished by its reddish fibres, when cut in one direction, and by the distinct rings, though few in number, observed on a transverse section. (Plate I., figs. 8 and 9.)

I consider that this proof of structure in the brown Methil coal, is decisive of the question as to the distinction between coal and the Torbanehill mineral. Every one allows, that of all the cannel coals, the brown Methil is the one which most closely resembles it. It has also been reported that no difference can be detected between them by the aid of magnifying glasses. To this I may reply, that I have always been able to distinguish them at once; that I have never been deceived in doing so, although the attempt has often been made; nor do I believe

* In reference to this point, I have carefully examined transverse and longitudinal sections of the following household and cannel coals, namely,—Buccleuch, or Dalkeith; Zetland; Newcastle; Wallsend; Jordan Hill; Knightswood; Arniston; Sheepmount; Drumfillan; Cowdenhill; Barton Hill; Eastfield, Glasgow; Stonilaw, Glasgow; Gartnavel, Glasgow; Claycross; Lesmahagow; Wemyss; Lochgelly; Capeldrae; Wigan; Civility Pit; Huddersfield; Bredisholm; Black Methil; and Brown Methil.

that any histologist who has made himself acquainted with the structure of coal on the one hand, and of the Torbanehill mineral on the other, could easily confound the two together.

There are two other modes of examination which also indicate the broad distinction in structure between coal and the mineral. These are by reducing them to powder and to an ash.

The powder of household coal contains numerous short black fibres, separated or aggregated together, mingled with mineral particles and fragments of cells. That of the Torbanehill mineral is composed of transparent yellowish masses, evidently the same as those seen in section, but more broken up, and without any trace of an envelope, mingled with fragments and the debris of the dark amorphous mineral matter. This mode of examination, though distinctive between the household coals and the mineral, is not so much so, when the brown Methil coal is chosen as the subject of comparison.

An examination of the ash, however, is still more characteristic. In the brown or blackish ashes of coals will be found, 1*st*, A greater or less number of mineral spicula, evidently the skeletons of the woody fibre; 2*d*, Siliceous masses of various irregular forms, obtained from the interstices of the organic substance; 3*d*, Black fibres, separated or in masses, evidently the woody fibre carbonized; 4*th*, Flat carbonaceous plates, presenting round apertures corresponding in size to the woody cells which passed through them, and exhibiting at their margins sections of larger circles, which doubtless bounded the large resin cells in the recent wood. (Plate II., fig. 3). None of these appearances are visible in the ash of the Torbanehill mineral, when care is taken to exclude such portions of it as are free from the stigmaria or other plants imbedded in it. Indeed I myself have never seen such appearances in the ash, even when no such precaution has been taken. Dr George Wilson gave me a considerable quantity of it, which everywhere exhibited nothing but an amorphous material, such as might result from the incineration of clay or other earthy non-organic substance. (Plate II., fig. 4). In all the cannel coals, traces of these forms, though not so numerous or abundant, can be seen. Mr Quekett has even applied this test to Welsh anthracite, in which substance no rings or fibrous structure can be made out in sections, yet where he says, the ash gives unmistakable evidence of the presence of woody tissue.*

II. Such, then, are the facts which an investigation into the structure of coals the one hand, and of the Torbanehill mineral on the other, has elicited. If the account I have given of them be correct, it must be evident that the differences

* Quarterly Journal of Microscopical Science, No. VI., p. 43. This number of the Journal for January 1854, was not published until February, after the present paper was written. I was enabled however, by the kindness of Mr Higuley, the publisher, to peruse a proof of Mr Quekett's valuable paper, before my own was read to the Society, and to interpolate the above passage.

they present are marked and distinctive; that the one is essentially a woody structure, whilst the other is not. Every kind of coal, including the Brown Methil, may be at once distinguished from the Torbanehill mineral, by the rings contained in a well-made transverse section. I further contend that such an appearance constitutes, in the majority of cases, a practical and evident test, distinctive of genuine coal, and that by means of it all kinds of known coal, whether household or cannel, can at once be distinguished from the Torbanehill mineral.*

Now if this be the case, it may well be asked how it happened that, at the late celebrated trial,† so many persons, all of whom represented themselves as being skilful observers with the microscope, should have been made to give diametrically opposite evidence, not only as to matters of opinion, but as to what appeared to be matters of fact? In endeavouring to place the remarkable histological controversy which has originated out of the trial of GILLESPIE *versus* RUSSEL on its correct basis, it must be remembered that unquestionable organic structure is only present in the Torbanehill mineral at certain places. No one, for instance, can doubt that the scalariform ducts seen by all parties are of vegetable origin; but it is nowhere pretended that these were everywhere present in the mineral. It is of great importance, therefore, not to confound the organic plants imbedded in a substance, with the substance itself. The occurrence of Stigmaria or other vegetable remains in coal, or in the Torbanehill mineral, no more constitute those substances coal, than they convert sandstone and limestone into coal, in both which rocks they are also found. Nor do I imagine it can be generally maintained that because animal substances, such as teeth, jaw-bones, or the skeletons of fishes and lizards, are occasionally found imbedded in stone, that therefore they form an essential and necessary part of the stone itself. At the trial, great amount of confusion resulted from not keeping this distinction clearly in view.

Thus when Mr QUEKETT‡ stated that all that which may be supposed like vegetable structure in the Torbanehill mineral disappears when the structure is thin, he was asked by the Dean of Faculty, "When you speak of that which appears as vegetable structure, you mean those isolated fossil plants?" to which Mr QUEKETT unfortunately answered, "Yes;" for what he really meant was, not

* Considering that hitherto no distinct definition of coal has yet been made, and that the efforts of mineralogists and chemists have only shewn that those differences they have detected are of degree rather than of kind, the structural distinction here pointed out must be of great importance.

† "A full report of the trial before the Lord Justice-General and a special Jury of the Issues in the action at the instance of Mr and Mrs GILLESPIE, of Torbanehill, against Messrs RUSSEL and SON, coal-masters, Blackbraes, for infringement of lease of coal, ironstone, &c. Reported by Mr ALEXANDER WATSON LYELL, short-hand reporter. Edinburgh: Bell and Bradfute. London: Longman and Co.; and W. Maxwell, 1853." 4to, pp. 246.

This report is acknowledged by all parties to be very accurate, and it may therefore be regarded as a trustworthy record of the scientific opinions held by numerous individuals, concerning the mineralogical properties, chemical composition, and minute structure of the Torbanehill mineral and of various kinds of coal.

‡ Mr LYELL's Report, page 67.

the isolated imbedded plants, but the structure of the mineral itself. In consequence, the counsel for the pursuer and for the defender truly played at cross-purposes throughout the whole of the structural evidence; for, notwithstanding the clearness of Dr Balfour's statement, he was asked, after saying that the mineral consists of a plant, whether he had seen fossil plants in stone? to which he answered, Yes. But then being asked whether he considered that an example of such an appearance, he very correctly, according to his views, answered, No.

From the published report of the trial, however, by Mr Lyell, it is evident that the eminent gentlemen who contended that the Torbanehill mineral was a vegetable substance abounding in cells, did not adopt this idea because various plants were imbedded in it, but because they believed the clear rounded masses I have described were themselves vegetable cells. Unfortunately, the possibility of this theory being adopted had not been anticipated, nor was it perceived by the counsel for the pursuer. In consequence, the witnesses on the one side were made to declare that the Torbanehill mineral was not vegetable, and on the other that it was, without the true reason of this discrepancy ever having been made to appear.

Dr Balfour stated in court, that he believed the yellow part of the Torbanehill mineral to consist of vegetable cells; that it was not the mere impression of a foreign fossil, but the actual structure of the mineral at that place.* In the same manner Dr Redfern, when asked,† "What do you think these yellow spots indicate?" replied, "They indicate the existence of vegetable cells." The reasons he gave for so considering them were, "That they can be perfectly isolated—they project upon the edges of all sections of the mineral—they are rounded—they are as uniform in size as the cells of other vegetable structures—the general appearance of the section is that of a piece of vegetable cellular tissue—the yellow spots do not act upon polarized light, or act upon it very feebly."

Dr Greville, also, speaking of the same bodies, said,‡ that "he had no more doubt of their being vegetable cells than he had of his own existence;" that "in one specimen it was so unequivocally marked, and so regular, that it might be compared to that of a recent plant;" and that "no person accustomed to botanical sections would hesitate in believing it to be cellular tissue."

From these quotations it must be evident that both parties saw the same things, but that while on one side it was contended that they were not vegetable cells, but bituminoid masses imbedded in clay, on the other it was strongly asseverated, in the language I have quoted, that *because* they were vegetable cells, therefore the Torbanehill mineral was a fossil plant. But in consequence of the reason of this difference in opinion not having been distinctly brought out in examination, the greatest confusion seemed to prevail in the minds of judge, counsel, and jury; and it was thought that the witnesses for the defender being skilful botanists, were enabled to see what the witnesses for the pursuers did not see.

* Mr Lyell's Report, pp. 168–9. † Ibid., p. 170. ‡ Ibid., pp. 171–2.

This result, as well as the confusion occasioned by the examination of the witnesses, is evident from the observations made by the learned Judge to the jury, from which I shall take the liberty of quoting:—

"One general remark may be made on the microscopic testimony, and it is, that there are those who see a thing, and also those who do not see it,—those who do see it, cannot see it unless it is there, and those who cannot see it do not see it at all. But very skilful persons looking for a thing and not seeing it, creates a strong presumption that it is not there. But when other persons do find it, it goes far to displace the notion that it is not there. But there is another observation on the microscopic evidence that occurred to me. I do not know whether I am under any misapprehension, but I think that three, certainly two, of those examined by the defenders are botanists also; and I do not think that any of those examined for the pursuer, three of them from London, represented themselves as botanists. Now the defenders' witnesses are accustomed to look for plants, and can understand them when they see them. The gentlemen on the other side, again, looking for woody fibre or tissue, are not, as I understand, conversant or skilful in fossil plants."*

Now, so far from the botanists seeing what the histologists did not see, it is nowhere made to appear in their evidence that they ever observed those rings on a transverse section, which I have endeavoured to show are distinctive of true coal. On the contrary, they contended that coal and the Torbanehill mineral were similar in structure, the elements of the one existing in the other, both containing vegetable cells; that the numerous yellow clear masses observed in the latter were in point of fact such cells, and constituted the proof of vegetable organization.

I think it of great importance to rescue the mode of investigation by means of the microscope from all reproach in this case, and to point out that the discrepancy which existed is not one of fact, but one of inference. I hope then it will be evident that the true scientific controversy is altogether connected with the question of whether these yellow masses, which both parties saw, described, and figured, are or are not vegetable cells.

Now the view taken up by myself from the first, and which was also taken up by Dr Adams and Mr Quekett, independently of each other, was that they are not cells, but masses of a concrete bituminoid or resinoid substance, imbedded in earthy matter. We could nowhere discover in them any trace of cell wall or contents. Their mode of fracture was more crystalline in its character than anything else; they occurred confusedly together, and nowhere presented that definite arrangement to one another, or to ducts and woody tissue, which exists in plants. Numbers of them present no envelope or definite boundary, but are scat-

* Mr Lyell's Report, pp. 238–9.

tered through a substance often more than two feet deep, extending for acres, and it may be for miles. If these yellow masses be cells, what is their origin? They cannot come from the woody tissue of the neighbouring coal, for, as we have endeavoured to show, such coal is destitute of them. The rings in coal are much smaller in diameter, are of regular size, and present the character of a tube cut transversely. Such rings could never be confounded with the yellow masses of the mineral. But supposing these latter to be cells, could such multitudes of them be derived from the gigantic ferns of the coal formation, or such as are imbedded in the mineral? I think not; because the amount of scalariform and woody tissue is too disproportioned to the number of the cells to favour such an idea. Besides, what kind of force or power could have been in operation that would have separated and collected the delicate cells, and left the ducts and other tissues of the plants by themselves, and out of sight, throughout such enormous masses. I have carefully examined the cells in large ferns, and observed the singular markings of cellular tissue, woody fibre, and scalariform ducts, many of them present, visible even to the naked eye,—than which nothing can be more unlike the Torbanehill mineral. The cells themselves are also larger, of more uniform size, and contain numerous starch granules; whilst the true resin cells are exceedingly large and distinct, strongly analogous, indeed, to what I have described as existing in the woody texture of coal, but wholly dissimilar to any thing observable in the Torbanehill mineral. Such a view, indeed, would, it seems to me, lead to the extraordinary conclusion that this mineral is composed of a vegetable tissue, more cellular than any plant ever yet met with, recent or fossil, and so rich in cells as to be wholly dissimilar to what we can even imagine to have existed, taking its size and bulk into consideration. Such masses of cells could not have been formed or nourished without ducts passing through them in various definite directions, to convey a nutritive fluid; and yet we find such ducts only to be accidental, and only distinctly connected with plants imbedded here and there in the general mass.

Whilst, then, the notion of these yellow masses being vegetable cells seems to me opposed to every known or conceivable fact yet ascertained to exist in vegetable histology, or from such as are demonstrable in the Torbanehill mineral, the theory of their being bituminoid masses imbedded in clay, appears to be in perfect harmony with all of them, and especially answers the reasons given by Dr Redfern.

With a view of determining whether the Torbanehill mineral could by any possibility be produced by a process similar to that of the formation of peat, which was described at the last meeting of the Society by Dr Fleming,* I have examined various specimens of peat, and have confirmed his description. They consist of mosses, especially of the Sphagnum, the spiral cells of which plant are peculiar, and easily recognized, associated with broken-down woody tissue, root-stalks, and bundles

* Proceedings of the Royal Society of Edinburgh. Session 1853–4, p. 216.

of simple ducts, more or less carbonized and condensed together. The deeper the peat is taken from the bog, the more condensed, broken up, and altered these textures are; still, however, sufficiently retaining their characters to be readily distinguishable. The peat of Scotland between this and Glasgow, and that of the north of Ireland, of which I have examined numerous specimens, taken from mountain bog, as well as the flow bog, are identical in structure. One specimen of peat, however, given to me by Dr TRAILL, which he obtained in Lancashire, and which answers in description to what is called Pitch Peat, is blacker in colour, the carbonizing process is more complete, and the vegetable tissues less distinct. But here and there, in a thin section of this peat, there exist rounded masses of the same bituminoid character as are found in the cannel coals and in the Torbanehill mineral. This fact confirms the theory formerly advanced, that these bodies are not cells, but a concrete bituminoid substance, probably derived from the beds of coal in Lancashire, in the immediate neighbourhood of the peat.

We may therefore conclude that every kind of coal has a distinctly woody basis, which is easily demonstrated by its longitudinal and transverse sections; that the cannel coals have, in addition to this woody structure, a greater or less number of the bituminoid masses imbedded in it; and that the Torbanehill mineral has no such woody texture, but is essentially composed of the bituminoid masses imbedded in clay.

III. In the third place, the theory which I am disposed to put forward as most in harmony with the various facts and arguments previously stated, is as follows:—1*st*, That the various organic appearances found in the sections and ashes of coal, are explicable by the supposition that coal is wood chemically altered, and for the most part coniferous wood, or wood allied to it in structure, because, from a careful comparison of recent fir wood with the various kinds of coal, I find the structural appearances of the cellular tissue, resin cells, and ducts, to be very similar. Further, no fir wood growing in this country contains spiral ducts; and it is remarkable that no traces of such ducts are to be found in any of the coals I have examined. Further, the assumption that coal is formed from fir or allied woods, not only explains its structure, but accounts for the large amount of bitumen, resin, or inflammable matter it contains, resin being a well-known abundant product of the coniferous tribe of plants.*

2*d*, The Torbanehill mineral, although it presents essentially no traces of ve-

* In the above passage, I have carefully avoided any expression which would suggest the notion that in my opinion the wood from which coal is formed, is *exclusively* coniferous wood. I believe, that with regard to the varieties and even genera of the plants of the coal-formation, there is still much to be discovered. But so far as my examinations have gone, the appearances observed warrant the general inference stated in the text, one which has also been arrived at by Mr QUEKETT. (*Mic. Journal*, No. vi. p. 42.) The important fact to be kept in remembrance is, that coal is fossil or transformed wood, whilst the Torbanehill mineral, and all the shales which I have examined, are not.

getable structure, is rich in the bituminoid substance;—a circumstance, I think, explained by the fact that it is found in the neighbourhood of coal, so that the bituminoid or resinoid matter formed in the partially woody structure of the latter has flowed out, mixed itself with, and solidified in the essentially earthy substance of the former. It is easy to conceive how enormous pressure, conjoined with chemical change and heat, may have effected this, and how sometimes such fluid bituminoid matter may have run into neighbouring beds of peat, of clay, or even of sandstone. Facts, indeed, are not wanting to show that occasionally large collections of such substance, almost pure, may be formed, unmixed with either peat or clay, of which the remarkable specimen I now exhibit to the Society, taken from the Binnie Quarry, and for which I am indebted to Dr Christison, is an example. Fragments of this substance, under the microscope, closely resemble the yellow masses which exist in the Torbanehill mineral.

In conclusion, I would remark that the controversy on this subject is only an example of a far more extensive one which is now everywhere taking place throughout the natural sciences, in reference to the influence which more improved methods of research in chemistry and histology should exercise on our thoughts and nomenclature. Those who, with myself, recognise that differences in structure indicate differences in function, and that these should be studied as the foundation for a correct classification, will recognise in the question, what is coal? an analogue to the questions, what is wood or coral?—what is bone or tooth?—what is a fibrous or a cancerous tumour? The progress of science, and especially of micro-chemistry, has already answered some of these questions, and will ultimately determine others; and in doing so, will overthrow the more vague and incorrect views and terms which previously prevailed. At the trial, indeed, it was very plausibly argued, that, in a bargain between man and man, scientific terms were of no value, and that a whale among whalers was still a fish.* But in this Society, as no naturalist, conversant with the structure and functions of a whale, would for a moment suppose it to be a fish, because it inhabits the water and resembles one; so I contend no histologist, acquainted with the structure and properties of the Torbanehill mineral, ought to maintain that it is coal, because it is dug out of the earth and burns in the fire.

* Mr Lyell's Report, p. 231.

Description of the Plates.

PLATE I., Fig. 1. Transverse section of Buccleuch or Dalkeith coal, magnified 80 diameters linear. It displays imbedded in the bistre-brown mass, 1*st*, The rings described in the text; 2*dly*, The reddish masses supposed to be resin cells; and, 3*dly*, The large circles considered to be sections of spore cases.

Fig. 2. ANOTHER portion of the same section, magnified 200 diameters linear, showing more particularly the appearance of the rings held to be characteristic of coal.

Fig. 3. Longitudinal section of the same coal. 200 diameters linear.

Fig. 4. Transverse section of the Wemyss cannel coal, showing, in addition to the rings, several bituminoid masses. 200 diameters linear.

Fig. 5. Longitudinal section of the Wemyss cannel coal. 200 diameters linear.

Fig. 6. Transverse section of the Lesmahagow cannel coal, showing a less number of the rings, but a greater number of the bituminoid masses. 200 diameters linear.

Fig. 7. Longitudinal section of the Lesmahagow cannel coal. 200 diameters linear.

Fig. 8. Transverse section of the Brown Methil coal, showing very few of the rings, but a greatly increased number of the bituminoid masses. 200 diameters linear.

Fig. 9. Longitudinal section of the brown Methil coal. 200 diameters linear.

Fig. 10. Transverse section of the darker coloured Torbanehill mineral, showing the bituminoid masses imbedded in clay. No rings are anywhere visible. 200 diameters linear.

Fig. 11. Transverse section of the lighter coloured Torbanehill mineral, showing the deep orange-coloured masses, and the melting together of the bituminoid masses. 200 diameters linear.

In these sections it will be observed, that common coal abounds in the rings, and possesses no bituminoid bodies. The cannel coals have rings and bituminoid bodies, whilst the Torbanehill mineral is principally composed of the bituminoid masses without any rings at all. It will be further seen, that in different cannel coals these various elements vary greatly in amount.

Fig. 12. Transverse section of the Torbanehill mineral, at the upper portion of the seam, where veins of coal run through it. 200 diameters linear.

PLATE II. Fig. 1. Bituminoid masses imbedded in the clay of the Torbanehill mineral, at the edge of a section, magnified 750 diameters linear, to show their radiated texture, and mode of fracture.

Fig. 2. Section of the lighter coloured Torbanehill mineral, in which a plant is imbedded, showing the scalariform vessels. 200 diameters linear.

Fig. 3. Ashes of the Zetland coal, showing mineral masses and spicula, black fibres and plates, perforated with round openings. 200 diameters linear.

Fig. 4. Ashes of the Torbanehill mineral, showing their amorphous structure. 200 diameters linear.

NATURAL HISTORY.

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NATURAL HISTORY

Fig. 1.

Fig. 2.

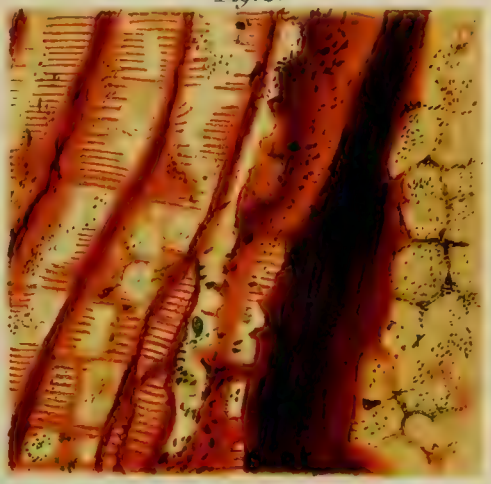


Fig. 5

Fig. 3.

Fig. 4.

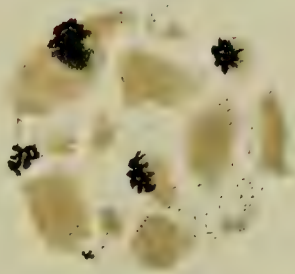
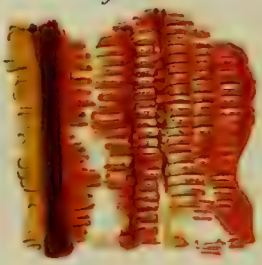


Fig. 11



J. BENNET DEL.

Fig. 6.

Fig. 7.

Fig. 13.

Fig. 10

Fig. 8.

Fig. 9.

Fig. 12.

Fig. 15.

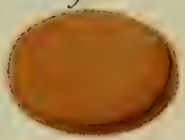


Fig. 14.

Fig. 17

Fig. 18.

Fig. 16.

J. GREVILLE DEL.

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XI.—*On certain Vegetable Organisms found in Coal from Fordel.* By John Hutton Balfour, M.D., F.L.S., Professor of Medicine and Botany in the University of Edinburgh.

(Read 20th February 1854.)

The Society has had its attention of late so much directed to the subject of coal, that some apology, perhaps, is necessary for bringing this substance again under notice. I may premise, however, that in doing so, it is not my intention to revive the disputed question as to Boghead gas-coal, nor to take up the time of the Society with what may be considered as unprofitable discussion. I purpose to bring forward a few facts relative to a coal, concerning which there is no dispute, and which presents some vegetable organisms and products calculated, in my opinion, to throw light on the history of carboniferous deposits. Much of the recent difference of opinion on the question of coal has arisen from the mode in which some histologists have chosen to define it. I trust that the result of all our discussions will be to lead to a fuller examination of coal in all its forms,—to a comparison of specimens, both mineralogically and microscopically, from different localities,—and, finally, to an extended report on the subject in which geologists, chemists, and histologists will combine.

In the meantime, I feel that we are called upon to collect facts, and to bring together authentic specimens in our public museums, which may aid in the investigation. I have already commenced the collection of specimens in the Museum of Economic Botany at the Botanic Garden, and I have secured the co-operation of parties capable of giving most efficient assistance in this respect. The establishment of a Museum of Economic Geology in Edinburgh will, it is hoped, ere long supply the means of illustrating fully the coal-fields of Scotland.

The present communication is made in the hope that the facts brought under notice may be useful in the elucidation of the nature of the plants which have been concerned in the production of coal.

The coal to which I am now to refer, occurs at Fordel Collieries, near Inverkeithing, in Fife; and the specimens have been kindly supplied by Mr Daw, Comptroller of Customs at Leith, who has taken a deep interest in the appearances and structures presented by coal, and has allowed no opportunity to pass of making observations on it. I take this opportunity of expressing publicly my obligations to him.

The coal is a splint-coal, which, when burnt, yields a considerable amount of ashes, and hence is not well suited for household purposes. It exhibits numer-

ous vegetable impressions, particularly of Sigillaria and Stigmaria. These plants appear to have been concerned in the formation of this coal, and specimens in the Edinburgh Botanical Museum seem to prove this. That these plants do frequently form coal, has been long believed by geologists and fossil botanists; but this opinion has been lately called in question by Mr Quekett, who, as the result of his histological researches, states that "such plants rarely, if ever, form coal." This startling statement appears to me to be founded on very slender data. We undoubtedly meet with separate specimens of these plants converted into carbonaceous matter, and if so, why should they not occur in a similar condition in mass, so as to form coal beds?

Sigillarias are perhaps the most important plants in the coal formation, forming a conspicuous feature in almost every field, appearing in all the strata, and very generally distributed both in the Old and New World. There are upwards of sixty species described. The plants appear to have been Acrogenous, and to have had a lax tissue, allied to the succulent ferns belonging to the suborders Marattiaceæ and Danæaceæ. Their roots, denominated Stigmarias, are also very abundant, and are common in the underclay of coal beds, as well as in the coal itself.

The tissue of these plants has in some instances been well preserved; in other cases it has been much compressed, and so altered as to show scarcely any structure under the microscope. J. D. Hooker remarks, "Considering the exceedingly lax and compressible tissue of the ordinary coal plants, it is not wonderful that instructive specimens are rare. Plants whose tissues are so *loose* as to be convertible after death into a mass of such uniform structure as coal, evidently could not retain their characters well during fossilization." The singularly succulent texture, and extraordinary size of both the vascular and cellular tissue of many Sigillarias indicate possibly a great amount of humidity. The vascular tissue of Sigillarias consists chiefly of scalariform and dotted vessels; the former marked by bars more or less complete, and the latter by dots or pits on the walls. (Plate II., figs. 6–11.)

The absence of other parts of plants, and indeed of any plants but the roots of Sigillarias, in the underclay, seems to indicate that the soil was not fitted for the growth of other vegetables. It is probable that the decay of those plants whose roots struck into the underclay would produce a uniform bed of peat, adapted to the growth of the ferns and other plants which are fossilized in the superincumbent shales. Sigillarias also occur in the shale above the coal, and in many cases their Stigmaria roots appear to have been incorporated with the coal below.* Quekett, on the other hand, maintains that coal is not formed, in any instances, from plants with a lax tissue, but in all cases from Coniferæ; and

* J. D. Hooker, on the Plants of the Coal Measures, in Report of Geological Survey, vol. ii.

he restricts his term coal to a substance formed of woody tissue of this nature. As regards this point, I think he has come to a conclusion which is not confirmed either by the external configuration or by the internal structure of the plants concerned in the formation of coal. The true characters of the coniferous wood are the *circular markings*, with the *dot* in the centre, and hence the name punctated or disc-bearing. This structure is seen both in recent Coniferæ and in the true fossil Coniferæ, such as the Dadoxylons of the sandstones in this neighbourhood. It is not however restricted to Conifers, for it has been detected in Drimys Winteri, Illicium floridanum, and other plants. This punctated structure is not easily demonstrated in the coal of the carboniferous epoch, although it has been detected in the brown coal of the tertiary beds, according to Goeppert, and in the needle-coal of Bohemia (Plate II., fig. 5). What has been considered as punctated tissue in coal appears to be, in many instances, *dotted* or *pitted vessels* (Plate II., figs. 6–9), some of which assume a scalariform appearance (Plate II., fig. 10). These vessels (often with complete rounded or elliptical perforations, owing to the disappearance of the walls) are seen evidently in Sigillarias; and Brongniart has figured them in his account of Sigillaria elegans. They occur in Arniston, Newbattle, and other coals, found in the neighbourhood of Edinburgh; and now that I have seen them in the Fordel coal, in the very substance of Sigillaria, I look upon them as the pitted vascular tissue of that plant. In Fordel and other coals, we also meet with true scalariform vessels (Plate II., fig. 11), which may be looked upon as intermediate between spiral and pitted tissue.

In some specimens of Fordel coal, where the impressions of Sigillaria are very evident, there is no difficulty in seeing under the microscope these pitted vessels, having their walls covered with perforations (Plate II., figs. 8, 9), and not as in coniferous wood (Plate II., fig. 5), where the punctated discs are confined to two sides of the tubes, and can only be seen properly when the section is made in the line of the medullary rays. The absence of central punctation, the general distribution of the perforations over the walls, and their close approximation, all, in my opinion, show the tissue to be pitted vessels (Bothrenchyma or Taphrenchyma), and not punctated woody tubes.

We have, then, in my opinion, evidence here, both from external characters and microscopic texture, that the plants forming Fordel coal were in part Sigillarias. The question then comes, What are these Sigillarias? Fossil botanists place them among Acrogens, and in the immediate vicinity of Ferns. They seem to have a close affinity to Lycopodiaceæ, and probably form a connecting link between them and Cycadaceæ. We have the scalariform tissue of the one, and the dotted tissue of the other. This appears to me to be a very interesting result of microscopic investigation; and I think we shall be confirmed in this opinion by what I have further to state in regard to Fordel coal.

I am also disposed to think that what Mr Quekett and Dr Bennett consider

to be the ends of woody coniferous tubes, are not so, but simply sections across cavities or spaces containing orange or yellow matter;—the depth of colour depending on the thickness of the section. In many instances where yellow matter exists in coal, we find it formed in cavities of different sizes, and in the centre it is common to meet with dark-coloured carbonaceous matter. On a section, such cavities in many instances exhibit a rounded contour, with a dark spot in the centre. Rounded or elliptical bodies, having a cellular or spore-like aspect, and containing yellow matter, occur more or less in all illuminating coals, whether splint, cherry, or cannel.

The quantity of yellow matter in coals varies much. It abounds in many good gas-giving coals, such as Boghead, Methil, and Capeldrae. Coal must be regarded as a rock, varying in its composition in different localities. There is a gradation in its structure and constitution in passing from anthracite to household and parrot coals; and the limit between coal and what is called bituminous shale is by no means definite. Judging by microscopical and other characters, as well as by chemical analysis, there seems no reason for separating Boghead or Torbane, Capeldrae, Methil, and other brown parrot coals from the category of true coals. Careful analyses show that the products of all are the same, viz., ammoniacal liquor, tar, naphtha, benzole, napthaline, grease-oil, paraffine, and pitch. Bitumen, or a matter soluble in naphtha, exists in very small quantity in coals, and is more abundant in English caking coals than in cannel coals. The quantity of inflammable matter, or rather of hydrogen, in coals seems to determine the quantity of fixed carbon. In such coals as Boghead the quantity of hydrogen is very large, and hence the complete nature of the combustion.

In reviewing the plants which are concerned in the formation of coal, J. D. Hooker, in his paper, published in the Reports of the Geological Survey of Great Britain, remarks, that Coniferæ are chiefly found in the sandstone; and their remains being exceedingly rare in the clays, shales, and ironstones, it may be concluded that they were never associated with the Sigillarias and other plants which abound in the coal seams, but that they flourished in the neighbourhood, and were at times transported to these localities.

Mr Binney of Manchester gives an instance of an erect fossil conifer passing from the roof of one coal seam *through* another one, and having deposited round it many feet of sandstone, followed by underclay, a bed of coal, shale, and other successive deposits. This is looked upon by some as a proof of the rapidity with which the coal-beds were formed, of the rapid decomposition of those plants which constituted the coal, in comparison with the coniferous wood, and of the probable soft-tissued nature of the plants which formed that deposit.

In coal from Newbattle, I have seen a remarkable cellular structure containing yellow matter, associated with the ordinary dense carbonaceous matter forming the darker portion of the coal. The specimen seems to show, that different

kinds of plants, some of them cellular, have entered into the composition of coal. Believing Sigillarias and Stigmarias to have had a large amount of cellular tissue in their structure, we can understand that in coal formed from them this tissue may, in certain instances, remain more or less entire, while in other instances it may have been compressed in such a way as to obliterate or rupture the cell-cavities. Dotted vessels, moreover, are not so dense, nor so much thickened in their walls, as woody tissue, and hence, in many cases, pressure may have in like manner destroyed their characteristic appearance, and by the approximation of their walls have given rise to some of the so-called fibrous appearances in coal.

Besides Sigillarias and Stigmarias, we also detect in the Fordel coal peculiar rounded organisms, which have the appearance of seeds (Plate II., figs. 12, 13). Dr FLEMING informs me, that similar bodies have been observed by him in coal, and that he exhibited them to Mr WITHAM about twenty years ago. They have also been seen by Dr FLEMING in Lochgelly and Arniston parrot, and in the coal at Boghead; and from having observed them in cherry, splint, and cannel coals, he is disposed to consider them as a somewhat common feature. I have seen them in coal from Miller-hill, near Dalkeith, as well as in the coal from Fife. They do not appear to have been fully described. The nearest approach to them is the Lycopodites, figured by Mr MORRIS in the Appendix to Mr PRESTWICH'S paper on the Geology of Coal-Brook Dale.* They appear to be certainly allied to the fructification of the Lycopodiaceæ of the present day, more particularly to that form of it which consists of two valves placed in apposition, and containing what is called Lycopode-powder, or minute cells having a yellow glistening aspect, interspersed sometimes with matter of a dark wine-colour.

These seed-like bodies in Fordel coal (Plate II., figs. 13, 14, 15), I therefore consider to be the sporangia or spore-cases of some plant allied to Lycopodium, perhaps Sigillaria. They are remarkably preserved in the coal, and occur in many instances in vast quantity. They have a rounded form,—their colour is dark brown, and they seem to be formed by two valves inclosing a cavity which is often filled with black carbonaceous matter (Plate II., figs. 16, 18). In some specimens we remark one valve separated so as to expose a dark mass in the hollow of the other valve (Plate II., fig. 16), which is imbedded in the coal. At other times, when a section is made of the coal, these sporangia are cut across, and exhibit an evident cavity (Plate II., figs. 17, 18). When thin sections of the coal are viewed by transmittted light, the walls of the sporangia appear of a brownish or orange-yellow colour (Plate II., fig. 14).

Under the microscope, the valves often present a reticulated appearance, and minute granular matter seems to be attached to the inner surface. These granules I suppose to be some of the minute powdery spores slightly altered. It may be

* Geological Transactions, v., p. 485. Plate 38, fig. 9.

that the bodies called by some spores, and which exist in many coals, especially cannel coals, may be the larger spores contained in the other sporangia of this plant. These fossil spores are large, and appear in the form of a thickened ring, probably from the pressure to which they have been subjected. Here, then, we seem to have evidence, that Acrogenous fructification is found in coal, leading to the conclusion that the plants which produced these sporangia flourished at the coal epoch, and aided in the formation of this substance.

It is probable also, that the inflammable yellow-brown matter which enters into the composition of Lycopodes at the present day, and which has caused their small spores to be denominated vegetable sulphur, may also have been present in fossil plants of a similar nature, and have contributed to form the yellow substance which exists in great quantity in some coals.

A substance derived from the organic kingdom also occurs abundantly in the Fordel coal. This is the resin-like matter called Middletonite. This was seen about thirty years ago by Dr Fleming, in the splint coal of Balbirnie in Fife, and afterwards at Clackmannan. Dr Fleming is also disposed to think, that certain veins of a rich wine-yellow, which occur in Boghead coal, contain Middletonite. This organic substance has been described by Professor Johnston of Durham.* He found it about the middle of the main coal or Haigh More seam, at the Middleton collieries near Leeds. It occurred sometimes in small round masses, but more commonly in thin layers, scarcely thicker than $\frac{1}{16}$th of an inch, between layers of coal. In Mr Daw's specimens, the quantity of the substance is very large, occurring both in layers and in granular pieces, and giving a peculiar rusty-brown aspect to the surface of the coal. Specimens are seen with several distinct layers of this substance, separated by thin laminæ of coal of about $\frac{1}{4}$th of an inch thick, which also seems to be penetrated by the Middletonite.

Middletonite is hard, brittle, easily scraped to powder by a knife. In small fragments it is transparent; by reflected light it shows a reddish-brown colour, by transmitted light a deep-red colour. It has a resinous lustre. It blackens by long exposure to the air, and then can only be distinguished from the coal by a slight peculiarity in the lustre. It burns like resin, and leaves a bulky charcoal.

In one analysis, Johnston gives the following composition:—

Carbon,	86·437
Hydrogen,	8·007
Oxygen,	5·563

On examining this substance, and comparing it with the appearance presented by Lycopode powder, as well as with that exhibited by the inner surface of the Fordel sporangian valves, I am disposed to hazard the conjecture, that the two may be closely connected. It seems not improbable, that the inflammable spores

* Brewster's Journal, xii. (1838), 261.

when acted on by heat and other causes, may have thus formed a continuous layer of Middletonite in the seams of coal. At all events, the matter deserves consideration.

In conclusion, I think that this coal gives evidence of Sigillarias and Stigmarias having entered into its formation,—of Acrogenous plants allied to Lycopodiaceæ having also been present, as indicated by the abundance of peculiar sporangia, and of the probable origin of Middletonite from the contents of these sporangia. In the further prosecution of the subject, it will be interesting to observe if in those coals which contain Middletonite similar sporangia can be detected.

Explanation of PLATE II., *Figures* 5 *to* 18.

Fig. 5. Punctated woody tissue, apparently coniferous, from the needle-coal of Töplitz in Bohemia; from a specimen sent by Professor HARKNESS (magnified 190 diameters).

Figs. 6 and 7. Dotted or Pitted vascular tissue (Bothrenchyma) from Arniston coal (magnified 190 diameters).

Figs. 8 and 9. Pitted vascular tissue, from Fordel coal (magnified 190 diameters). This kind of tissue is common in the carbonaceous matter, which is often found between the laminæ of coal and which soils the fingers.

Fig. 10. Pitted vessel from coal with the dots elongated transversely, and giving a scalariform appearance (magnified 190 diameters).

Fig. 11. Scalariform vessels from coal, resembling those of ferns (magnified 190 diameters).

Fig. 12. Seed-like bodies or sporangia, found in vast abundance in Fordel splint coal, natural size.

Fig. 13. The same sporangia magnified about 8 diameters, imbedded in a mass of Fordel coal; some lying on the surface, others projecting from the broken edges of the coal. They seem to occur frequently in coal from different localities, both in Scotland and in England. Mr BINNEY has seen them in Wigan coal. Similar sporangia occur in enormous quantity in specimens of a brown inflammable deposit sent by Sir W. DENISON from Van Diemen's Land.

Fig. 14. Section of Fordel coal, showing the sporangia as viewed by transmitted light, and magnified 20 diameters. The orange-yellow lines indicate the walls of the sporangia cut across in a microscopic section.

Fig. 15. Sporangium magnified 20 diameters.

Fig. 16. Valves of sporangium separated, containing a quantity of black carbonaceous matter in its interior (magnified 24 diameters).

Fig. 17. Sporangium cut transversely, showing the internal cavity (magnified 24 diameters).

Fig. 18. Sporangium cut obliquely, showing the cavity and the dark-coloured contents (magnified 24 diameters).

XII.—*Researches on some of the Crystalline Constituents of Opium.* Second Series. By Thomas Anderson, M.D., Regius Professor of Chemistry in the University of Glasgow.

(Read 3d January and 1st May 1854.)

In pursuing the investigation of the crystalline constituents of opium, which formed the subject of a previous communication to this Society, I have succeeded in obtaining from the same mother liquor which formed the raw material of my previous researches, a considerable quantity of papaverine, the base recently detected by Merck, and of meconine, the indifferent crystallizable substance discovered by Couerbe in the year 1830. The former was encountered quite unexpectedly in the precipitate from which narcotine and thebaine were prepared by the process described in the first series of these researches. The latter was only obtained after many fruitless trials, in which I was induced to persevere by the desire of comparing it with the substance discovered by myself among the products of the decomposition of narcotine by nitric acid, and described under the name of opianyl. The composition of that substance, as determined by my analyses, approximates very closely to that of meconine; and though the formula assigned to the former is double that obtained by Couerbe from his analysis of the latter, the sole reason for adopting the higher atomic weight was, that the mode in which opianyl was obtained by the decomposition of narcotine, afforded satisfactory grounds for establishing its true constitution. Nor does their similarity stop here, for it is at once obvious, on a comparison of their properties, that they present many points in common, although even in this respect there are differences which will be afterwards particularly referred to, much too prominent and important to be overlooked. Partly on this account, and partly because my previous investigations of the constituents of opium had shown me that in all other instances the analytical results arrived at by Couerbe were very far from correct, I hesitated to assert their identity until I had had an opportunity of submitting them to an exact comparison, and of repeating the analysis of meconine.

The experiments to be detailed in this paper prove incontestibly that they actually are identical; and while the composition of meconine is confirmed, the necessity for doubling its formula is clearly established. A careful comparison of the properties of the two substances has also shown that they are perfectly alike; but in this respect, and especially as regards the action of various agents on meconine, the statements made by Couerbe require very material correction.

Papaverine.

In the purification of narcotine by repeated crystallizations from boiling alcohol, in the manner described in the paper already referred to, the mother liquors of each successive crystallization had been carefully preserved, and at the conclusion of the investigation they were worked up for the purpose of obtaining the very considerable quantity of narcotine which they obviously still contained. For this purpose the fluids were mixed together, and the greater part of the alcohol separated by distillation. The residue on being left to itself, deposited a considerable quantity of dark-coloured crystals mixed with resinous matters, which were collected on a cloth and expressed, and the fluid which passed through was again concentrated and allowed to crystallize, and this was repeated until it ceased to yield anything on further evaporation. The crystals so obtained were dissolved in the smallest possible quantity of boiling alcohol, from which they were deposited in a tolerably colourless state as the liquid cooled. On further concentrating this mother liquor, and again allowing it to cool, it became filled with crystals of a substance obviously much more soluble in alcohol than narcotine, and differing from it in its general characters. It was particularly observed to restore the blue of reddened litmus, to dissolve readily in acids, and to saturate them completely; properties not possessed by narcotine. Its ready solubility in alcohol led at first to the suspicion that it might be thebaine, which had been already observed accompanying the narcotine, but the difference in its crystalline form, as well as its complete precipitation from its solution in acetic acid, by subacetate of lead, proved it to be different. A very few experiments sufficed to show that the base thus obtained was still contaminated with narcotine, from which it might no doubt have been separated by repeated crystallizations; but as this would have been an extremely tedious process, and have caused the loss of a considerable quantity of material, I preferred a method founded on the marked difference between its basic properties and those of narcotine. The whole of the crystals obtained from the mother liquors, and consisting partly of narcotine and partly of this other base, were reduced to a fine powder, and digested with a limited quantity of acetic acid. The acid was rapidly saturated, and as soon as the fluid had lost its acid reaction it was filtered from the undissolved portion, which was again treated with the acid; and this treatment was cautiously repeated as long as it continued to be thoroughly saturated. The solution was then filtered from the undissolved narcotine, precipitated by ammonia, and the precipitate crystallized from boiling alcohol. The base was now pure, and analysis showed it to be papaverine, with which its characters were found to agree in all respects. A consideration of the properties of papaverine now enabled me to perceive that it must have been present in very large quantity in the mother liquor obtained in the first crystallization of the crude narco-

tine, which had not been mixed with the others, but treated in a different manner for the preparation of thebaine, and it seemed probable that it would be found in abundance in the precipitate produced in that fluid by subacetate of lead. This opinion proved to be perfectly correct, and after a few trials it was found that it might be readily extracted in the following manner. The subacetate of lead precipitate was reduced to fine powder and boiled with alcohol, by which means a very dark fluid was obtained, which deposited a few crystals, apparently of narcotine, on cooling, and on further concentration, a dark-coloured resinous substance was left. After the complete expulsion of the alcohol, the residual mass was treated with dilute hydrochloric acid, which dissolved narcotine and papaverine, and left a quantity of a black resinous matter, which was separated by filtration. The fluid was then concentrated and left to itself. In the course of a few days crystals of the sparingly soluble hydrochlorate of papaverine began to make their appearance, and continued gradually to increase for some time. As soon as they ceased to increase they were removed from the solution, which was found to contain narcotine, and purified by several crystallizations. The base was then separated by ammonia, and obtained in a state of absolute purity, by solution in boiling alcohol, and crystallization.

Papaverine, as thus obtained, is in the form of minute radiated crystals, highly soluble in boiling alcohol; so much so, that a saturated hot solution becomes nearly solid on cooling. It saturates the acids completely, and gives in the most distinct manner the reaction with strong sulphuric acid described by Merck. Although the formula assigned to papaverine by its discoverer appeared to be correct, an analysis of that obtained by the process just described was made; and as in the course of the investigation it became necessary to ascertain by analysis whether particular specimens were free of narcotine, several additional combustions were obtained, the results of which are also given. The analyses were made with the chromate of lead.

I. { 4·250 grains of papaverine, dried at 212°, gave
11·020 ... carbonic acid, and
2·410 ... water.

II. { 4·525 grains of papaverine gave
11·714 ... carbonic acid, and
2·635 ... water.

III. { 4·416 grains of papaverine gave
11·430 ... carbonic acid, and
2·569 ... water.

{ 8·380 grains of papaverine gave, by Peligot's method,
0·369 ... nitrogen.

8·960 grains of papaverine gave, by Peligot's method,
0·355 ... nitrogen.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . .	70·71	70·60	70·58	70·79	C_{40}	240
Hydrogen, . .	6·29	6·46	6·46	6·20	C_{21}	21
Nitrogen,* . .	4·40	3·96	...	4·14	N	14
Oxygen, . .	18·60	18·96	...	18·78	O_8	64
	100·00	100·00		100·00		339

These results correspond completely with the calculated numbers, and were sufficient to show that the base is papaverine, but for further security, a determination of the platinum in its platinum salt was made, with the following results:—

5·684 grains of platinum salt gave
1·013 ... platinum = 17·82 per cent.

This corresponds with calculated result, which gives 18·10 per cent.

The salts of papaverine having been already sufficiently investigated by Merck, I did not think it necessary to extend my experiments in this direction, but proceeded to examine the action of various agents upon the base itself.

Action of Nitric Acid.

Papaverine dissolves in dilute nitric acid without decomposition, and with the formation of a nitrate; but if the solution be mixed with an excess of nitric acid, especially if concentrated, and heat be applied, a brisk action takes place, some red fumes are evolved, the fluid becomes dark red, and orange-coloured crystals begin to make their appearance, and gradually increase in quantity. Nothing can be simpler than the preparation of this substance. The degree of concentration of the acid is immaterial, for the product is sure to be obtained; and if the fluid be sufficiently concentrated, crystals begin to be formed the instant the heat is applied, and soon become so abundant as entirely to fill the fluid. If the solution be more dilute, they appear more slowly, and are then larger, more regularly formed, and much paler coloured. The only precaution necessary is, to avoid the use of too large an excess of nitric acid, as it dissolves the new product much more abundantly than water. The orange-coloured crystals are the nitrate of a new base, to which I give the name of nitropapaverine.

Nitropapaverine — is readily obtained from its nitrate, by dissolving it in boiling water, and precipitating the hot solution by ammonia; but as the salt is very sparingly soluble, even in hot water, it is more convenient to dissolve it in nitric acid, and then to add at once an excess of the alkali. A light yellow flocculent precipitate is immediately deposited, which is collected on a filter, dried,

* Merck has published only one nitrogen determination of papaverine, giving 4·75 per cent., which greatly exceeds the calculated number. I have observed the same tendency to give an excess, having in one experiment obtained as much as 4·66. The only mode of explaining this anomaly, was by supposing that the base carried down a small quantity of the ammonia which had been used to precipitate it. In order to ascertain whether this opinion was well-founded, a quantity thrown down by potash was analysed, and the results are those of the second determination.

and dissolved in boiling alcohol, from which it is deposited on cooling. Nitropapaverine is thus obtained in pale buff-coloured needles, insoluble in water, but soluble in alcohol and ether. It restores the blue colour of reddened litmus, dissolves in the acids, neutralizes them completely, and forms a series of salts, all of which have a pale buff colour and are sparingly soluble in water. When heated, it undergoes fusion, and at a higher temperature it burns rapidly with a sort of deflagration. Concentrated solution of potash, when boiled with it, causes the evolution of traces of a volatile base. It does not give the purple reaction of papaverine with sulphuric acid. Its analysis gave the following results:—

{ 5·065 grains of nitropapaverine, dried at 212°, gave
11·573 ... carbonic acid, and
2·640 ... water.

	Experiment.	Calculation.		
Carbon, . . .	62·31	62·50	C_{40}	240
Hydrogen, . .	5·21	5·20	H_{20}	20
Nitrogen, . .	...	7·29	N_2	28
Oxygen, . . .	...	25·01	O_{12}	96
		100·00		384

This corresponds closely with the calculated results for the formula

$$C_{40}\ H_{20}\ (NO_4)\ O_8$$

which is further confirmed by the analysis of the platinum salt given below.

The crystals of the base deposited from alcohol are a hydrate, as indicated by the subjoined experiment.

{ 5·237 grains of nitro-papaverine dried in the air,
lost at 212° 0·14 grains = 2·67 per cent.

One equivalent of water requires 2·29 per cent., so that the crystals have the formula $C_{40}\ H_{20}\ (NO_4)\ O_8 + HO$.

Nitrate of Nitropapaverine.—This salt is obtained by the action of nitric acid on papaverine, in the manner already described. It appears in the form of four-sided tables, sometimes of considerable size, and generally of a dark orange colour. By crystallization from boiling alcohol, it is got in a state of absolute purity, and then forms fine yellow crystals of great beauty. It is almost insoluble in cold water, but on boiling it is taken up in somewhat larger quantity, and deposited on cooling in an imperfectly crystalline form. It is much more soluble in water containing nitric or hydrochloric acids, as well as in alcohol and ether. When gently heated it melts, and then deflagrates, leaving a quantity of a nearly black substance, which burns away completely at a higher temperature. Potash and ammonia, when digested on it at ordinary temperatures, rapidly separate the base. The ease with which this substance is formed, and its extreme insolubility render its production a very useful reaction for papaverine, and by means of it I have succeeded in proving that this base also occurred in the fluid from which narco-

tine was separated by precipitation with ammonia. Nitrate of nitropapaverine is anhydrous. Its analysis gave these results:—

5·497 grains nitrate of nitropapaverine gave
10·820 ... carbonic acid, and
2·450 ... water.

	Experiment.	Calculation.		
Carbon,	53·68	53·69	C_{40}	240
Hydrogen,	4·95	4·69	H_{21}	21
Nitrogen,	...	9·38	N_3	42
Oxygen,	...	32·24	O_{18}	144
		100·00		447

corresponding with the formula

$$C_{40}\,H_{20}\,(NO_4)\,O_8 + HO\,NO_5.$$

Hydrochlorate of Nitropapaverine—is a sparingly soluble salt, crystallizing in pale yellow needles. It dissolves readily in spirit, and in excess of hydrochloric acid.

Sulphate of Nitropapaverine—is but little soluble in water; it crystallizes in minute prisms.

Platino-Chloride of Nitropapaverine—is thrown down as a pale yellow precipitate, when bichloride of platinum is added to a solution of the hydrochlorate. Its analysis gave the following results,—

5·555 grains, dried at 212°, gave
8·245 ... carbonic acid, and
1·900 ... water.

5·579 grains, dried at 212°, gave
0·924 ... of platinum.

	Experiment.	Calculation.		
Carbon,	40·47	40·66	C_{40}	240
Hydrogen,	3·80	3·55	H_{21}	21
Nitrogen,	...	4·72	N_2	28
Oxygen,	...	16·26	O_{12}	96
Chlorine,	...	18·09	Cl_3	106·5
Platinum,	16·56	16·72	Pt	98·7
		100·00		590·2

corresponding completely with the formula

$$C_{40}\,H_{20}\,(NO_4)\,O_8\,HCl + PtCl.$$

The rest of the salts of nitropapaverine have not been minutely examined.

Action of Bromine on Papaverine.

When bromine water is added, drop by drop, to a solution of hydrochlorate of papaverine, a precipitate is obtained which at first immediately redissolves, but on the further addition of bromine water, at length becomes permanent. It is the hydrobromate of a derivative base, bromopapaverine.

Bromopapaverine itself is easily obtained from this substance, by digesting it with ammonia, and dissolving the product in boiling alcohol. It is deposited, on cooling, in minute white needles, insoluble in water, but readily soluble both in alcohol and ether. It dissolves in the acids and forms salts, the greater number of which are characterized by their sparing solubility. Its crystals were anhydrous, and gave on analysis the subjoined results,—

5·168 grains bromopapaverine gave
10·845 ... carbonic acid, and
2·335 ... water.

4·780 grains, dried at 212°, gave
2·185 ... bromide of silver.

These results correspond with the formula $C_{40}\ H_{20}\ Br\ NO_8$, as is obvious from the calculation given below.

	Experiment.	Calculation.		
Carbon,	57·23	57·41	C_{40}	240
Hydrogen,	5·02	4·78	H_{20}	20
Nitrogen,	...	3·34	N	14
Oxygen,	...	15·34	O_8	64
Bromine,	19·45	19·13	Br	80
		100·00		4·18

Hydrobromate of Bromopapaverine.—This salt is obtained, as already mentioned, by the addition of bromine water to a solution of hydrochlorate of papaverine. It varies somewhat in its appearance, according to the degree of concentration of the solution from which it is obtained. If concentrated, it is deposited with a more or less yellow colour, and is apt to form resinous lumps, particularly if the bromine water is added rapidly; but when the solutions are more dilute, it appears in the form of a perfectly white powder. It is well washed with water, dissolved in boiling spirit, and on cooling is deposited in a state of purity in the form of a crystalline powder, insoluble in water, and sparingly soluble in alcohol. When gently heated it melts, and is decomposed. When digested with potash or ammonia, it is decomposed, with separation of the base. Its crystals are anhydrous, and gave the following results on analysis:—

5·647 grains, dried at 212°, gave
10·015 ... carbonic acid, and
2·215 ... water.

6·398 grains, dried at 212°, gave
4·883 ... bromide of silver.

	Experiment.	Calculation.		
Carbon,	48·36	48·09	C_{40}	240
Hydrogen,	4·35	4·20	H_{21}	21
Nitrogen,	...	2·80	N	14
Oxygen,	...	12·85	O_8	64
Bromine,	32·48	32·06	Br_2	160
		100·00		499

corresponding with the formula

$$C_{40}\,H_{20}\,Br\,NO_8 + HBr.$$

Hydrochlorate of Bromopapaverine — is soluble in water, though sparingly. The rest of its salts have not been examined.

Action of Chlorine on Papaverine.

When a current of chlorine is passed through a solution of hydrochlorate of papaverine, the fluid becomes brown, and after a time a dirty gray precipitate makes its appearance, which is insoluble, or nearly so, in water, but dissolves in boiling alcohol, and gives a resinous deposit as the solution cools. When treated with ammonia, hydrochloric acid is separated, and a powder is obtained which is obviously chlorine base. It dissolves in the acids, and is reprecipitated by ammonia. Alcohol takes it up, and deposits it in an amorphous form on cooling or by spontaneous evaporation. The properties of this substance proving unsatisfactory, I then tried the action of chlorate of potass added in small successive portions to a solution of papaverine in excess of hydrochloric acid, but the same amorphous gray powder being produced, I did not pursue the subject further; for although one or more derivative bases are unquestionably produced, their properties are not sufficiently well marked to admit of their proper purification.

Action of Iodine on Papaverine.

When an alcoholic solution of papaverine is mixed with tincture of iodine, and the solution is left to itself for some hours, small crystals are slowly deposited, and by further evaporation of the fluid from which they have been removed, another substance is obtained.

Teriodide of Papaverine.—To the crystals first separated I give this name. They are purified by solution in boiling alcohol, from which they are slowly deposited in small rectangular prisms. Their colour is purple by reflected, and dark red by transmitted light. They are insoluble in water, but soluble in alcohol. It is not acted upon by dilute acids, but ammonia and potash rapidly decompose it, removing iodine, and leaving papaverine. From this fact it is clear that it is not the salt of a substitution base, but corresponds in constitution with the teriodide of codeine, which I described in a former paper.* Analysis gave the subjoined results:—

7·065 grains teriodide of papaverine, dried at 212°, gave
8·550 ... carbonic acid, and
2·045 ... water.

7·650 grains teriodide of papaverine gave
7·489 ... iodide of silver.

* Edinburgh New Philosophical Journal, vol. l., p. 103.

	Experiment.	Calculation.		
Carbon,	33·02	33·47	C_{40}	240
Hydrogen,	3·21	2·92	H_{21}	21
Nitrogen,	...	1·95	N	14
Oxygen,	...	8·95	O_8	64
Iodine,	52·90	52·71	I_3	378
		100·00		717

The formula of the substance is therefore

$$C_{40}H_{21}NO_8 + I_3.$$

Pentiodide of Papaverine.—By evaporating the mother liquor of the teriodide this compound is deposited, and is purified from excess of iodine by crystallization from alcohol. It then forms slender needles, with an orange colour by transmitted light, and a reddish-bronze surface colour. It is much more soluble in alcohol than the teriodide, but is insoluble in water. It loses iodine by the application of a moderate heat, although perfectly stable at 212°. It is insoluble in acids, and is rapidly decomposed by ammonia. Its composition was:—

{ 5·502 grains pentiodide of papaverine gave
5·000 ... carbonic acid, and
1·283 ... water.

{ 3·902 grains pentiodide gave
4·665 ... iodide of silver.

	Experiment.	Calculation.		
Carbon,	24·78	24·76	C_{40}	240
Hydrogen,	2·59	2·16	H_{21}	21
Nitrogen,	...	1·44	N	14
Oxygen,	...	6·63	O_8	64
Iodine,	64·60	65·01	I_5	630
		100·00		969

These results give the formula

$$C_{40}H_{21}NO_8 + I_5.$$

It is difficult to form a rational conception of the constitution of these substances. The formulæ given above seem to be on the whole the most probable; but it is clear that the analyses admit of a different interpretation, and we may assume these compounds to be hydriodates with excess of iodine, and represent their constitution by the formulæ

$$C_{40}H_{21}NO_8HI + I_2$$
$$C_{40}H_{21}NO_8HI + I_4$$

the additional atom of hydrogen making too small a difference to be important. We have, however, no experiments to enable us to decide between these two views, which could only be done by an extended investigation of the similar compounds of the other bases; and in the absence of further information, I have

chosen to represent them as iodides, simply because it involves less hypothesis, and sufficiently expresses their constitution.

Action of Soda Lime on Papaverine.

Papaverine was mixed with about four times its weight of a soda lime, composed of equal weights of its constituents, and the mixture heated in the oil bath. At 250° traces of a volatile base began to appear, which increased in abundance when the temperature rose to 300°. After the heat had been continued for some hours, a small quantity of a pungent base was collected in the receiver. It gave white fumes with hydrochloric acid, and with bichloride of platinum a highly soluble salt, precipitated by alcohol and ether in shining plates. A determination of the platinum in this compound gave the following results :—

5·197 grains platinum, dried at 212°, gave	
1·882	... platinum.

This corresponds to 36·21 per cent. of platinum, and lies almost exactly between the numbers required for propylamine and ethylamine, the calculated number for the former being 37·21, for the latter 35·34. It is probable that the substance analysed actually contained both bases, but from the small scale on which it was necessary for me to work, I was precluded from any attempts to substantiate this opinion.

Meconine.

In preparing narceine by the process described in the first series of these researches, I expected to have obtained it mixed with meconine, in the same way as it had been previously found by Couerbe and other observers. Much to my disappointment, however, the narceine proved to be remarkably pure; and even after one crystallization, ether extracted from it only a minute trace of a resinous matter, but no meconine. As it could not be doubted that meconine, if present at all, must still remain in the mother liquor from which the crystals of narceine had been separated, the fluid was concentrated and left to itself for some time, but no meconine made its appearance. On further concentration, and standing for some months, granular crystals were abundantly deposited, which proved to be nothing but muriate of ammonia, mixed with a resinous matter. Attempts were then made to remove the amorphous residual matters which abounded in the fluid, and appeared to prevent the crystallization of meconine, and a variety of methods were tried to make it deposit crystals, but without success.

After many abortive trials, which it is unnecessary to detail, I found that meconine might be easily separated by agitating the fluid with ether. For this purpose the concentrated fluid was poured into large bottles in which it was mixed with about a fifth of its bulk of ether. The bottles were tightly corked, set in a warm place where their temperature was kept at about 80° Fahr., and frequently shaken.

After standing for 24 hours, the ether, which had acquired a brown colour, was drawn off with a syphon and a new supply added; and this process was repeated as long as it continued to extract any considerable quantity. The ether was then distilled off in the water-bath, and left behind a dark amber-coloured or brown syrupy fluid. The first extracts remained thick and syrupy, even after 24 or 48 hours; but those obtained after the agitation with ether had been several times repeated, occasionally, though not always, became filled with crystals on standing. On the addition of water, a quantity of a thick resinoid substance, not unlike turpentine, separated and remained for some time in a semifluid form at the bottom of the fluid. When hydrochloric acid was dropped into it, part of the resinoid matter dissolved, and the rest solidified into a dark-gray crystalline powder, which was separated by filtration. The hydrochloric solution gave with alkalies a dirty gray precipitate which soon became resinous on standing, with bichloride of platinum a yellow, and with corrosive sublimate a dirty white precipitate. It contained therefore an alkaloid, which was thrown down by ammonia, and purified by solution in alcohol, in which it was highly soluble, and by boiling with animal charcoal. It was deposited on cooling in irregular needles, which nearly filled the fluid, and was shown to be papaverine, by its giving a blue colour with concentrated sulphuric acid and orange crystals of nitrate of nitropapaverine with nitric acid.* The granular precipitate was impure meconine. It was purified by solution in boiling water, which left a small quantity of an indifferent resin, and the fluid on cooling deposited an abundance of needle-shaped crystals, still retaining a yellowish shade of colour, which was easily removed by animal charcoal.

Composition of Meconine.

Meconine has been already analysed by its discoverer COUERBE, and by REGNAULT. The earlier analyses of the former† of which the details are not given in the original memoir, led to the formula $C_{18}\,H_9\,O_8$; but as these are admitted to have been erroneous, it is unnecessary to refer to them further. The results contained in his second memoir,‡ and those of REGNAULT,§ are collected in the following table, in which all are recalculated according to the atomic weight of carbon now in use.

* Although the salts of papaverine are highly crystallizable and sparingly soluble, the presence of resinous impurities in the opium fluid has a remarkable tendency to prevent their separating in the crystalline form; and we see that in this instance a certain quantity of the base had even resisted precipitation by ammonia. It is probably the same peculiarity which prevents its appearing as an impurity in the hydrochlorate of morphia, prepared by the process of ROBERTSON and GREGORY. The hydrochlorate of papaverine is less soluble than the hydrochlorate of morphia, and *a priori* we should expect it to be the first substance to deposit from the concentrated fluid, yet the commercial hydrochlorate of morphia does not contain a trace of it, the whole being retained in the mother liquor.

† Annales de Chimie et de Physique, 2d Series, vol. l., p. 347.

‡ Ibid., vol. lix., p. 140.

§ Ibid., vol. lviii., p. 157.

	Couerbe.				Regnault.		
Carbon, . . .	60·87	61·03	60·86	61·63	61·36	61·22	61·51
Hydrogen, . .	5·11	5·54	5·09	5·22	5·40	5·33	5·34
Oxygen, . . .	34·02	33·43	34·05	33·15	33·24	33·45	33·15
	100·00	100·00	100·00	100·00	100·00	100·00	100·00

numbers which agree very well with the formula $C_{10}\ H_3\ O_4$, given for meconine in the same paper. My own analyses completely confirm these results. The details are as follows:—

I. 5·405 grains of meconine, dried at 212°, gave
12·170 ... carbonic acid, and
2·495 ... water.

II. 5·445 grains of meconine, dried at 212°, gave
12·280 ... carbonic acid, and
2·515 ... water.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	61·40	61·50	61·85	C_{20}	120
Hydrogen, . .	5·12	5·13	5·15	H_{10}	10
Oxygen, . . .	33·48	33·37	33·00	O_8	64
	100·00	100·00	100·00		194

In the calculated numbers, I have assumed its formula to be the same as that of opianyl, and it will be immediately shown that the true constitution of both substances is actually so represented.

Properties of Meconine.

Meconine crystallizes from its solution in water in brilliantly white and very beautiful needles, which are arranged in branched tufts, in a highly characteristic manner. Its taste is slightly but distinctly bitter. It is soluble in water, alcohol, and ether, and its saturated hot solution in the first menstruum becomes filled, on cooling, with a net-work of crystals, which occupies the whole of the fluid. It requires for solution 22 times its weight of boiling water, and upwards of 700 times its weight of water at 60°. These numbers differ considerably from those of Couerbe, who finds it to be soluble in 18·5 parts of hot, and 265 of cold water. Three careful determinations on solutions made by boiling with excess of meconine, allowing the fluid to cool, and filtering from the deposited crystals, concurred in giving the former result; and in this respect meconine completely agrees with opianyl, which also requires 700 parts of cold water for solution. When dry meconine melts at 230°, and not at 194°, as stated by Couerbe. It fuses under water at a temperature of 170°, and it is possible that Couerbe may have been misled by this appearance, and determined its fusing point by heating it in water; but

there is no statement to this effect in the paper, nor does he mention how that employed for analysis was dried, so that it is impossible to ascertain how he made his experiment. When heated to a higher temperature it volatilizes completely, and is deposited on cooling in fine crystals. If the quantity be large, and the heat carelessly applied, a trifling carbonaceous residue is left. Meconine does not appear to dissolve more abundantly in the alkalies than water, and gives no compounds with the metallic oxides. Its effect on subacetate of lead was particularly examined, but no precipitate could be obtained by any mode of operating. Couerbe states that he obtained and analysed such a precipitate; but his description of the whole reaction is extremely unsatisfactory, his two analyses differ immensely, and there can be little doubt, that the substance he obtained was not a true chemical compound, but due to some impurity contained in the meconine he employed. Nitric acid, chlorine, bromine, and iodine, act upon meconine with the production of particular compounds, to be afterwards described. When treated in the cold with concentrated sulphuric acid, it forms a colourless solution, but on gently heating, a faint purple makes its appearance, which gradually increases in depth, as the temperature rises, until it becomes so intense, that a stratum of a quarter of an inch thick is quite opaque; and at length, if the temperature becomes too high, sulphurous acid is evolved, and the substance charred. When the purple solution is diluted with water, a dark-brown precipitate is thrown down, and a brown fluid obtained. The precipitate dissolves in alkalies, with a fine red colour, and appears to be a true colouring matter. Exactly the same phenomena are observed when opianyl is treated in the same manner. Couerbe obtained with meconine, a green colour, but I have never seen this when it was pure; although, if it be taken after its first crystallization, and while still yellow, I have found it to produce a dirty greenish purple colour, and Couerbe's results are no doubt to be attributed to the presence of some impurity.

The observations just made are sufficient to show that meconine and opianyl are actually identical; and if further proof be required, it is to be found in the properties of the substitution products of both substances, to be immediately described. It is clear, also, that the discrepancies in the characters of the former substance as described by myself, and those attributed to meconine, such as its melting point, precipitation of subacetate of lead and green colour with sulphuric acid, which caused me to hesitate in asserting their identity, are due to erroneous observations on the part of Couerbe.

In what follows, I propose to make use of the name of opianyl, which appears much more suited to the substance than that of meconine. For some reasons, perhaps, the name proposed by the original discoverer ought to be retained; but having been applied to it as an isolated substance, it must be considered as in some sense as a provisional name, and ought to give way to another, which points out its relation to opianic acid, and through it to narcotine, from which it can also

be obtained. I shall only employ the word meconine to distinguish that got directly from opium from that produced by the decomposition of narcotine.

Action of Nitric Acid on Opianyl.

The relation in formulæ of opianyl and opianic acid led me to anticipate the easy conversion of the one into the other; but this anticipation has not been confirmed by experiment. I had immediate recourse to nitric acid, as the readiest means of effecting the expected change; but in place of opianic acid there was formed a substitution product corresponding with COUERBE's hyponitromeconic acid, although neither its properties nor the mode of its preparation agree with his description.

Nitropianyl.—Opianyl dissolves abundantly in cold concentrated nitric acid, and a few red fumes are given off when heat is applied. On dilution the fluid becomes filled with bulky crystals, which are obtained pure by washing and solution in boiling spirit. Nitropianyl is obtained in fine white needles and prisms, sometimes of considerable length. It is very sparingly soluble in cold water, more so in boiling, and the solution on cooling deposits it in minute crystals. It is much more soluble in boiling alcohol, and is also dissolved by ether. All these solutions are perfectly colourless, and absolutely neutral to test-paper. It melts at 320° into a transparent fluid, which solidifies on cooling into a crystalline mass. Heated in small quantity on a platinum knife, it volatilizes almost completely, leaving only a trifling carbonaceous residue; but in a test-tube it decomposes suddenly, when the temperature rises to a certain point, leaving a bulky, porous charcoal. It is not precipitated by metallic salts, and possesses no acid properties. It does not dissolve in cold potash or ammonia more abundantly than in water; but on boiling, a larger quantity is taken up, the fluid becomes yellow, but nothing is deposited on cooling or on the addition of hydrochloric acid, and the nitropianyl obviously undergoes some change, which deficiency of material has prevented me from examining. Nitropianyl is insoluble in hydrochloric acid; strong nitric acid dissolves it readily in the cold, and deposits it in crystalline flocks on dilution. When treated with concentrated sulphuric acid the crystals immediately become yellow, and on the application of heat they dissolve with a red colour, which becomes more and more intense as the temperature rises. On dilution with water, the fluid acquires a dirty red colour, which is rendered darker by ammonia. The analysis of nitropianyl gave the following results:—

5·170 grains of nitropianyl, dried at 212°, gave
9·500 ... carbonic acid, and
1·840 ... water.

	Experiment.	Calculation.		
Carbon, . . .	50·11	50·20	C_{20}	120
Hydrogen, . . .	3·92	3·76	H_9	9
Nitrogen, . . .	...	5·85	N	14
Oxygen, . . .	...	40·19	O_{12}	96
		100·00		239

This leads to the formula $C_{20}\,H_9\,(NO_4)\,O_8$, which is exactly the same as that of the substance described by COUERBE under the name of hyponitromeconic acid. This substance, according to COUERBE, is an acid capable of combining with the alkalies, and yielding precipitates with the salts of iron and copper. It occurs in long yellow needles, gives yellow solutions with water and alcohol, but not with ether, and dissolves in the alkalies with a dark yellow, almost red colour, and is precipitated by acids. It is said to melt at 302°. These characters, though differing very remarkably from those of nitropianyl, approximate so closely in other respects that there can be no doubt about the identity of the two substances, although the discrepancies are somewhat puzzling. Some of them may, no doubt, be explained on the supposition that COUERBE's substance was impure; but others scarcely admit of any such explanation. This particularly applies to his description of the mode in which it was obtained. He says that nitric acid is to be boiled with meconine, and the fluid evaporated to complete expulsion of the acid, and the residue, on treatment with water, gives hyponitromeconic acid. I found, on the contrary, that the residue, which represented only a small quantity of the opianyl or meconine employed, was highly soluble in water, and the solution gave, on evaporation, an amorphous mass which dissolved in almost all proportions in alcohol and ether. In one instance only in which this process was tried, a small quantity of a crystalline matter was obtained. The soluble substance was not examined, as its properties were not such as to admit of easy purification. It is probably a further substitution product.

Action of Chlorine on Opianyl.

When a current of chlorine is passed into a cold saturated solution of opianyl in water, crystals soon begin to make their appearance, and gradually fill the whole fluid. In preparing this substance in large quantity, it is convenient to employ a warm solution, so that the bulk of the fluid may be smaller; but in this case care must be taken to stop the process before it has gone too far, as the crystals are apt to diminish, or even disappear, apparently owing to the formation of a farther product of decomposition, and the same precaution is necessary, though to a less degree, with the cold solution. The crystals are purified by solution in boiling alcohol. Chloropianyl may also be obtained by passing dry chlorine over fused opianyl. The gas is rapidly absorbed, hydrochloric acid is evolved, and the mass becomes gradually less and less fusible, so that the temperature of the oil-bath in which the tube is immersed must be slightly raised. After the action has gone on for some time, the current of gas must be stopped and the heat removed. If the action has been properly managed, the mass is highly crystalline, and on solution in boiling alcohol gives crystals identical with those obtained in the wet way. In this case, also, care must be taken to stop the action at the proper point, for if it be continued too long, no crystals are obtained, and the fluid solidifies into a resinous mass like Canada balsam.

Chloropianyl as obtained by either of these processes is in the form of transparent colourless needles, scarcely soluble in cold water, rather more so in boiling. Alcohol and ether dissolve it much more abundantly, and deposit it in fine crystals. It is not more soluble in the alkalies than in water, and they do not remove chlorine from it. Nitric acid dissolves it with a red colour, and on heating it is decomposed. It is soluble in cold oil of vitriol, and on heating, a fine greenish-blue solution is obtained, from which water throws down brown flocks, soluble in the alkalies with a red colour. Chloropianyl melts at 347 , and sublimes unchanged at a higher temperature. The properties of specimens prepared from opianyl and from meconine extracted directly from opium, were compared, and found to be perfectly identical. Of the following analyses, the first two were made with chloropianyl obtained from meconine in the dry way, and the third was from opianyl, and prepared in the wet way.

I. { 5·095 grains of chloropianyl, dried at 212°, gave
9·790 ... carbonic acid, and
1·985 ... water.

II. { 3·352 grains of chloropianyl gave
6·435 ... carbonic acid, and
1·280 ... water.

III. { 4·285 grains of chloropianyl gave
8·265 ... carbonic acid, and
1·620 ... water.

{ 4·950 grains of chloropianyl gave
3·039 ... chloride of silver.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . . .	52·40	52·35	52·60	52·51	C_{20}	120
Hydrogen, . .	4·32	4·21	4·20	3·93	H_9	9
Chlorine, . .	15·17	...	...	15·53	Cl	35·5
Oxygen, . . .	28·11	...	...	28·03	O_8	64
	100·00			10·000		228·5

This corresponds with the formula C_{20} H_9 Cl O_8.

The action of chlorine upon meconine has been examined by COUERBE, but his results differ entirely from those just described, and his statements are very loose, unsatisfactory, and in some points absolutely conflicting. He passed chlorine into fused meconine, and obtained a product containing 25·75 per cent. of chlorine, and consisting of crystals mixed with a resinous matter. The greater part of the chlorine exists in the latter, the former containing only 5·43 per cent., which is removed by oxide of silver, or by potash. The product is said to crystallize from alcohol in short prisms, to contain no chlorine, but to have a powerfully acid reaction, and to precipitate the salts of lead and copper. COUERBE designates it mechloic acid, and assigns to it the formula C_{14} H_7 O_{10}, which obviously bears no relation to that of the original substance, and is in itself impro-

bable. The alleged absence of chlorine must also be considered as in the highest degree questionable; indeed COUERBE appears to have relied entirely on the action of oxide of silver in removing the chlorine he found in the crude substance, and does not appear to have tested by heating with lime, and hence has overlooked the substitution chlorine which his substance undoubtedly contained. Nor is this surprising, for his paper was written at a time when the law of substitution was unknown, and little attention had been paid to the organic chlorine compounds.

I have already mentioned that if the current of chlorine is passed through fused opianyl for too long a time, no crystals are deposited, and a substance resembling Canada balsam is obtained, which is no doubt identical with the resinous matter which COUERBE got mixed with the crystals of his mechloic acid. This substance is insoluble in water, but highly soluble in alcohol and ether, and deposited in an amorphous condition when these menstrua are evaporated. When treated with potash it dissolves, and the fluid on saturation with an acid yields a pulverulent precipitate, soluble in alcohol, and giving irregular crystals on evaporation. This substance I at first suspected might be mechloic acid, but it is entirely different, contains abundance of chlorine, and is obviously a further substitution product, although it was not obtained of definite constitution. A single analysis gave—

4·540 grains, dried at 212°, gave	
8·295 ... carbonic acid, and	
1·535 ... water.	
Carbon,	45·82
Hydrogen,	3·75

This lies nearly half-way between the numbers of chloropianyl and those required for a product containing two equivalents of chlorine.

COUERBE gives for his resinous substance the formula $C_{10} H_5 O_8$; but if we assume it to have contained chlorine, his analysis would agree pretty well with the formula $C_{20} H_8 Cl_2 O_8$. His numbers, recalculated with the correct atomic weight of carbon, are given below, and compared with that formula.

	COUERBE.		Calculation.		
Carbon,	46·93	45·53	45·63	C_{20}	120
Hydrogen,	3·72	3·83	3·04	H_8	8
Chlorine,	...	...	26·99	Cl_2	71
Oxygen,	...	...	24·34	O_8	64
			100·00		263

It is highly probable that this substance actually did contain chlorine, but the approximation to the formula is possibly only fortuitous.

Action of Bromine on Opianyl.

When bromine water is dropped into an aqueous solution of opianyl, crystal-

line flocks of bromopianyl are immediately deposited, which are purified by solution in boiling spirit. Bromopianyl is thus obtained in colourless transparent needles, very sparingly soluble in water, much more so in alcohol and ether. It is rather more fusible than chloropianyl, its melting point being 332°; but in its general characters and relations to acids and bases it is so similar to that substance that the same words may serve to describe both. Its analysis gave—

5·105 grains of bromopianyl, dried at 212°, gave
8·202 ... carbonic acid, and
1·565 ... water.

4·200 grains of bromopianyl gave
2·883 ... bromide of silver.

	Experiment.	Calculation.		
Carbon,	43·81	43·95	C_{20}	120
Hydrogen,	3·40	3·29	H_9	9
Bromine,	29·21	29·30	Br	80
Oxygen,	23 58	23·46	O_8	64
	100·00	100·00		273

corresponding with the formula $C_{20}\ H_9\ Br\ O_8$.

Action of Chloride of Iodine on Opianyl.

Iodine, whether in solution in alcohol or in the solid form, is entirely without action on opianyl. I therefore tried whether the ingenious process described by Mr Brown for causing the substitution of iodine for hydrogen would succeed in this case, and the experiment proved completely successful, although the change was very slowly effected. When chloride of iodine was added to an aqueous solution of opianyl, no crystals were deposited at first, and the fluid retained its colour. It was placed upon the sand-bath, in a moderately warm place, where it was left for several days, at the end of which time crystals of an inch in length, and mixed with some iodine had deposited. The crystals were separated, and dissolved in boiling alcohol, which deposited them pure and colourless.

Iodopianyl is obtained in colourless needles of considerable size and great beauty, which are scarcely soluble in water, but dissolve in alcohol and ether. At 234° it fuses to a colourless liquid, which soon becomes brown, and at a higher temperature it is decomposed and iodine evolved. Nitric acid destroys it, with separation of iodine. Sulphuric acid dissolves it, and on heating a dark colour is produced. Its analysis gave—

6·993 grains of iodopianyl gave
9·530 ... carbonic acid, and
1·865 ... water.

4·735 grains of iodopianyl gave
3·460 ... iodide of silver.

	Experiment.	Calculation.		
Carbon, ...	37·16	37·48	C_{20}	120
Hydrogen, . . .	2·96	2·81	H_9	9
Iodine, ...	39·48	39·70	I	127·1
Oxygen, ...	20·40	20·01	O_8	64
	100·00	100·00		320·1

The formula is therefore $C_{20}\,H_9\,I\,O_8$.

Action of Peroxide of Lead and Sulphuric Acid on Opianyl.

I had fully expected that either opianic or hemipinic acids would have been produced by the action of nitric acid on opianyl; but having failed to obtain them, I had recourse to peroxide of lead as a convenient oxidizing agent. When opianyl is heated very gently along with that substance and sulphuric acid, an action takes place, carbonic acid is evolved, and an amorphous substance obtained in solution. Deficiency of material, however, prevented the extension of my experiments in this direction as far as was desirable; but having recently contrived a process by means of which opianyl may be obtained with great certainty from narcotine, I hope to return to the subject in a future paper. There can, I think, be no doubt that by the use of an oxidizing agent incapable of producing a substitution product, one or other of these acids must be obtained.

The preceding experiments having established the identity of meconine with a decomposition product of narcotine, it seemed natural to suppose that the former was not an original constituent of opium, but had been produced by the decomposition of the latter, either during the process of inspissation, or in the chemical treatment to which it had been submitted; and in that case, the mother liquors from which it was extracted ought also to contain cotarnine. As that base is not precipitated by ammonia, the probability was, that if present at all, it must have existed in the fluid along with opianyl, and ought to have been extracted from it by ether, but no satisfactory evidence of its presence could be obtained, the only basic substance contained in the ether, being the small quantity of papaverine which was separated in the manner already described. Nor can this excite surprise, for the difficulty there is in making cotarnine crystallize under any circumstances, and the extent to which its properties are masked by the presence of other substances, are such as almost to preclude the hope of extracting it from the large mass of indeterminate amorphous substances with which it must be mixed. Resinous matters possessed of basic properties, and giving precipitates with bichloride of platinum, and corrosive sublimate, were found along with thebaine, and abound in all parts of the opium mother liquor; but no special attention was paid to these, very few trials having shown that their purification would have been attended with great difficulties. It is possible that cotarnine may exist in some of them.

The relationship thus brought out between two substances which have been long described as isolated constituents of opium is entitled to much consideration, and is the first step towards the simplification of the chemistry of a substance so remarkable for its complexity. As a matter of speculation, the opinion has long been entertained, that when several well-defined substances exist in the same plant, they must bear some relation to one another, and it has not unfrequentlyhappened that this idea has been borne out by the formulæ assigned to them. Quinine and cinchonine, which were once supposed to differ only in the proportion of oxygen they contain, may be cited as striking instances; and it was commonly believed that these two substances must be related in the same manner as two different oxides of a metal, and by the proper use of oxidizing and reducing agents must be mutually convertible. But neither in this nor in any similar instance has the anticipated conversion been realized, except with harmaline and harmine, the two bases of the Peganum harmala, and it is somewhat remarkable, that theformer is converted into the latter by the oxidation of two equivalents of hydrogen, exactly as I have found to occur with narcotine, excepting that there is no splitting up into two products, as with the latter substance.

But although the conversion of other alkaloids has not hitherto been successful, there cannot be a doubt that the close approximation in the formulæ of substances occurring in the same plant, indicates some natural connexion, and it is important that all such relations, where they exist, should be kept distinctly in view. For this reason, I propose to direct attention to those which have been brought out by the investigation of the compounds of opium.

Between the two best known of these alkaloids, morphia, and codeine, a very simple relation subsists. Their formulæ are,

Morphia, ...	$C_{34}\,H_{19}\,NO_6$
Codeine, ...	$C_{36}\,H_{21}\,NO_6$
Difference, .	$C_2\,H_2$

and their relation is that of the two immediately adjoining members of a homologous series. Yet there is little, if anything, except their formulæ, to bear out the opinion that they actually are homologous. They show but little of that close similarity in properties, which frequently renders the separation and distinction of such substances so difficult; and especially in their relations to solvents, there are very marked differences. Still the connexion is worthy of mention; the more especially, as the experiments of Mr How* lead to the opinion that they are both nitryl bases.

Thebaine and codeine are likewise somewhat closely connected,—

Thebaine, ...	$C_{38}\,H_{21}\,NO_6$
Codeine, ...	$C_{36}\,H_{21}\,NO_6$
Difference, .	C_2

* Quarterly Journal of Chemical Society, vol. vi., p. 125.

The difference is here only two equivalents of carbon; but at present we know too little regarding the former base, to permit more than a simple reference to this difference. Again, comparing thebaine with papaverine,—

Thebaine, ...	$C_{38}\ H_{21}\ NO_6$
Papaverine, . . .	$C_{40}\ H_{21}\ NO_8$
Difference, .	C_2 O_2

we find them to differ by the elements of two equivalents of carbonic oxide.

Passing from these, which may be considered as forming a group of substances closely allied, at least in formulæ, to one another, we come to other bases nearly related among themselves, but which cannot be connected in any simple way with those just mentioned. Narcotine and narceine deserve the first notice.

Narceine, ...	$C_{46}\ H_{29}\ NO_{18}$
Narcotine, ...	$C_{46}\ H_{25}\ NO_{14}$
Difference, .	H_4 O_4

They differ, therefore, by four equivalents of water only, and narceine might be considered as a hydrate of narcotine. It appeared to me that it must be easy to ascertain whether the molecular condition of narceine admits of this construction; for in that case, it might be expected to undergo decompositions similar to those of narcotine. I accordingly tried the action of nitric acid upon it, but there was no production of opianyl or cotarnine, and though the decomposition was not followed up, enough was seen to lead me to the inference that most probably a substitution base was produced.

It is scarcely necessary to refer to the constitution of narcotine, and its relation to opianyl, further than to observe, that in my former memoir I have assumed it to be produced by coupling cotarnine with a substance, $C_{20}\ H_{12}\ O_8$, to which I have given the name of hydruret of opianyl. I recur to it here, because I am inclined to think that opianine is similarly constituted, although it must be admitted, that the unsatisfactory and incomplete information we at present possess regarding that base gives the opinion I am now about to hazard a somewhat speculative character.

HINTERBERGER,* in his preliminary notice announcing the discovery of opianine, assigned to it the formula $C_{66}\ H_{36}\ NO_{23}$, which is altered in the published paper† to $C_{66}\ H_{36}\ N_2\ O_{21}$. The difference between these two statements is attributed by HINTERBERGER to his having at first employed VARRENTRAP and WILL'S method of determining nitrogen, by which only half of that element is separated in the form of ammonia, and he consequently obtained only 2·22 per cent. of nitrogen; but in subsequently repeating its determination by DUMAS'S method, he obtained

* Annalen der Chemie und Pharmacie, vol. lxxvii., p. 207. † Ibid., vol. lxxxii., p. 320.

twice that quantity. If this be the case, opianine must be a base entirely unique in its properties; for in all the fixed alkaloids hitherto examined, the whole of the nitrogen is converted into ammonia with the utmost facility by ignition with soda lime. On this ground alone I should be inclined to question the accuracy of HINTERBERGER's results; and these doubts are increased by a more minute examination of his analyses. In two determinations by DUMAS's method, he obtains 4·12 and 4·41 per cent. of nitrogen, while the calculation for his formula requires 4·45. Now it is a familiar fact, that, owing to the impossibility of removing the last traces of atmospheric air from the tube, the determination of nitrogen as gas invariably gives a larger quantity than theory requires, so much so that an excess of a half per cent. is not uncommonly met with, even in the hands of the most careful experimenters. But in both of HINTERBERGER's experiments, the quantity falls short of theory, and in one of them to the extent of about 0·4 per cent. Partly on this account, and partly because his formula is at variance with the law of the divisibility of formulæ, I incline to the opinion that it cannot be correct, and that in all probability his first nitrogen determinations are more accurate, and to them I shall adhere. The details which HINTERBERGER has supplied us with regard to opianine and its salts are extremely meagre, and do not afford very decided grounds on which to found a formula; but weighing them all, the constitution of opianine appears to be best represented by $C_{66}H_{37}NO_{22}$. That this formula would agree almost as well with HINTERBERGER's experiments as that he has himself given, is at once obvious from the following comparison, in which all his results excepting the nitrogen determinations are contained.

Opianine.

	Experiment.	Calculation.	
		$C_{66}H_{36}N_2O_{21}$	$C_{66}H_{37}NO_{22}$
Carbon, . . .	62·99	63·06	63·56
Hydrogen, . .	5·69	5·73	5·93
Nitrogen, . . .	...	4·45	2·24
Oxygen, . . .	...	27·76	28·27
		100·00	100·00

Mercury Compound of Opianine.

	Experiment.	Calculation.	
		$C_{66}H_{36}N_2O_{21}$	$C_{66}H_{37}NO_{22}$
Carbon, . . .	49·14	49·50	49·81
Hydrogen, . . .	4·60	4·63	4·77
Nitrogen, . . .	...	3·50	1·76
Oxygen, . . .	...	21·00	22·16
Chlorine, . . .	9·31	8·87	8·93
Mercury, . . .	12·28	12·50	12·57
		100·00	100·00

Now if this be its formula, we have only to subtract from it an equivalent of hydruret of opianyl to have narcotine.

Opianine, ...	$C_{66}\,H_{37}\,NO_{22}$
Hydruret of opianyl, . .	$C_{20}\,H_{12}\,O_8$
Narcotine, .	$C_{46}\,H_{25}\,NO_{14}$

And pursuing this point, we should develop a remarkable relation between these two bases and narcogenine, for all these would be compounds of cotarnine and hydruret of opianyl in different proportions, as thus represented:—

Opianine, . . .	$C_{66}\,H_{37}\,NO_{22} = C_{26}\,H_{13}\,NO_6 + 2\,(C_{20}\,H_{12}\,O_8)$
Narcotine, . . .	$C_{46}\,H_{25}\,NO_{14} = C_{26}\,H_{13}\,NO_6 + C_{22}\,H_{12}\,O_8$
Narcogenine, . .	$2\,(C_{36}\,H_{19}\,NO_{10}) = 2(C_{26}\,H_{13}\,NO_6) + C_{20}\,H_{12}\,O_8$

The possibility of representing narcotine and narcogenine in this manner has been already suggested by WERTHEIM,* and derives great support from the researches contained in this and the previous paper; and should the view I have taken of the constitution of opianine be confirmed, these three will form a very remarkable and important group of bases. I have had no opportunity of examining Egyptian opium, from which alone opianine is obtained, and have been unable to submit my view to the test of experiment, but I have thrown it out as deserving the attention of any one who may have occasion to re-examine that substance.

The following is a table of the compounds analyzed in this paper:—

Papaverine, ...	$C_{40}\,H_{21}\,NO_8$
Nitropapaverine, ...	$C_{40}\,H_{20}\,(NO_4)\,NO_8$
Nitrate of Nitropapaverine, . . .	$C_{40}\,H_{20}\,(NO_4)\,NO_8 + HO\,NO_5$
Platinum salt of papaverine, . . .	$C_{40}\,H_{20}\,(NO_4)\,NO_8\,H\,Cl\,Pt + Cl_2$
Bromopapaverine, ...	$C_{40}\,H_{20}\,Br\,NO_8$
Hydrobromate of bromopapaverine, . .	$C_{40}\,H_{20}\,Br\,NO_8 + H\,Br$
Teriodide of papaverine, ...	$C_{40}\,H_{21}\,NO_8 + I_3$
Pentiodide of papaverine, ...	$C_{40}\,H_{21}\,NO_8 + I_5$
Opianyl (meconine), ...	$C_{20}\,H_{10}\,O_8$
Nitropianyl, ...	$C_{20}\,H_9\,(NO_4)\,O_8$
Chloropianyl, ...	$C_{20}\,H_9\,Cl\,O_8$
Bromopianyl, ...	$C_{20}\,H_9\,Br\,O_8$
Iodopianyl, ...	$C_{20}\,H_9\,IO_8$

In conclusion, I may state that in pursuing the investigation of the compounds of opium, I have ascertained that by treatment with sulphuric acid, opianic acid is converted into a true colouring matter capable of giving, with iron and alum mordants, all the colours produced by madder; and taking into consideration that the formula of alizarine, $C_{20}\,H_6\,O_6$, differs from that of opianic acid by the elements of four equivalents of water only, I think it is not impossible that the new colouring matter may actually prove identical with it. Should it do so,

* Annalen der Chemie und Pharmacie, vol. lxx., p. 71.

a very important and remarkable relation would be brought out between madder and opium. At the same time I must mention that my experiments are not as yet sufficiently advanced to enable me to speak with any degree of certainty on this point. I have also ascertained that apophyllic acid is an acid methylamide corresponding in its constitution with oxamic and tartramic acids, but containing methylamine in union with a non-nitrogenous acid. I have likewise contrived a process by which opianyl can be obtained from narcotine by a different decomposition from that described in my former paper, and with absolute certainty.

The full details of these and other matters required to complete the history of these compounds, I must reserve for a future paper.

XIII. — *On the Products of the Destructive Distillation of Animal Substances.* Part III. By Thomas Anderson, M.D., Regius Professor of Chemistry in the University of Glasgow.

(Read 17th April 1854.)

In the preceding parts of the investigation of the products obtained by the destructive distillation of animal substances, I have entered fully into the method of treating the raw material, and have shown the existence in it of not less than three different series of bases; one, that of which methylamine is the type; a second, of which picoline is an example; and a third series, not yet further examined, to which the provisional name of pyrol bases has been applied. Besides these, aniline is also met with, but whether as an isolated substance or accompanied by the other members of its series, cannot be determined, as none of them possess sufficiently distinctive reactions to permit their detection in a complex mixture.

To the series of which picoline is a member my attention has hitherto been specially directed, and chiefly owing to the interest attaching to these bases from their identity in composition with the corresponding members of the aniline series, aniline and picoline being the first instance in which the isomerism of two organic bases, of which we have now so many examples, was distinctly made out. In the second part of the investigation, three members of the series in question are described, namely—

Pyridine, ...	$C_{10} H_5 N$
Picoline, ...	$C_{12} H_7 N$
Lutidine, ...	$C_{14} H_9 N$

of which the two latter are isomeric with aniline and toluidine. It was further remarked, that the phenomena observed seemed to indicate that the members of this series present in Dippel's oil, did not terminate with lutidine, but that bases of higher atomic weight and boiling point manifestly existed in it. The object of the present paper is to show that this statement was well founded, by giving a description of another member of the series, and further to define their true constitution.

On pursuing the distillation of the different fractions of basic products obtained by the process described in the second part of this investigation, and distilling at temperatures above 305°, which is about the boiling point of lutidine, it was found that, when converted into platinum salts, the percentage of platinum gradually diminished as the boiling point rose. Taking advantage of the well-known empirical

law that the boiling points of homologous substances rise by 34° of Fahrenheit for every addition of $C_2 H_2$ to the atom, and from which the boiling points of pyridine, picoline, and lutidine do not greatly differ, I directed my attention to the portion of mixed bases boiling about 340°, in which it was reasonable to expect that the next base of the series should be found. But even after repeated rectifications, the base distilling at this temperature still gave a very powerful reaction of aniline, with chloride of lime, and the percentage of platinum in its double compound was but little lower than that of the lutidine salt, or at all events never reached the number required by theory for the higher base. Being convinced that the separation of two bases approximating so closely in their boiling points as aniline and the substance I expected to find, could not be effected by fractionated distillation, or at least only by an expenditure of time, labour, and material, altogether out of proportion to the importance of the object to be attained, I endeavoured to accomplish it by crystallization. Having observed that the other members of the picoline series gave highly soluble and even deliquescent oxalates, I conceived that by converting the mixed bases into salts of that acid, it would be easy to separate the rather sparingly soluble and highly crystallizable oxalate of aniline, and obtain the oxalate of the other base in a state of purity. But this expectation was not confirmed by experiment; for neither from the portion boiling about 340°, nor even from that collected at 360°, and corresponding, therefore, with the boiling point of pure aniline, could the slightest trace of crystallized oxalate of aniline be obtained, although both fractions gave the reaction of that base in the most powerful manner. The experiment was varied in every possible way, by the use of water, spirit, and absolute alcohol, but by allowing these fluids to evaporate spontaneously only a thick syrup was obtained, without the slightest indication of crystallization. Even the addition of ether to its alcoholic solution gave only a syrupy fluid, and no crystals; and I was forced to conclude, that even in the portion of the mixed bases corresponding to the boiling point of aniline, its quantity was so small in proportion to the other substances, that the properties of its salts were entirely masked by them.

Not succeeding in obtaining the pure aniline, and so separating it from the other base, the question came to be, how to get rid of the former substance in the best possible way. For this purpose I availed myself of the extreme stability of the bases of the picoline series mentioned in my former paper, which is so great that they resist even the action of strong nitric acid, by which aniline is entirely destroyed. When the base, boiling between 340° and 345°, is mixed rapidly and in large quantity with nitric acid, much heat is evolved, and a brisk action takes place; and if the portion boiling about 360° is employed, the action is so violent as to be almost explosive, and it is requisite to add the base drop by drop to the acid, which must be kept carefully cool. The acid fluid acquires a deep red colour, and on boiling, red fumes are abundantly evolved, accompanied by an odour resembling that of

bitter almonds. After the action has ceased, the fluid becomes muddy when mixed with water, and a thick reddish-yellow oil is deposited, which has exactly the odour of nitrobenzide, and resembles it in many of its properties. The quantity of this substance produced is by no means large, and it is evidently mixed with some resinous substance. Owing to this circumstance I have not been able to submit it to purification and analysis, so as to ascertain whether it really is nitrobenzide; but though that substance has not yet been obtained by a similar action on pure aniline, it is quite possible that it may be produced, and the reason why it has not hitherto been observed, is probably because no one has had occasion to sacrifice large quantities of aniline in this manner. The acid solution of the undecomposed base is passed through a wet filter, in order to separate the oil, and the fluid boiled for some time to expel the last traces. On saturation with potash, and distillation, an oily base passed over with the water, and collected on the surface. This base, on being converted into a platinum salt, was found still to give a result greatly above that required by theory for the substance of which I was in quest. On submitting it to distillation, it was found to commence boiling at about 320°, and hence to contain a large quantity of lutidine; and it was only the very last portion which gave a platinum salt corresponding with theory. It was clear that a large quantity of lutidine had been retained at a boiling point above that which naturally belonged to it, by the presence of aniline, and that substance being destroyed, it came over at its natural boiling point. The higher fractions of the oil containing aniline were therefore treated in a similar manner, and the undecomposed bases, which amounted to from a half to two-thirds of the original quantity acted on by nitric acid, were submitted to fractionated distillation. The product was found to spread over a considerable number of degrees, and a quantity of that collected between 340° and 345° was converted into a platinum salt and analysed, but the results indicated the presence of much lutidine. The product being still obviously impure, was submitted to a systematic fractionation, and it was observed that the thermometer remained remarkably steady about 354°. The portion boiling between 350° and 360° was collected apart, and after several rectifications, a fraction was obtained, which distilled entirely between 352° and 356°, and proved to be the pure base, to which I gave the name of collidine.

Collidine.

Collidine is obtained in the form of a transparent and colourless oil, which may be preserved for a long time in bottles only partially filled with it, without acquiring colour. A rod dipped in hydrochloric acid brought in contact with it, gives abundant white fumes. It is insoluble in water, and floats on its surface, without undergoing diminution. It dissolves a small quantity of water, which is readily separated by caustic potash. It is highly soluble in alcohol, ether, and the fixed

and volatile oils. It dissolves with great facility in the acids, but even when added in large excess, it does not neutralize them. It precipitates alumina, chromium, zinc, and peroxide of iron from their solutions, but gives no precipitate with baryta, lime, magnesia, manganese, or nickel. It throws down oxide of lead from the nitrate, but not from the acetate, a remarkable peculiarity, which it shares with methylamine and ethylamine. With corrosive sublimate, it forms a double salt, but from salts of the suboxide of mercury, it throws down the oxide. Its odour is strong, aromatic, and far from unpleasant. Its specific gravity is 0·921, and it boils at 354°. The following results were obtained by analysis,—

I. { 4·075 grains of collidine gave
11·800 ... carbonic acid, and
3·450 ... water.

II. { 4·079 grains of collidine, gave
11·800 ... carbonic acid, and
3·393 ... water.

III. { 4·124 grains of collidine, gave
11.980 ... carbonic acid, and
3·560 ... water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . .	78·97	78·89	79·22	79·33	C_{15}	96
Hydrogen, . .	9·40	9·24	9·58	9·09	H_{11}	11
Nitrogen, . .	11·63	11·87	11·20	11·58	N	14
	100·00	100·00	100·00	100·00		121

These numbers correspond with the formula C_{15} H_{11} N. Collidine forms, therefore, another member of the picoline series, and corresponds in constitution with the base described by Cahours, under the name of zylidine, in the aniline series; with which, however, it is isomeric only, and not identical, its properties being different in all respects.

The salts of collidine are, for the most part, highly soluble and deliquescent. When evaporated, they form uncrystallizable gummy masses, some of which, on standing, show traces of crystallization. They are soluble also in alcohol, but not in ether. The only highly crystallizable compounds, are the mercury and platinum double salts.

The mercury double salt is thrown down in the form of a curdy white precipitate, on the addition of a solution of corrosive sublimate to a solution of the hydrochlorate of collidine. It dissolves in boiling spirit, and is deposited, on cooling, in needles. It could not be obtained of definite composition.

Platinochloride of Collidine—is obtained, when strong solutions of hydrochlorate of collidine and bichloride of platinum are mixed. It is slowly deposited in the form of orange-yellow prisms or needles, according to the degree of concentration of the fluids. It is readily soluble in water, but insoluble in alcohol and ether. Its analysis gave the following results,—

I. { 6·013 grains of platinochloride of collidine gave
6·345 ... carbonic acid, and
1·937 ... water.

II. { 5·040 grains of platinochloride of collidine gave
5·360 ... carbonic acid, and
1·650 ... water.

I. { 5·620 grains of platinochloride of collidine gave
1·705 ... platinum,

II. { 4·535 grains of platinochloride of collidine gave
1·362 ... platinum.

III. { 5·097 grains of platinochloride of collidine gave
1·657 ... platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . . .	28·77	29·00	...	29·33	C_{16}	96
Hydrogen, . .	3·57	3·63	...	3·66	H_{12}	12
Nitrogen, . .	...	...	...	4·31	N	14
Chlorine, . .	...	...	...	32·54	Cl_3	106·5
Platinum, . .	30·33	30·03	29·89	30·16	Pt	98·7
				100·00		327·2

These results correspond with the formula C_{16} H_{11} N HCl $PtCl_2$ and entirely confirm the constitution of the base. The rest of its salts have not been particularly examined, as they did not present anything of interest.

Constitution of the Bases of the Picoline Series.

Having in this and the previous part of these researches, accumulated sufficient evidence of the existence of a class of bases isomeric with that of which aniline is the type, it became important to determine to which of the three classes of volatile bases they belong. For this purpose, pyridine, picoline, and collidine, were submitted to the action of iodide of ethyl. The experiments were carried out in considerable detail with picoline, but with the other two salts, no more was done than sufficed to substantiate the fact, that iodide of ethyl acted on them in a similar manner.

Action of Iodide of Ethyl on Picoline.

Anhydrous picoline and iodide of ethyl were mixed, in the proportion of one volume of the former to two of the latter, and sealed hermetically in a combustion tube. The two fluids mix readily, but if the tube containing them be gently heated, by plunging it for the space of half a minute into the water-bath an action takes place, attended with the evolution of much heat, the fluid becomes muddy, and separates into a thick oily stratum, which rises to the surface, and a more fluid one, which descends. On cooling, the former solidifies into a highly crystalline mass, and well-formed crystals appear in the latter, which consists of the excess of iodide of ethyl. Even without the application of heat, the action takes

place, though more slowly, and it is necessary to have them in contact for some days before the action is finished. When heat is applied, the action is complete in ten minutes, but in every instance the tubes were allowed to stand for twenty-four hours, so as to present the complete crystallization of the new compound. The tubes were then opened, the whole contents thrown upon a filter, and the crystals slightly washed with a mixture of alcohol and ether, pressed between folds of filtering paper, and dissolved in the smallest possible quantity of a mixture of boiling alcohol and ether. On cooling, the new substance is deposited in beautiful silvery plates. It is highly soluble in water, and though not deliquescent, it becomes slightly damp in moist air. Its aqueous solution, on evaporation, solidifies into a mass of crystals. It is readily soluble in alcohol, especially when boiling, and the hot solution on cooling becomes filled with crystals. It is less soluble in ether. It fuses below 212° into an oily fluid. Its analysis gave these results,

5·350 grains dried *in vacuo*, gave
7·570 ... of carbonic acid, and
2·375 ... water.

6·440 grains, dried *in vacuo*, gave by direct precipitation
6·065 ... iodide of silver.

	Experiment.	Calculation.		
Carbon, . . .	38·57	38·70	C_{16}	96
Hydrogen, . . .	4·93	4·83	H_{12}	12
Nitrogen, . . .	5·61	5·67	N	14
Iodine, . . .	50·89	50·80	I	126
	100·00	100·00		248

These results agree with the formula $C_{16}\ H_{11}\ N\ HI$, which is that of hydriodate of ethylopicoline.

On the addition of a few drops of caustic potash to the solution of this salt, no odour of a volatile base evolved, nor is there any separation of an oily layer, but the addition of a large quantity of strong potash, causes the precipitation of a viscid oil, which solidifies on standing for some hours into a mass of crystals, generally much coloured, and which proves to be the hydriodate partially altered by decomposition. When boiled with strong potash, a volatile base is slowly formed, which is a product of decomposition, and will be afterwards referred to. From these characters it is obvious, that ethylopicoline belongs to the ammonium class of bases, and hence, picoline itself, must be a nitryl base; and this being the case, the formula of the iodine compound might be written thus, $C_{16}\ H_{12}\ N + I$, representing it as the iodide of a base corresponding to ammonium, of which the constitution must be $C_{16}\ H_{12}\ N$, and whose oxide, $C_{16}\ H_{12}\ NO$, must exist in its oxygen acid salts. If we adopt the nomenclature proposed by HOFFMAN for the ammonium bases, we should have a very clumsy name for this substance, and I shall therefore continue to call it ethylopicoline, which, though not perfectly correct, is sufficiently distinctive.

Ethylopicoline, or rather its oxide, is readily obtained, by agitating the aqueous solution of the iodide with moist oxide of silver, when iodide of silver is precipitated, and the base obtained in solution. In performing this process, heat must be avoided, as oxide of silver decomposes the base at a high temperature, fine violet streaks appearing in the fluid, which rapidly acquires a deep crimson colour. The same change occurs, though more slowly, in the cold, especially if the oxide of silver be added in large excess, and it is therefore desirable, that the solution should be separated as rapidly as possible. If this be done, a colourless solution is obtained, having a faint peculiar odour, and highly alkaline properties. It restores the blue of reddened litmus, and gives an intense brown with turmeric; it has a powerfully caustic taste, and produces a soapy sensation when rubbed between the fingers. It absorbs carbonic acid from the air, precipitates alumina, and redissolves it when added in excess. From a solution of corrosive sublimate it throws down the oxide, and with the metallic salts generally, it reacts in precisely the same manner as potash or soda. On boiling, its solution acquires a deep red colour, and the odour of a volatile base becomes apparent. By evaporation *in vacuo*, a hard gummy mass, with a green metallic lustre is left behind, which gives a magnificent blood-red solution with water, and deliquesces when exposed to the air. At first I entertained the hope that this substance, though coloured, might, when submitted to analysis, give the results of ethylopicoline, a very few experiments, however, sufficed to show that it had undergone decomposition, and no attempt was made to analyse it, but attention was directed to obtaining such double salts as might serve to confirm the constitution of the base.

Platinochloride of Ethylopicoline.—In order to obtain this salt, nitrate of silver was added to the iodide as long as a precipitate was formed, which was separated by filtration, and the excess of silver thrown down by hydrochloric acid. The filtrate was then mixed with a strong solution of bichloride of platinum, and set aside. In the course of a few hours the salt was deposited in orange-red tabular crystals, of remarkable beauty, and often of considerable size. It is readily soluble in cold water, and still more so in hot, and is deposited unchanged from its solution. It possesses considerable stability, but by long-continued boiling, it undergoes decomposition. Its analysis gave,—

6·015	grains of	platinochloride of ethylopicoline gave
6·430	...	carbonic acid, and
2·040	...	water.

6·825	grains of	platinochloride of ethylopicoline gave
2·031	...	platinum.

6·910	grains of	platinochloride of ethylopicoline gave
2·058	...	platinum.

6·970	grains of	platinochloride of ethylopicoline gave
2·085	...	platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . . .	29·15	...	...	29·33	C_{16}	96
Hydrogen, . .	3·76	...	...	3·66	H_{12}	12
Nitrogen, . .	...	...	...	4·31	N	14
Chlorine, . .	...	...	...	32·54	Cl_3	106·5
Platinum, . .	29·75	29·78	29·91	30·16	Pt	98·7
				100·00		327·2

Aurochloride of Ethylopicoline.—This compound is readily formed, by adding a solution of chloride of gold to the nitrate, with excess of hydrochloric acid, obtained from the iodide, in the manner employed for the production of the platinum salt. It is slowly deposited in the form of golden-yellow flattened prisms of great beauty. It is sparingly soluble in cold water, readily in hot, and is deposited unchanged on cooling. It is insoluble in alcohol and ether. Ammonia converts it into a cinnamon-brown powder, and it is instantly blackened on the addition of potash to its hot solution. The specimen analysed, was dried at 212°, and burnt with chromate of lead.

{ 6·745 grains of aurochloride of ethylopicoline gave
{ 5·093 ... carbonic acid, and
{ 1·675 ... water.

{ 5·300 grains of aurochloride of ethylopicoline gave
{ 2·265 ... gold.

	Experiment.	Calculation.		
Carbon, ...	20·59	20·83	C_{16}	96
Hydrogen, . . .	2·75	2·60	H_{12}	12
Nitrogen, . . .	...	3·06	N	14
Chlorine, . . .	...	30·82	Cl_4	142
Gold, ...	42·73	42·69	Au	196·6
		100·00		460·6

Corresponding with the formula $C_{16}\ H_{12}\ N\ Cl + Au\ Cl_3$.

It has been already mentioned that though ethylopicoline is fixed and inodorous, its iodide cannot be distilled with potash, or the base itself boiled or even evaporated *in vacuo*, without undergoing a decomposition, attended with the evolution of volatile base. In the latter case the decomposition is slow, and even after the ebullition has been continued for some hours the odour is given off with undiminished intensity, till by long-continued boiling it at length becomes extremely faint although it does not altogether disappear. When the iodide is boiled with potash, the change is more rapid, and after three or four hours' boiling a considerable quantity of base is found in the receiver. The product has a pungent and putrid odour, fumes strongly with hydrochloric acid, and forms with it a salt entirely

soluble in absolute alcohol. Two analyses were made of the platinum compound of this base, the one from a portion collected at the commencement, the other towards the end of the distillation, which show that the product was of uniform composition throughout. The results were as follows:—

I. { 6·440 grains of platinochloride gave
2·430 ... carbonic acid, and
1·920 ... water.

II. { 11·775 grains of platinochloride gave
4·210 ... carbonic acid, and
3·457 ... water.

I. { 4·385 grains of platinochloride gave
1·705 ... platinum.

II. { 6·580 grains of platinochloride gave
2·575 ... platinum.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	10·29	9·75	9·55	C_4	24
Hydrogen, . . .	3·31	3·26	2·78	H_8	8
Nitrogen, . . .	...	...	5·99	N	14
Chlorine, . . .	...	...	42·39	Cl_3	106·5
Platinum, . . .	38·88	39·23	39·29	Pt	98·7
			100·00		251·2

Its formula, therefore, is C_4 H_7 N H Cl Pt Ch, and the base itself is ethylamine.

The base obtained by the distillation of the ethylopicoline alone was found to have the same composition, for 6·177 grains of its platinum salt gave 2·413 grains of platinum, equal to 39·06 per cent.

The decomposition which thus occurs is very remarkable, and differs entirely from that observed by HOFFMAN in the ammonium bases examined by him. The oxide of tetrethylammonium, for instance, is not decomposed when evaporated *in vacuo*. Even at 212° it undergoes no change until it becomes nearly dry, but then a base and a permanent gas are evolved, the former being triethylamine, and the latter olefiant gas. In this case, one out of the four ethyl atoms which the complex base contained is decomposed, and the other three remain with the ammonia in the form of a nitryl base; in fact we may fairly assume that the atom of ethyl added to the triethylamine to convert it into tetrethylammonium is decomposed, and the base which formed the starting-point of that action is regenerated. With methethylopicoline the case is different; we start, indeed, from a nitryl base, but in place of reproducing it in the decomposition, the atom of ethyl which has been added takes possession of the ammonia, and produces an amide base, leaving the radicals, which we must assume to have replaced the three atoms of hydrogen in the ammonia from which the picoline was originally produced, in some other form of combination. In another point, also, the decom-

position of ethylopicoline differs from that of tetrethylammonium. According to HOFFMAN, the latter base is entirely converted into triethylamine and olefiant gas; but ethylopicoline, even after long-continued boiling, gives an abundant residue on evaporation. The substance so obtained is amorphous, has an intense blood-red colour, and is a base forming a platinum salt insoluble in water. Although these experiments were made on a very small scale, and the slowness of the action rendered it impossible to say with certainty whether the decomposition was complete, this platinum compound was analysed, and the results were—

5·840 grains of the platinum salt gave
8·550 ... carbonic acid, and
2·390 ... water.

7·555 grains of the platinum salt gave
1·652 ... platinum.

Carbon, ...	39·92
Hydrogen, . . .	4·54
Platinum, . . .	21·86

From a single analysis such as this, it is impossible to deduce a formula; but it is obvious that a base, of much higher atomic weight than ethylopicoline has been produced, the farther examination of which must be deferred to a future paper, and which will probably lead to interesting results.

Action of Iodide of Ethyl on Pyridine.

When pyridine is treated with iodide of ethyl, the action, as might be expected, is in all respects similar to that which occurs with picoline. A homogeneous mixture is first formed, and then, on gently warming, the action takes place, with the evolution of much heat, and the hydriodate of ethylopyridine rises to the surface as an oily layer. The crystallization of this substance, as it cools, is an extremely beautiful phenomenon. Minute rhombs make their appearance here and there in the viscid fluid, where they increase in size so rapidly that they may actually be seen to grow; and in a successful operation they sometimes increase to the size of from a quarter to three-eighths of an inch in diameter in the course of half an hour. By and by the crystals come into contact with one another, and the fluid is converted into a solid crystalline mass. The crystals are removed from the tube, pressed in folds of filtering paper, and crystallized from a mixture of absolute alcohol and ether. They then form fine silvery plates, highly soluble in water, and slightly deliquescent; in alcohol and ether they are also extremely soluble, though less so than in water. With reagents, their behaviour is so exactly the same as that of the ethylopicoline salts, that it is unnecessary to enter into any details. By analysis the following results were obtained:—

{ 6·110 grains of hydriodate of ethylopyridine gave
{ 8·105 ... carbonic acid, and
{ 2·525 ... water.

{ 5·445 grains of hydriodate of ethylopyridine gave
{ 5·395 ... iodide of silver.

	Experiment.	Calculation.		
Carbon, . . .	36·17	35·89	C_{14}	84
Hydrogen, . . .	4·59	4·27	H_{10}	10
Nitrogen, . . .	5·70	6·04	N	14
Iodine, . . .	53·54	53·80	I	126
	100·00	100·00		234

The formula of the substance therefore is C_{14} H_{10} N I.

Ethylopyridine itself may be separated from the salt by the action of oxide of silver. It forms a highly alkaline fluid, which undergoes decomposition when heated, with the evolution of a base which is no doubt ethylamine, and agrees completely with it in properties, though the small scale on which the experiment was performed prevented my establishing this fact by analysis. It unites with acids, and forms salts, which are all crystallizable, and generally highly soluble. The platinum and gold salts are extremely beautiful compounds.

Platinochloride of Ethylopyridine.—This salt was prepared in the same manner as the corresponding ethylopicoline compound. It is sparingly soluble in cold water, and insoluble in a mixture of alcohol and ether. When slowly formed, it is obtained in beautiful garnet-coloured rhomboidal plates with bevelled edges, which are easily got of a quarter of an inch in diameter, even when operating on very small quantities. Its analysis gave—

{ 7·152 grains of ethylopyridine platinum salt gave
{ 6·905 ... carbonic acid, and
{ 1·885 ... water.

{ 6·435 grains of ethylopyridine platinum salt gave
{ 2·035 ... platinum.

	Experiment.	Calculation.		
Carbon, . . .	26·33	26·81	C_{14}	84
Hydrogen, . . .	2·92	3·19	H_{10}	10
Nitrogen, . . .	...	5·56	N	14
Chlorine, . . .	...	33·93	Cl_3	106·5
Platinum, . . .	31·62	31·51	Pt	98·7
		100·00		313·2

The formula of the compound is C_{14} H_{10} N Cl + Pt Cl_2.

The gold compound of ethylopyridine is obtained in fine yellow plates of extreme beauty, sparingly soluble in cold water, and readily decomposed in boiling, especially if an excess of chloride of gold be present. They were not analysed.

Action of Iodide of Ethyl on Collidine.

Iodide of ethyl and collidine react upon one another in the same manner as the bases already mentioned. An oily layer separates on heating the mixture, which refuses to crystallize on cooling. After removal from the tube in which the action was effected, and separation from the excess of iodide of ethyl, the fluid was allowed to stand for some time, but no crystals appeared. It was then exposed to cold, in the hope of inducing crystallization, but without success; and no better result followed the attempts made by dissolving in the smallest possible quantity of absolute alcohol, and adding ether. As the properties of the compound did not appear promising, no further experiments were made with it; but it was converted into a platinum salt, for the purpose of ascertaining whether the collidine had actually combined with ethyl. The process employed was the same as that used for preparing the ethylopicoline salt. A sparingly soluble and scarcely crystalline compound was obtained, the platinum of which was determined by the following experiment:—

5·855 grains of the platinum salt gave
1·618 ... platinum.

	Experiment.	Calculation.		
Carbon, . . .	...	34·06	C_{20}	120
Hydrogen, . . .	...	4·50	H_{16}	16
Nitrogen, . . .	...	3·68	N	14
Chlorine, . . .	...	29·98	Cl_3	106·5
Platinum, . . .	27·65	27·78	Pt	98·7
		100·00		355·2

This corresponds completely with the platinum salt of ethylocollidine, but as that substance did not appear likely to give results of interest, I contented myself with this experiment as a sufficient proof of its existence.

The experiments described in the preceding pages sufficiently establish the fact that picoline and its homologues must be considered as nitryl bases, that is to say, bases capable of taking up only one additional atom of ethyl or any similar radical, by doing which they are converted into fixed compounds, of the class designated ammonium bases. If this be their constitution, we must, according to the views at present entertained, assume that these bases are formed from ammonia by the replacement of its three atoms of hydrogen by as many different radicals. Of the exact nature of these radicals, the experiments we at present possess afford no data for drawing definite conclusions; but a moment's consideration suffices to show that they must be substances remarkable for the simplicity of their constitution. If we confine our attention to pyridine, as the fundamental

member of the series, it is obvious that the ten equivalents of carbon and five of hydrogen which it contains must be distributed among these three substances; and although we cannot, without further researches, determine how they are distributed, it is at least sufficiently obvious that the choice among different speculative arrangements is by no means large. In fact, our knowledge of the laws governing the constitution of organic compounds, enables us to see that the total number of possible permutations† of the elements of pyridine is only eight. They are as follows:—

$$\overset{*}{\left.\begin{matrix}C_4H_2\\C_4H_2\\C_2H\end{matrix}\right\}}N \qquad \left.\begin{matrix}C_4H_3\\C_4H\\C_2H\end{matrix}\right\}N \qquad \left.\begin{matrix}C_4H\\C_4H\\C_2H_3\end{matrix}\right\}N \qquad \left.\begin{matrix}C_4H\\C_4H_2\\C_2H_2\end{matrix}\right\}N.$$

$$\overset{*}{\left.\begin{matrix}C_6H_2\\C_2H_2\\C_2H\end{matrix}\right\}}N \qquad \overset{*}{\left.\begin{matrix}C_6H_3\\C_2H\\C_2H\end{matrix}\right\}}N \qquad \left.\begin{matrix}C_6H\\C_2H\\C_2H_3\end{matrix}\right\}N \qquad \left.\begin{matrix}C_6H\\C_2H_2\\C_2H_2\end{matrix}\right\}N.$$

Involving the existence of the following nine radicals, all, with the exception of methyl, at present unknown:—

C_6H_3	C_4H_3	C_2H_3
C_6H_2	C_4H_2	C_2H_2
C_6H	C_4H	C_2H

Of these, two at least, C_6H and C_4H, are so extremely improbable, that we may, without much hesitation, pronounce against them; and if so, the probable formulæ of pyridine are reduced to those marked with an asterisk. The question for consideration is, whether even these can be supposed to represent the constitution of the base in a feasible manner. On this point no experimental evidence can at present be adduced; but taking into account all the circumstances connected with them, my impression is, that none of them give the rational expression of its constitution, and that pyridine and its homologues belong to a class of bases of which we have as yet no other examples.

In illustration of this opinion, it is necessary to enter into some details regarding the constitution of the bases generally. It is scarcely necessary to remind the reader that when HOFFMAN described his two new series of volatile alkaloids, he applied to those already known the name of amide, and to the new series those of imide and nitryl bases. This nomenclature, which has been more than once employed in the preceding pages, was founded on the analogy in constitution of those substances with the well-known amides, imides, and nitryls. A very little consideration, however, suffices to show that this analogy is by no means complete. The first series of bases may be correctly compared to the amides, but the other two have no close resemblance to the imides and nitryls. On the contrary, they are strictly comparable with the secondary and tertiary

† I assume, with GERHARDT, that the number of atoms of carbon in any radical must always be divisible by two.

amides recently described by GERHARDT and CHIOZZA, which are formed from the primary amides by a process similar to that employed by HOFFMAN to produce his two classes of bases. The closeness of this analogy may be seen from the subjoined comparison of these methyl bases with the benzoyl amides.

Methylamine.	Bimethylamine.	Trimethylamine.
$\left.\begin{matrix} C_2\,H_3 \\ H \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\}N$
$\left.\begin{matrix} C_{14}\,H_5\,O_2 \\ H \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} C_{14}\,H_5\,O_2 \\ C_{14}\,H_5\,O_2 \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} C_{14}\,H_5\,O_2 \\ C_{14}\,H_5\,O_2 \\ C_{14}\,H_5\,O_2 \end{matrix}\right\}N$
Primary Benzamide.	Secondary Benzamide.	Tertiary Benzamide.

From which we see that in every case hydrogen is replaced, atom for atom, by a compound radical, the only difference being, that in the one set of substances the ammonia retains, in the other it loses, its basic properties.

But the constitution of an imide or a nitryl is materially different. Of the former, indeed, we know too little to admit of any satisfactory conclusions regarding their constitution; but taking benzonitryl with the formula $C_{14}\,H_5\,N$, as an example of its class, and examining its constitution in the same point of view, we may consider it as an ammonia, in which three atoms of hydrogen have been replaced by a single radical $C_{14}\,H_5$. While, therefore, an amide is formed by the replacement of one or more atoms of hydrogen in ammonia by an equal number of molecules of a monobasic radical, a nitryl may be viewed as an ammonia with its three atoms of hydrogen replaced by one atom of a tribasic radical; and in the same manner there must exist a class of compounds, which for the present we may call imides, although they are not comparable with the substances known under that name, in which part of the hydrogen is replaced by a bibasic radical. The different forms of combination possible under this view may be best rendered intelligible if we make use of general formulæ, and take X′, X″, and X‴ as representing respectively a monobasic, a bibasic, and a tribasic radical. We have then the following expressions for the different classes:—

(1.)	(2.)	(3.)	(4.)	(5.)	(6.)
$\left.\begin{matrix} X' \\ H \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} X' \\ X' \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} X' \\ X' \\ X' \end{matrix}\right\}N$	$\left.\begin{matrix} X'' \\ H \end{matrix}\right\}N$	$\left.\begin{matrix} X'' \\ X' \end{matrix}\right\}N$	$X'''\}N$

Of these the first three represent either the amides, or the bases described by WURTZ and HOFFMAN; the last is a nitryl, and the others are substances at present scarcely known.

Now as regards the first three classes, it is manifest that they prove amides or bases, according to the properties of the radicals replacing the hydrogen; and we may fairly argue from analogy that the members of the last may be also either basic or non-basic. The nitryls at present known are all non-basic, but it is my belief that the most probable explanation of the constitution of the bases of the

pyridine series is to suppose that they are true basic nitryls, and that, for instance, in pyridine itself, the tribasic radical $C_{10} H_3$ replaces three atoms of hydrogen in ammonia. The opinion thus expressed regarding the constitution of these bases, and even the possibility of such compounds existing, is speculative, but at the same time it is not altogether unsupported by facts, for though we have no bases in which a tribasic radical exists, there certainly are instances in which two atoms of hydrogen are replaced by a bibasic radical. A marked example is found in GERHARDT's platinamine, although there the replacing substance is not a compound but a simple radical. Its formula may be written thus:—

$$\left.\begin{matrix} Pt \\ H \end{matrix}\right\} N$$

in which platinum is a bibasic radical replacing two equivalents of hydrogen. Diplatinamine may in the same manner be represented, with its formula written thus:—

$$\left.\begin{matrix} Pt \\ H_4 \end{matrix}\right\} N_2$$

in which two equivalents of ammonia have been brought into play. Lastly, in furfurine we have a purely organic base, formed from two equivalents of ammonia by replacement of the whole of its hydrogen by three atoms of a compound radical, its formula being—

$$\left.\begin{matrix} C_{10} H_4 O_2 \\ C_{10} H_4 O_2 \\ C_{10} H_4 O_2 \end{matrix}\right\} N_2$$

The view now expressed would make the constitution of the bases correspond very closely with that of the acids, as explained by GERHARDT. According to that chemist, a monobasic acid is formed from one atom of water (viewed as $H_2 O$) by replacement of hydrogen by a monobasic radical, while a bibasic acid is formed from two atoms of water, by the replacement of two atoms of hydrogen by a bibasic radical.

I have been led into these observations by a desire to explain in a more satisfactory manner than our present knowledge of the bases will permit, the constitution of pyridine and its homologues; but I am now about to enter upon a series of experiments, with a view of obtaining some of the bases

$$\left.\begin{matrix} X'' \\ H \end{matrix}\right\} N \qquad \left.\begin{matrix} X'' \\ X'' \end{matrix}\right\} N \qquad \left.\begin{matrix} X''' \end{matrix}\right\} N$$

the probable existence of which I have now indicated on theoretical grounds, which may probably form the subject of a future communication.

I may further mention, that I have found that the platinum salts of pyridine and picoline undergo a peculiar decomposition when boiled, platinum bases of very remarkable constitution being formed. I am extending this investigation to the other bases, and hope that my experiments will, at no distant date, be sufficiently advanced for publication.

XIV.—*Further Experiments and Remarks on the Measurement of Heights by the Boiling Point of Water.* By James D. Forbes, D.C.L., F.R.S., Sec. R.S.Ed., &c., Professor of Natural Philosophy in the University of Edinburgh. (With a Plate.)

(Read 4th December 1854.)

In 1843 I presented a paper to the Royal Society of Edinburgh, giving an account of experiments made on the boiling point of water in the Alps, under various barometric pressures. My object was twofold: first, to describe an apparatus which I considered more practically available than those previously in use; and, secondly, to give a simple, and, as I believed, new formula for computing heights from such observations.

With reference to the second point, I became aware, some time after the publication of my paper, that Sir John Leslie had proposed to compute heights by the thermometer, assuming the change of the boiling point to be *exactly* in proportion to the height ascended. While cheerfully conceding to Sir John Leslie priority on this point, I submit that he did not bring forward experiments to justify its practical adoption.

Of late years both the instrument and the formula have been objected to by M. Regnault of Paris, and the latter by Dr Joseph Hooker, who finds that it does not correctly represent his Indian observations. This has caused me to examine the whole subject, and also Dr Hooker's observations on the boiling point, with the particulars of which he has kindly furnished me, and I proceed to lay the details before the Society.

In 1843, when I wrote, the method of determining heights by boiling water had fallen very much into abeyance, principally owing, as I believed, to the inconvenient instruments employed, and partly to the uncertainty of the deduction of heights. As the thermometric method is principally valuable when barometers cannot be safely transported, and must always be inferior in accuracy to good barometric results, my intention was to do a service to physical geography, by introducing a convenient and effective instrument, by means of which water could be certainly made to boil even in untoward circumstances, and the temperature ascertained, not to the illusory nicety of two or three decimals of Fahrenheit's degree, as Archdeacon Wollaston attempted, but to within about $\frac{1}{20}$th of a degree, corresponding to about 25 feet of elevation, which I stated as the *utmost* degree of accuracy which I expected to attain, even in favourable

circumstances.* The formula of one degree of lowering of the boiling point for 550 feet of elevation, in an atmosphere at 32°, I stated to represent my observations quite sufficiently, and better than DALTON's Table of the Elasticity of Vapour, which was the one then commonly in use. I refer to my former paper for a description of the boiling apparatus, consisting of a thin copper pan heated by a "Russian furnace," having a powerful jet of inflamed alcoholic vapour. The thermometer (contrary, I believe, to the usual practice) had its bulb in the water, not in the steam.

In 1844 M. REGNAULT published in the Annales de Chimie a table of the elasticities of vapour at moderate temperatures, and a comparison with some boiling water experiments in Switzerland and the Pyrenees. He also contrived (I am not sure at what date) a small apparatus for the use of travellers, somewhat resembling Archdeacon WOLLASTON's.

In 1845 he published a second paper on the same subject in the same Journal, in which he quotes my observations, which he rejects as not conforming to his law of Elasticities of Steam, and attributes their discrepancy to faults in the boiling apparatus, and to errors of graduation of the thermometer.

The slightest comparison of M. REGNAULT's paper with mine, shows, however, that the discrepancies complained of do not argue anything against the accuracy either of his Table of Elasticities, or of my mode of observing; but they disappear almost entirely when the correction for the index error of the thermometer I used is applied to the temperatures observed. This index error (0°62 in excess) is given in my paper (page 414), and, of course, should have been applied when it was intended to compare *absolute* temperatures with *absolute* pressures, but had not been used by me when my object was merely to ascertain the *relative* variation of those quantities, as in page 412. When the index correction is applied, the deviations of my observations from M. REGNAULT's Table fall, as will be immediately seen, considerably within those of M. MARIÉ quoted by him, of which he says that they "s'accordent avec la formule aussi bien qu'on peut le désirer."†

To the observations of 1842, made in the Alps, and published in my former paper, I have now added a fresh series, made in 1846, with the same apparatus and thermometer, which confirms them in a remarkable manner. These series are denoted by I. and II. in the following Table. They were projected in the manner described in my former paper, the ordinates being the temperatures, the abscissæ the logarithms of the corresponding observed barometric pressures, which numbers are proportional to the heights in an atmosphere of uniform temperature. Through the points a straight line may be drawn, with a very close approximation (See Plate III., fig. 1). The deviations of the observations from this

* Transactions of the Royal Society of Edinburgh, vol. xv., p. 411.
† Annales de Chimie, 3me Serie, vol. xi., p. 332.

Ed. Royal Society Trans. Vol. XII. p. 235

DETERMINATION OF HEIGHTS BY THE BOILING POINT.

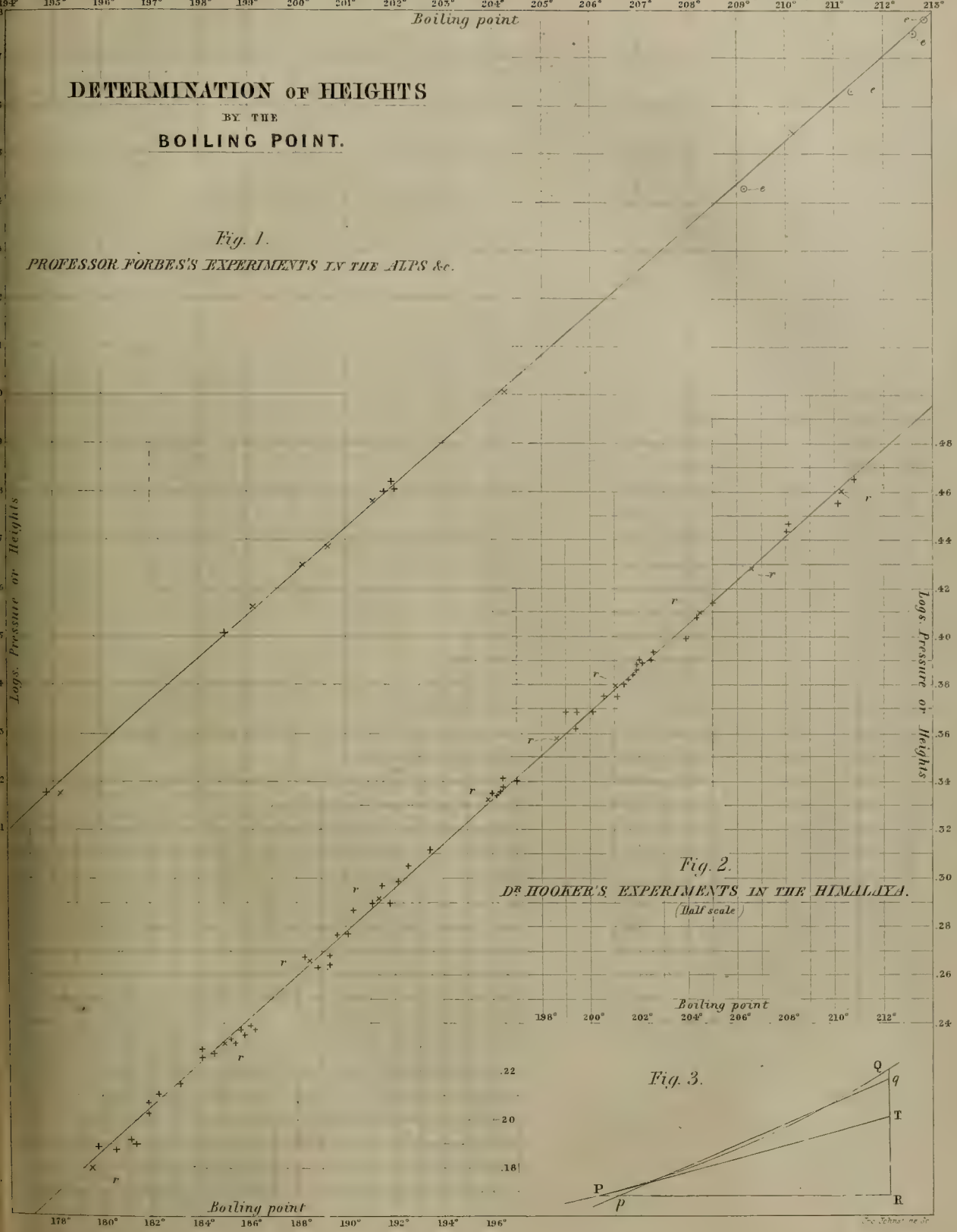


Fig. 1.

PROFESSOR FORBES'S EXPERIMENTS IN THE ALPS &c.

Fig. 2.

Dr HOOKER'S EXPERIMENTS IN THE HIMALAYA.

(Half scale)

Fig. 3.

NATURAL HISTORY

simple law are given in the 6th column of the following Table. Column (7) gives the temperature from M. REGNAULT's Table in the *Annales de Chimie.*

SERIES.	Barometer corrected and reduced to 32°.		Temp. Boiling Water Observed*	Linear Formula.	Difference.	REGNAULT's Formula.	Difference.	LOCALITY.
	Millim.	Eng. Inch						
I.	527·5	20·79	194°·50	194°·28	+0°·22	194°·18	+0°·32	Col Collon.
II.	528·05	20·79	194·25	194·33	−0·08	194·24	+0·01	Aiguille du Moine.
II.	569·0	22·40	197·90	197·94	−0·04	197·78	+0·12	Breven.
I.	575·9	22·67	198·43	198·51	−0·08	198·37	+0·06	St Bernard.
I.	588·1	23·15	199·45	199·52	−0·07	199·39	+0·06	Tacul.
I.	593·2	23·35	199·95	199·94	+0·01	199·80	+0·15	Do.
I.	606·9	23·89	200·93	201·04	−0·11	200·90	−0·03	Prarayon
II.	609·3	23·99	201·15	201·24	−0·09	201·07	+0·08	Montanvert.
II.	610·15	24·02	201·35	201·31	+0·04	201·15	+0·20	Do.
II.	612.3	24·105	201·30	201·47	−0·17	201·33	−0·03	Do.
I.	638·6	25·14	203·55	203·51	+0·04	203·36	+0·19	Naversch (Gressonay.)
II.	675·0	26·57	204·60†	...	...	...	...	Chamouni.
I.	723·6	28·49	209·47	209·54	−0·07	209·53	−0·06	Martigny.
(1.)	(2.)	(3.)	(4.)	(5.)	(6.)	(7.)	(8.)	(9.)

* Corrected for Index Error +0°·65.
† Evidently a mistake; the error amounting to nearly 1½°.

The following comparisons with the same apparatus and a different barometer, I find recorded as made at Edinburgh in January 1843.

Barom. corrected.	Boiling Point corrected.
27·760	208·57
29·040	210·72
29·879	211·95
30·064	212·18

If we project the Alpine observations alone (which are the most consistent with one another), in the manner exemplified in my former paper, as is done in Plate III, fig. 1, I find that they may be admirably represented by a straight interpolating line (giving the results recorded in column (5) of the preceding Table), and yield the following results.

The *uncorrected* reading of the thermometer, at the standard pressure of 30 inches, is 212°·75; at a pressure of 760 millimetres (29·922 inches), 212°·62, The Edinburgh observations (distinguished by the letter *e*), taken separately, give almost the same results.

The Alpine observations above give a uniform rate of ascent of 543·2 feet for 1° of fall in the boiling point. This is when we use LAPLACE's barometric coeffi-

cient for a standard atmosphere at 32°. It is about 6 feet less for 1° than I formerly deduced. If we include the *whole* of the observations (Edinburgh and the Alps), the rate of ascent will not be sensibly altered, though the squares of the errors will be somewhat increased.

The coincidence of the formula with observation, shewn in column 6, is highly satisfactory. In only three instances does it exceed a tenth of a degree of FAHRENHEIT.

The coincidence with M. REGNAULT's Table, shown in column 8, is not less satisfactory (a very slight change in the index error* would neutralize the preponderance of positive errors).

From the mode of experimentally obtaining his results, M. REGNAULT's Table is *in fact a table of boiling points* (see *Ann. de Chimie*, 1844), which was not the case with DALTON's Table, which differs from it sensibly: and I have no doubt that M. REGNAULT's are, on the whole, the most accurate numbers we at present possess.†

Hence,

1. Observations of height by the thermometer down to about 190°, or for elevations not exceeding 12,000 feet, may, within the usual limits of error, be reduced *indifferently* by M. REGNAULT's Table, or by my arithmetical proportion.

2. The method of placing the thermometer in water instead of steam, and of using a powerful alcoholic furnace, which may be removed to one side until the escape of steam becomes uniform and moderate, ‡ appears to give remarkably steady and consistent results. The graduation of the thermometer, as regards the length of the degree (which was entrusted to Mr ADIE, and on which M. REGNAULT throws doubts), is sensibly correct.

3. 543·2 feet per degree seems to express the observations better than 549·5, formerly given.

Let us now turn to Dr HOOKER's observations. In his "Himalayan Journals," vol. ii., p. 456, speaking of his numerous observations on the boiling point at a

* The reason for this change in the index error, according to the two hypotheses, will be seen in the concluding paragraph of this paper.

† I still retain the doubt expressed in my former paper, as to whether the boiling point can be taken correctly to represent the temperature of steam whose elasticity is that of the atmospheric pressure at the time. This doubt is confirmed by the difference of M. REGNAULT's and MAGNUS's Tables of Elasticity, as also by experiments of a different kind. I take this opportunity of adding, that I have obtained true ebullition of water in an exhausted receiver at the low temperature of 46°; the syphon-gauge then stood at 0·25 inch, being ·06 below the elasticity of vapour, at that temperature, as given by M. REGNAULT.

‡ The superabundance of heating power, and the mass of liquid in ebullition, I consider very important to the good result. No other portable apparatus that I am aware of, gives so ready means of adapting the force of the flame to the circumstances of the case. With a common spirit-lamp in fixed position below a boiler, it is next to impossible to regulate the rate of boiling, especially in an exposed situation. Mine is also the only instrument, so far as I know, which can be used in a gale of wind.

great range of elevation, he states, that having deduced the heights by my formula, he finds that it "is certainly not applicable to the Sikkim Himalaya," but that Captain BOILEAU'S Tables, founded on M. REGNAULT'S, give, at all ordinary elevations, a "very close approach to accuracy, on the mean of many observations." We find, however, from the results which Dr HOOKER has himself given at page 458, the average difference of the barometric and thermometric results is far from showing a close agreement, and seems to throw a doubt on the sufficiency of the observations for testing one formula rather than another.

The *average* error (without respect to sign) of each observation is about 140 feet, or 0°·26 of a degree, whilst it occasionally rises to half a degree, or even a whole degree; and this by the formula preferred by Dr HOOKER. The *average* deviation of my observations from the formula is only 0°·08.

Desiring to arrive at the exact truth, and to test the alleged inapplicability of a linear formula to the Indian observations, I wrote to Dr HOOKER, requesting him to send me as large a selection as possible of *direct* comparisons between the barometer and the boiling point at different heights. With this request he most kindly complied, sending me (with much trouble to himself) a long series of comparisons, which he had drawn from his original note-books. From the extent and range of these, I consider them well worthy of preservation, and accordingly transcribe them in the following Table.

Sikkim, Himalaya, Boiling Points.

STATION.	Boiling Point. S.*	L.*	Barom. Corrected.
Great Rungeet River, .	210·8	...	29·211
Bhomsong, . . .	210·2	...	28·559
Gt. Rungeet Guard-house,	208·4	207·8	27·972
Do., .	208·4	207·7	27·781
Choongtam, . . .	202·5	202·6	24·697
Dengha, . . .	200·6	...	23·726
Mr MULLER'S (Dorjiling),	...	199·0	23·372
Do., . .	199·6	199·5	23·358
Dr CAMPBELL'S (Dorjiling),	200·1	200·1	23·369
Mr HODGSON'S (Dorjiling),	199·5	199·3	23·030
Sinchul, . . .	197·0	...	21·892
Lachoong Village, .	196·4	...	21·928
Do., . . .	196·4	...	21·751
Lamteng, . . .	196·3	...	21·654
Zemu Samdong, . .	195·9	196·1	21·605
Do., . . .	196·3	196·2	21·596
Do., . . .	196·3	196·1	21·633
Mainom, . . .	193·4	...	20·480
Junction of Zemu & Thlonok,	192·6†	192·6	20·212
Tallum Samdong, .	191·4	191·6	19·758
Do., . . .	192·1	192·2	19·881
Yeumtong, . . .	191·1	191·2	19·490
Do. . . .	191·8	...	19·505

* S means small and L large Thermometer.

† 193·6 in my "Journals," by error apparently; for I find 192·6 in my Note-book and Calculation-book.—J. D. H.

Sikkim Himalaya, Boiling Points—(continued.)

Station.	Boiling Point. S.	Boiling Point. L.	Barom. Corrected.
Zemu River,	190·6	190·2	19·386
Tungu,	189·5	...	18·869
Do.,	189·5	...	18·974
Do.,	190·1	...	18·952
Jongri,	188·8	...	18·356
Zemu River,	188·5	...	18·507
Lachee-pia,	185·7	...	17·267
Do.,	186·1	186·2	17·317
Momay Samdong,	186·0	...	17·221
Do.,	185·9	...	17·215
Do.,	186·5	186·2	17·230
Palung,	185·6	185·4	17·062
Do.,	185·3	185·2	17·091
Kongra Lama,	184·1	...	16·959
Snow-bed above Yeumtong,	184·6	...	16·881
Tunkra Pass,	184·1*	...	16·817
Yeumtso,	183·2	183·1	16·385
Donkia,	182·4	...	16·235
Mt. above Momay,	181·9	...	16·106
Sebolah Pass,	181·9	...	15·928
Kinchin-jhow,	181·0	...	15·919
Donkia (W. flank),	180·6	...	15·376
Do. (S. flank),	179·9	...	15·442
Bhomtso,	...	181·2	15·548
Donkia Pass,	181·0	181·2	...
Do.,	181·6	181·3	15·489

* 164·1 by misprint in my Journals.—J. D. F.

Khasia Mountains Boiling Points.

Station.	Boiling Point. S.	Boiling Point. L.	Barom. Corrected.
Churra,	204·3	204·4	25·596
Amwee,	205·1	...	25·981
Nurtiung,	205·0	...	25·913
Nunklow,	203·9	...	25·083
Kala Panee,	202·1	...	24·492
Do.,	202·5	...	24·559
Do.,	202·0	...	24·556
Do.,	202·6	...	24·668
Myrung,	201·9	...	24·453
Do.,	202·0	...	24·386
Syong,	201·8	...	24·219
Do.,	201·9	...	24.319
Moflong	201·0	201·1	23·936
Do.,	201·5	...	24·009
Do.,	201·7	...	24·133
Do.,	202·0	...	...
Chillong,	201·2	...	23·727

Upon carefully projecting Dr Hooker's results in Plate III., fig. 2, in the same manner as my own, that is, by exhibiting the logarithms of the pressure (or the heights) in terms of the temperatures of boiling water, I found, in the first place, large deviations among the results, increasing also at great elevations. The breadth of the space is so considerable over which the individual observations are distributed, that it seems impossible, from the observations only, to assign any one curve as particularly indicated by them; and for the most part they are as well represented by a straight line as by any curve not absolutely sinuous. I must, however, note that below the temperature of 187°, or at heights above 13,600 feet, something like a dislocation occurs in the continuity of the observations. Dr Hooker was aware of this circumstance, and ascribes it to "the metal of the kettle, and consequently of the thermometer, getting heated above the temperature of the boiling water." Whatever may have been the cause, this part of the series, the most important for testing a formula, can hardly be relied on for that purpose.

There is no doubt that M. Regnault's numbers represent, as well as any numbers can be expected to do, the main features of Dr Hooker's observations.* But the differences between M. Regnault's numbers and my approximate formula are trifling, compared to the latitude of error which the projection of the observations themselves discloses. Balancing the errors as nearly as possible, the observations between 212° and 190° are well represented by a line which gives 538 feet of ascent for a fall of one degree in the boiling point, which it will be seen differs only by $\frac{1}{100}$th part from the corrected result of my Alpine observations.† I beg to observe, that this coincidence is the more striking, because, from the method of projection used, it was *impossible* to guess at the numerical result until the interpolating line had been fixed upon.

In criticising Dr Hooker's results, I do so with every feeling of courtesy and respect, in the same spirit, in short, in which I am sure he found it necessary to state his objection to my formula. The whole of his *barometrical* observations appear to have been made with the greatest care and fidelity, and, judging by the results, with great success. From not knowing his thermometric apparatus, I am unable to determine why these observations are of less value. I should attribute it rather to the boiler, or to the mode of using it, than to the thermometers; for Dr Hooker speaks of a coincidence in the readings of different thermometers so exact as to be unusual. Dr Hooker states that he finds the errors by actual calculation considerably less, if reduced by M. Regnault's numbers than by mine;

* A few points marked by the letter *r*, calculated from Regnault's formula, are inserted in the figure for the sake of comparison.

† The entire series of Dr Hooker's observations is best represented by 548 feet for 1°, when we include the (somewhat doubtful) highest observations. This agrees almost exactly with my earlier determination.

but I imagine that the difference will almost disappear, if he adopt the slightly altered value of the coefficient given in this paper, from which we have seen that the coefficient deduced from his observations alone does not sensibly differ.

To complete the subject, I have discussed, in the same manner with Dr Hooker's and my own, the observations made in the Alps and Pyrenees by MM. Martins and Bravais, by M. Marié, and M. Izarn, which M. Regnault has quoted very justly in confirmation of his formula, which represents them excellently well. These I find also extremely closely represented as far as 90° Cent., or 194° Fahr., by a linear formula, of which the coefficient for 1° Fahr. is 531 Eng. feet; but the greater part are made at comparatively small elevations. The observation for Mont Blanc alone gives 543·4 feet for 1°.*

These results, it will be seen, give a coefficient somewhat less than Dr Hooker's or my own. The greater part, however, were made at small elevation, and, consequently, give a less accurate *mean* coefficient.

It has been shown, in the previous part of this paper, that the Table of M. Regnault (which had not been published when I last wrote) represents my observations as satisfactorily as the simpler formula which I used, whilst it expresses probably better the results at very great and at very small elevations. Dr Hooker's observations, though hardly decisive as to the preference of one or the other formula, are also well represented by M. Regnault's numbers; whilst those made under M. Regnault's immediate superintendence, with instruments expressly constructed and graduated by his direction, coincide, as might be expected, very nearly with his Table. I claim, therefore, no preference for my formula, except as a convenient approximation to the truth, sufficiently accurate for heights under 12,000 or 13,000 feet. But I find that the results of M. Regnault's empirical table may be expressed with an accuracy almost perfect, within the *extreme* limits of observation, by a formula so remarkably simple as to dispense with the use of any tables whatsoever, and which, therefore, may be used instead of the still simpler approximation, and with little more trouble. If we assume the boiling point at the lower station to be 212°, then the elevation in feet of a station where water boils T degrees of Fahrenheit lower will be exactly

$$h = 517\,T + T^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

or in metres and centigrade degrees, $h = 284\,T + T^2$.†

* In the calculation of De Saussure's observation on Mont Blanc, in my former paper (Ed. Trans., vol. xv., p. 414), a slight mistake occurs. The depression of the boiling point should be 26°·81, instead of 26°·71, giving 545·9 feet for 1°, agreeing strikingly with the other results.

† It happens by a double fortunate coincidence, that the coefficient of T^2, is equivalent to unity in both cases, when the proper reductions are made.

This rigorously represents M. REGNAULT'S Tabular numbers up to a height of 20,000 feet, and probably much farther.

The distinction between the results of this formula and the one given in this paper

$$h = n\,\mathrm{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

(where n is 540 nearly),* is easily specified, and their practical agreement within certain limits shown, as follows:—

(A) is an equation to a parabolic arc PQ (Plate III., fig. 3), referred to a line PR parallel to a tangent at the vertex; (B) is an equation to a straight line $p\,q$, not necessarily passing through P (if we assume a small correction $\mathrm{P}p$ for the standard boiling point), but which shall represent the parabolic arc PQ as nearly as may be. It may be shown that the greatest deviation of the line $p\,q$ from the curve at any point need not exceed *one-eighth* of the value of T^2 in formula (A). If we suppose the range of the boiling point to be 20°, (corresponding to an elevation of 11,000 feet), T^2 is 400 feet, and the greatest error of the linear formula at any height inferior to 11,000 feet is 50 feet, corresponding to *one-tenth of a degree of* FAHRENHEIT, thus confirming the results previously arrived at.

EDINBURGH, *4th December* 1854.

* If we aim at representing M. REGNAULT'S Table only, between the temperatures 212° and 192°, the coefficient should be only 535. The difference in the kind of glass used in constructing thermometers would alone account for the variation. The reason of the different coefficient in the formulæ (A) and (B), arises from the different inclination of the tangent line PT, and the intersecting line $p\,q$, in Plate III., fig. 3, as explained in the succeeding paragraph of the text.

XV.—*Some Miscellaneous Observations on the Salmonidæ.* By John Davy, M.D., F.R.S. Lond. & Edin., Inspector-General of Army Hospitals.

(Read 18th December 1854.)

The interests connected with the natural family of the Salmonidæ, both in relation to river-sport, the pleasant recreation of angling, and to material wealth, in the instances of the migratory kinds, are so considerable and fully admitted, that I trust no apology is required for submitting to the Society any observations in the least likely to contribute to a more intimate knowledge of the species, whether as regards their structure or their habits.

1. *Of the Air-Bladder of the Salmonidæ and its contained Air*

I have examined this organ and the air contained in it in the salmon, white trout (*S. trutta*), charr, and common trout. In each instance I have found it very similar; of a cylindrical form, tapering at each extremity, composed of a transparent membrane, very faintly and partially vascular, extending the whole length of the abdomen, and, with one exception, without colour. The exception was that of the charr, in which, when the fish were strongly coloured red (their muscular substance), it, the air-bladder, was often seen of a rose-hue. I have not been able to detect in any of these fish a communication between the air-bladder and the œsophagus.

The proportional size of the organ, or the quantity of air distending it, on which its size, no doubt, very much depends, varies, it would appear, in different fish of the same species, and still more in those of different species. Commonly, I believe, the bladder of the migratory kinds, as the salmon and white trout, is smaller or less distended than that of the common trout and charr. In a fresh-run salmon of about 12 lbs. taken with the net in the first week of August at Ballyshannon, opened half an hour after its capture, I found in its bladder only about a quarter of a cubic inch of air. And one of the men, an intelligent person belonging to the fishing establishment of that place, who, this last season, had, he said, opened about two hundred fresh-run fish, varying in weight from about 8 to 28 lbs., assured me that, as well as he could remember, the quantity of air was "oftener under than over half a wine-glassful."

For the purpose of chemical examination, of course, it was necessary to open the fish under water. The contained air, extricated on puncturing the bladder, was collected in a graduated tube. For carbonic acid, it was tested by agi-

tation with lime-water with excess of lime. The proportion of this acid gas was in every instance small, barely a trace. For oxygen the test used was a stick of phosphorus, left exposed to the action of the air some time after it had ceased to fume, or the air to suffer diminution. In the instance of the salmon mentioned, the diminution from the action of the phosphorus was hardly appreciable. The same remark applies to the air of the bladder of the white trout: it was tried in two instances,—fish of about half a pound caught in the Claudy river in Donegal. In the trout, river-trout, fish of about a quarter of a pound (two were examined), the proportion of oxygen was greater; it amounted to about ten per cent. of the whole volume of air.

As these trials were mostly conducted when on fishing excursions, and under circumstances nowise favourable for minuteness of research, the experiments I have made on the air were chiefly limited to those above described, which sufficed to convince me that the air of the air-bladder was principally azote, and to allow the inference that the trace of carbonic acid was most likely rather accidental than essential, owing probably to the secondary action of the minute proportion of oxygen present on the organ itself.

These results, I may remark, accord with those obtained by former inquirers on the air of the air-bladder of several other fresh-water fish, whilst they differ so greatly from others—those afforded in like trials on deep-sea fish,—the air of the air-bladder of which was found to be principally oxygen.

That the same organ should secrete two gases so very different in their nature, appears anomalous, and deserving of further inquiry. Indeed, does not the entire subject need more minute inquiry? At present, the facts relating to it are few, and seem far from adequate to allow of any satisfactory conclusions being drawn as to the use of the bladder and its secretion in the animal economy, except of a mechanical kind, as affecting the specific gravity of the fish. Were the gas uniformly of one kind, were it constantly azote, it might be easy to assign it a plausible end; the function of the air-bladder might be inferred to be auxiliary to that of the kidneys. The secretion of oxygen is the anomalous fact, so contrary is it to the ordinary course of changes in living animals, in which the general tendency is to the consumption of oxygen. *A priori*, one might almost as much expect oxygen to be exhaled from the lungs in respiration, as to be separated from the blood by secretion by the air-bladder; and, had we not the authority of so accurate an observer as M. Biot, we might be led to suspect that the statement of its being so was founded in error.

2. *Of the Abdominal Aperture of the Female.*

In a paper on the impregnation of the ova of the Salmonidæ, which I had the honour of presenting to the Society last year, I expressed the opinion that the passage through which the ova have their exit is not constantly open; that after

spawning it becomes closed, but by a membrane so delicate as to be most easily ruptured.

Since that paper was published, I have, from further inquiry, been led to think that in forming that opinion I was in error, and that, strictly speaking, though the passage is virtually closed, it is not absolutely; that is to say, there is no membrane formed over it shutting it up, or union by adhesion, but merely the contact of its parietes, the close apposition of the inner surface, in effect equivalent.

This conclusion I have arrived at from the examination of the part in the salmon and the white trout, and the lake trout of a large size, during the spring and early summer, when their ovaries were little developed. In all these fish I found, on careful examination, that though air could not always be passed, even when impelling it with considerable force, yet that a small probe, carefully applied, might be passed, and when the passage was laid open, without any appearance of the rupture of an occluding membrane.

This admission does not appear to me to affect the argument against the impregnation of the ova *ab externo*, deducible from the structure of the reproductive organs, whether of the male or female fish; the minute papilla of the one seeming as totally unfit for intromission, as the somewhat larger and more prominent papilla of the other (the female) is to perform the part of a recipient.

It may perhaps be said, that when the ova are mature and fit to be impregnated, the passage may enlarge and become patulous. But admitting this, which I believe to be the fact, at the same time that it enlarges, it becomes more vascular, its marginal glandulæ more active, its fimbriæ elongated, so as, whilst favouring the exclusion of the ova, preventing the contrary,—admission of whatever kind from without.

In salmon fishing, early in March last spring, I took a salmon returning to the sea. In the cavity of the abdomen a few ova were found of their full size, such as they are when fit for impregnation; but they bore no marks of having been impregnated; there was no appearance in them of organic change; indeed they were quite transparent, and when put into water, after a minute they became opaque from the coagulation of their albumen, the effect of the imbibition of water through their membranous shell. And, hence, may it not be inferred, first, that no impregnation had been effected internally; and next, that after the exclusion of the greater portion of the ova, the abdominal aperture was virtually closed and impervious.

3. *Of the Breeding Localities of the Salmonidæ.*

It is commonly believed that the several species of the genus, at least the more distinguished, such as the salmon, white trout, bull trout, common trout, and charr, require their ova to be exposed to the action of running water for their

fecundation and hatching; that, for this purpose, the migratory kinds quit the sea for the river, the lake-fish the still water for the streams, and the river-fish their ordinary places of abode for the smaller tributaries.

From such information as I have been able to collect, I am led to infer, that though this belief is commonly well founded, there are exceptions; and that the conditions to successful breeding are not quite so restricted as has been generally supposed.

In a former paper, which was honoured with a place in the Transactions of the Society, entitled, "Some observations on the Charr, relating chiefly to its generation and early stage of life," proof was adduced that this fish more commonly avoids than seeks running water for the purpose of breeding, and that the gravelly and rocky shoals of the lakes it inhabits are its favourite breeding localities, rather than the bed of a river or brook, where the water is in rapid motion.

In artificial breeding, not only have the ova of the charr, but also of the common trout and salmon, been successfully hatched without the use of running water, merely by changing the water daily. And in accordance with this, I have been well assured of similar instances in nature, that is, of the ova of the trout and of the salmon having been laid, like those of the charr, on beds of gravel in lakes, and where it is believed they have been hatched. I shall mention the few instances which I believe are worthy of credit. In Connemara, county Galway, Ireland, there is a lake, about five miles from Clifden, called Lough Anaspick (The Lake of Contention), abounding in good trout, and which, from its situation in a flat part of the country, is fed more by the rain that falls into it, than by the stream which enters it,—a stream so small as to be unfit for a breeding place,—the same remark applying to the little outflowing stream. On a gravelly shoal of this lake I have been assured by fishermen residing in the neighbourhood, that the trout deposit their spawn, that they have been seen in the act, and that the roe has been found there.

In Blea-tarn, in the Lake District of England, observations of the same kind have been made. I have been informed that the roe of the trout has been detected in plenty near the shore. The person from whom the information was obtained remarked that such a laying of the spawn was unavoidable, inasmuch as, from obstructing obstacles, the fish could neither run up nor down the inflowing or outflowing stream. In this instance it was stated, that the favourite place of spawning was near to the fall of the little stream into the tarn, its principal feeder.

At Lough Melvin, in Ireland, a lake in which the gillaroo trout is plentiful, I learned, whilst there, that this trout never enters the tributaries with the other, the common trout, in the spawning season, and that it had never been seen in them; from whence the fishermen, my informants, who watched the streams, inferred, with confidence, that it bred exclusively in the lake. But though confident of

this, they had never, they said, found the ova,—which, indeed, is not surprising, considering the great extent of the lake, and that they had never made search for them.

The only instance I can mention as appearing to be well authenticated, of a salmon spawning in a lake, I learned from an old Irish fisherman residing in the neighbourhood of Lough Erne. He assured me that he had seen a pair of salmon preparing their spawning-bed in a shoal of the lake off an islet known by the name of Rabbit Island.

If what I have stated should be received as satisfactory, I hope it may prove not without use, as tending to show that the Salmonidæ may be bred in lakes and ponds; and thus encourage attention to these as breeding places,—for instance, in ponds, by providing beds of gravel, and in lakes, where fish are known to spawn, by affording protection from the depredation of the poacher, which, in the instance of the charr, we know, from experience, to be much needed in some of our Westmoreland lakes.

4. *Of the Variable Time of the Hatching of the Ova.*

In a paper already referred to, that "On the impregnation of the ova of the Salmonidæ," mention is made of the successful result of impregnation in the instance of the mixing together of the roe and liquid milt of the charr obtained from the living fish.

The roe, after having been thus brought into contact with the milt, was divided into three portions. One, the largest, was placed in an earthenware pan, about two feet in diameter, in which was a stratum of gravel taken from an adjoining brook, and water, soft spring water, to the depth of about three inches, which for the most part, about two-thirds, was changed daily. The ova were laid on the gravel, and the vessel was placed on the floor of a room without a fire, where the temperature was liable to little fluctuation.

Smaller portions were put into two water or finger-glasses, such as are used at table, about four inches in diameter, in which also gravel of the same kind was laid, with water from the same spring to the depth of about two inches, and which, as in the first mentioned, was in great part changed daily. Both glasses were placed on a stand about three feet from the ground, and within a few feet of the pan, and all three were exposed to about the same degree of light.

The experiments were commenced on the 25th of November. On the 8th of January, in each of the glasses, three young fish were found in the morning at large, excluded during the preceding night. On the 9th, more had made their appearance in the upper vessels, but none in the one below. The temperature of the water in the glasses was 50° Fahr., that of the water in the pan on the floor being 48°. On the 10th, many more young fish were produced in the glasses, but one ovum only was found hatched in the pan. On the 13th, whilst no more had

appeared in the lower, all but one egg were hatched in the upper, and that one proved to be dead. On the 20th, and not till then, another egg, the second, was hatched in the pan. On the 22d, it is noted that many more were hatched during the twenty-four hours, the temperature of the water being about 52°. From the 22d to the 31st, the hatching process continued, a few young fish appearing daily. Those last produced, it was observed, were smaller and feebler than the first, and died in larger proportion.

It is worthy of note—such was the remark made at the time—how, under the same circumstances, as in the water-glasses, or under slightly different circumstances, as in the earthenware vessel on the floor, ova from the same fish, impregnated with the same milt, at the same time, differ so much as to the time of being hatched, the size of the fœtal fish, and their vitality both before and after exclusion, some embryos dying in the egg, some in an advanced stage in the act of extricating themselves, other young fish at intervals of hours or days.

Now, what was witnessed in these experiments, it can hardly be doubted occurs in the natural process of hatching, in which the circumstances commonly must be very much more varied, and the results, it may be presumed, equally so, and not without advantage as regards the preservation of the species.

5. *Of the circumstances and agencies likely to exercise an influence on the Young Fish.*

In the paper before referred to, "Observations on the Charr," a section was given to the subject above named. The inquiry was, as it appeared to me, an interesting one; and it was my intention to prosecute it farther, but hitherto I have done little for want of opportunity, and that is comprised in two trials, one on keeping the young fish excluded from light; the other on placing them in a very small quantity of water, barely sufficient to cover them.

The first trial was commenced on the 31st of January. On that day, one of the water-glasses, with its brood of young charr, was placed in a dark cupboard, from which light was entirely excluded. Here they were kept till the 1st of April; during which time the vessel was taken out only once daily, for the purpose of changing the water and giving food, which occupied no more than a minute or two. Now, comparing them with the brood in the other water-glass, which had been daily exposed to the light, I could perceive no well-marked difference in the appearance of the fish as to form, colour, progress of growth, or activity. The only difference noticeable was, that those kept in the dark were much shyer than those exposed to light, which was indicated, on their being brought to light, by their endeavouring to hide themselves; this they did by thrusting the head under the gravel.

The other trial was made on the 13th and 17th February, with healthy young charr, hatched between the 8th and 10th January, and on the 10th March on a young salmon hatched about the 26th February. On the first-mentioned day a

young charr was put into a platina capsule about $1\frac{3}{4}$ inch across its brim, with just sufficient water to cover it. It remained active fifty-two hours; and it may have been active longer, as it was found dead in the morning, the night hours not being included in the time specified. In the second experiment, the young fish similarly treated lived rather more than seventy-four hours. The capsule, it may be mentioned, was covered with a slide of glass, to prevent rapid evaporation, and yet not exclude air. In the third experiment, the young salmon was put into a small liquor-glass, with a little water, little more than sufficed to cover it and enable it to move freely, in weight no more than 47 grains. Left till the 22d; during these nine days it did not appear to have suffered from being so limited and confined. Returned on this day to the vessel from which it had been taken, it seemed unimpaired in vigour and activity. The temperature of the water, in each instance, did not exceed 52° Fahr., and was never below 48°.

These experiments, I need hardly point out, were made with the intent of testing the power of endurance of young fish during periods of drought, when they are liable, from the lowering of the streams, to be left almost dry.

The result of the last experiment, I may remark, was somewhat different from what I had expected, supposing, as I previously did, that the air necessary for supporting life would soon be exhausted in so small a quantity of water; not taking into account that the motion of the fish (and it was very restless) might promote the absorption of air; and that even the small volume of water so fully exposed, and almost constantly agitated, would conduce to the same.

6. *Of the Food of the Young Fish.*

In consequence of the attention that is now being paid to the artificial process, as it has been called, of breeding fish, the question, what kind of food is most suitable to the young fish when it has to provide for itself, after the consumption of the store laid up in the vitelline sack, becomes one of considerable interest.

My own experience as regards the attempt to feed the young fish, is very limited. The first trial I made was with finely-grated boiled beef; the second with dried charr—the muscular part dried rapidly and pounded very finely. A small portion of each was given daily to a different brood. The young fish fed on the particles greedily, seizing them chiefly when in motion. Those which had the charr seemed to thrive somewhat better than those fed on beef; more of the former attaining their perfect shape, as denoted by the development of the fins, than of the latter. In a third trial no food was given. This was in the instance of the brood hatched in the larger vessel, the earthenware pan, in which was a considerable quantity of gravel and small stones, with the addition of some aquatic plants from the river Brathay, - that part of it which is one of the breeding-

places of the charr. In this instance the young fish, after the disappearance of the yolk, throve well, and, as I believe, chiefly on the infusoria present. This inference was made partly in consequence of finding, on microscopic examination, infusoria on the plants and stones and sides of the vessel, and vestiges of them in the excrements of the young fish, and in the intestine of some that were opened, and partly from observing how the young fish, when in pursuit, as it was supposed, of food, seemed to confine themselves to the spots where the infusoria were in greatest numbers. Another reason (were we to reason *a priori* on the subject) might be assigned, viz., the presumed fitness of these microscopic animalcules for the food of creatures of so small a size as the young of the Salmonidæ in their early stage, and the fitness of the latter, with their microscopic eyes, to see and make the infusoria their prey.

The fish, the subjects of these trials, were all young charr. Those from the eggs impregnated on the 25th of November were so advanced by the second week in April, as to be considered fit to be set at liberty. Some were taken to a lake in the Highlands of Perthshire; others to a mountain tarn in this neighbourhood. Both sets were from the earthenware pan, to which no food had been given.

I may offer as a suggestion, that where minnows abound, their young, it is probable, may be employed as a useful aid for the support of the young of the Salmonidæ. The time of breeding observed by the minnow, early in May, seems suitable, and especially so the minuteness of the ova of this little fish, and of their fry when hatched. Their eggs at maturity I have found to be ·06 of an inch in diameter; the fœtal fish on quitting the egg was about ·20 of an inch in length, and no more than ·35 inch when of its perfect form, as denoted by the growth of its permanent fins, which it acquired in a few days. In this stage its weight (moist) did not exceed ·03 grain, and when dried was reduced to ·01 grain! *

7. *Of the Parr.*

As discussion, wherever doubt exists, is always useful, so that it be temperately conducted, and lead to further inquiry, I venture to bring under the notice of the Society the question, the vexed question, whether there be or not a fish of the family of the Salmonidæ, a parr having so close a resemblance to the salmon fry as to be with difficulty distinguished, and yet a distinct species.

* The above results were obtained in the month specified. The eggs were from minnows from the river Rothay, a tributary of Windermere. On the 6th of May, they were impregnated by the artificial process, and placed in water, varying in temperature from 50° to 54°, which was changed daily. One fœtal fish burst its shell on the 11th of May; the next on the night of the 12th; the majority on the following day; some did not appear till the 15th. On the 31st of the same month, most of them had acquired their permanent fins.

In substance, the eggs of the minnow were found similar to those of the Salmonidæ, being composed of oil globules, and of an albuminous fluid, coagulable by admixture with water.

Some of the ova to which no milt was added—the omission intentional—died without showing the slightest appearance of organic development.

So far as the weight of authority is concerned amongst naturalists who have given their attention to the subject, I believe it is in favour of the negative; and on the ground, first, that the parr, according to their experience, has never been found in a river inaccessible by the salmon or sea-trout; and, secondly, that no reliance apart from this is to be placed in slight difference of form, or of colour or spots, as these vary in the known fry of the salmon, according to the quality of the water, food, and other influential circumstances, and in themselves, therefore, are insufficient to constitute a species.

The few who are opposed to this view, and who maintain that there is a parr, a distinct species, state that though rare, yet such a fish is to be met with in streams where the salmon and sea-trout have never been seen, and in some in which they assert it is impossible that either could resort, inasmuch as the access to them is prevented by impassable falls. Two rivers I have heard named as coming under this category, in which it is said that parr have been caught above falls that no salmon or white trout could by any possibility surmount,—one a tributary of the Shin, in Sutherlandshire, the other the Kirkiag, in the same county, on its west coast.

Granting the fact that in each of these rivers there is an impassable fall, is the inference drawn by the advocates of the distinct species quite conclusive? May not the parts of the river above or below the fall have some communication by a channel through which a salmon or sea-trout might be able to pass, either subterraneous, or by some collateral branch formed during a period of flood? or, if nothing of the kind be discoverable on careful examination, may not the parr found in the upper stream derive their origin from impregnated ova of the salmon or sea-trout conveyed by birds, such as the water-ouzel, adhering to its feet or plumage, or loose in its bill? Thus conveyed, it is presumed they would retain their vitality, and in due time be hatched. There is a well-authenticated instance of impregnated ova of the salmon, taken even from the stomach of the common trout, having produced salmon-fry.

But waving these arguments pro and con, what is the evidence that all parties would probably hold to be satisfactory or conclusive? Is it not the showing that the parr, the asserted distinct species, propagates its kind, and that in due season, and at the same time, the male and female fish are to be found with roe and milt mature,—the one of its maximum size, loose in the cavity of the abdomen, fit for exclusion,—the other in its liquid milky state, ready for expulsion?

So far as I can learn, such a coincidence has never been observed. I have examined hundreds of parrs, and a large number from a river where the salmon is rare, and is never known to be taken but by the poacher in the fall of the year, after a flood. Male parrs I have frequently found with mature milt, but never a female with roe correspondently developed; on the contrary, in the female fish, without exception, the ovaries have been so small, that had they not been sought

after carefully, they would have escaped notice, and the ova contained in them so minute as to be mere granules.

In conclusion, is it not reasonable to hold, that till such evidence as that referred to be adduced and clearly substantiated, the existence of a parr as a distinct species must be considered as not proved.

Lesketh How, Ambleside, *November* 22, 1854.

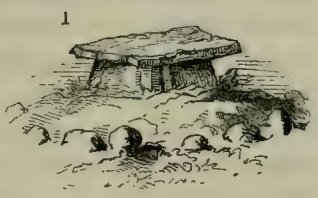
XVI.—*Notes on some of the Buddhist Opinions and Monuments of Asia, compared with the Symbols on the Ancient Sculptured "Standing Stones" of Scotland.* By Thomas A. Wise, M.D., F.R.S.E. (With a Plate).

(Read 2d January 1855.)

The general identity, in idea and design, of the ancient monuments of Southern and Western Europe, with those of Hindostan, is so marked, as to appear to justify the inference that races of Asiatics proceeded westward at different ages, and established themselves along the shores of the Baltic and Mediterranean Seas, and part of the Atlantic Ocean; along which route they have left characteristic monuments, which resemble those of their original country.* The ancient monuments common to these distant regions are—

1. *Cairns and Barrows.* These monuments, common to Celtic Europe and India, are mounds of earth, or piles of stones. One near Hidrabad, in Central India, was surrounded by a circle of stones, which exactly resembled those round cairns in Europe.

2. *Cromlechs and Kist-vaens,* consisting of two or more upright stones, which, as props, support a horizontal block or slab, forming a chamber underneath. Such monuments are pretty numerous in wild and retired places in the peninsula of India (fig. 1), and contain a sarcophagus, with the bones of the dead.† In others, urns are found, of red or black pottery, containing the ashes of the bodies which had been purified by their passage through fire. These monuments were usually paved with a large slab, and have a circular hole‡ in one of the upright slabs which formed the walls, to allow the passage of the soul, which was supposed to linger for a time near the remains of the body after death. These cromlechs were in some cases varied in their shape, and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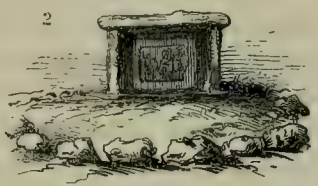
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* These eastern races appear to have proceeded westward by Scythia and Scandinavia, on the one hand (Worsaae, Primeval Antiquities of Denmark, edited by Mr Thoms, p. 132 *et seq.*); and by the shores of the Mediterranean, on the other. Hence, we find the same cromlechs and cinctures of pillar-stones on the mountains of Circassia, and the undulating plains of Tartary (Dennis's Etruria, vol. ii., p. 321); Asia Minor (Irby and Mangles's Travels, ch. vi.: Colonial Libr. Edit., p. 99); Tunis, in Africa (Dennis, *l. c.*); Etruria and Sardinia (Dennis, *ibid.*); the Atlantic shores of Spain (Borrow's Bible in Spain, ch. vii.); of Gaul (Histoire des Peuples Britons, par Courson); and the greater part of the British Islands.

† Colonel Mackenzie sometimes found in these cromlechs urns, arms, and even coins. See Maria Graham's Journal, p. 168.

‡ See Letter by Capt. Newbolt, Asiatic Society, 17th July 1846.

the slabs were dressed. In one found near Rodroog (fig. 2), by the late Col. Mackenzie, Surveyor-General of India, there was an attempt at sculpture; proving a certain advance in ornamental art. Sometimes they were surrounded by circles of stones.

3. *Obelisks*, or as they are frequently called, *Standing Stones*. Such large tapering, erect stones, or obelisks, are found in all Celtic countries, and resemble topes or solid cairns in Buddhist countries; one kind being funereal, erected over the grave of an individual; another, memorial, to commemorate some event; and a third being dedicated to the Deity. In general, such stones are placed over the dead in the Celtic countries of Europe, and are likewise found in Central India, as well as in Bengal and its neighbourhood. The drawing (fig. 3) represents such stones, as found in Central India by Col. Mackenzie. What renders this monument interesting is, that it appears to have formed part of a cairn similar to that mentioned above, which was surrounded by a circle of stones. In many parts of India, however, such large blocks of stone are not to be procured; and pyramidal structures, spires, or "*muts*" (fig. 4), evidently modifications, under the pressure of circumstances, of the original monolithal erection, were had recourse to. These are still erected by Hindus. They are sometimes cenotaphs, at other times mausoleums. In the former case, the wealthy erect these buildings as memorials of the dead; and in the latter, over the ashes of their relations, or over a bone of their body, after it has been purified by fire, on the banks of a sacred river. In this way each of the Maha-rajahs of Tipperah has a spire (fig. 4), erected over a bone of his predecessor, on the banks of the sacred Teeta River. A favourite wife, particularly if she had become a Suttee, had often a *mut* erected over some of the ashes of the body. In these cases the spires were usually smaller than those over the husband. These *muts* often contain an image of Siva or Kalee; others contain a *linga* (priapus), or a flat stone supporting a central pillar, representing the regenerator Siva, or Nature under the male and female symbols. These buildings are varied according to the means and the taste of the individual. In general, they consist of one, but in other cases, of many spires.

4. *Circles of Stones*. The circles of stones appear to have formed sacred spots, intended for other purposes besides that of being depositories for the remains of the dead. Of these, examples were found in the same retired places as the cromlechs in Central India. They are often of the same size as in this country, and, like them, are formed of boulders. One, of which a drawing is now before me, forms a circle of 32 feet; another of 26 feet; and a third is 30 by 27½ feet. As the arts improved in

Europe, the light of Christianity was also introduced, and the erection of these structures was abandoned. But in India, where idolatry held its ground, the arts, as they advanced, were employed in their enlargement and embellishment. The objects and edifices of superstitious veneration were increased in size, until they attained the scale which we see exemplified in the remains of the vast structure of *Depaldinna*, the Hill, or Mound of Light, near Amrawatty, in Central India. There we see immense excavations, surrounded by concentric circles, formed of vast numbers of stones, beautifully sculptured with mythological figures, and inscriptions in two or three different idioms of the Sanscrit language. The outer circle of this gigantic structure is 160 feet in diameter. In the neighbourhood are numerous remains of kist-vaens, circles, barrows, &c.

Some years ago I examined two interesting structures or temples, which had all the essential features of the stone circle, and of the ancient temples of Central India. They are situated near the banks of the sacred Bargaretta, or Hooghly River, at Culna, and belong to the Maha-Rajah of Burdwan. In this temple there are two concentric circles of stones in marble, formed into 108 lingas, or representations of the male and female energy of the world, with a temple over each. The external circle is formed of alternate white and black marble pillars; the internal circle entirely of white marble. The outer circle had its entrances north and south, and the inner east and west, much in the same manner as in the large temple of Depaldinna, in Central India; and while in the centre of this there was a tank, the temple in Bengal, where worship is regularly celebrated, has a well of water, the *yoni*, or symbol of Parvati, the female energy. A second circle of temples in the neighbourhood appeared to be merely a modification of the other.

This general identity of the ancient monuments of southern and western Europe with those of Hindostan, is further proved by the physical conformation of the races who inhabit these distant countries, by the similarity of many of their manners, customs,* and observances;† and by the decided and extensive affinities of the Celtic and other languages of western Europe with the Sanscrit,‡ which afford as strong evidence as we can be expected to obtain, of a connection so remote between races so widely separated. Indeed, the names of mountains, rivers, and other great natural features of the south and west of Europe, bear evidence of its having been in possession of a Celtic race anterior to the earliest date of authentic history; and this early connection indicates a line of inquiry, by following which much of the obscurity resting over the earliest monuments and history of western Europe may be cleared away.

As these Asiatic races emerged from their oriental seat, and settled on the shores of Europe at different stages of advancement in civilisation, we must expect differences in the idioms of their language, in their monuments,

* Primeval Antiquities of Denmark, p. 132 *et seq.*

† Pliny, Nat. Hist., xxv. 1.

‡ Prichard's Celtic Nations, pp. 20-22.

and in certain of their observances; which will be better understood by adding a few remarks on the Asiatic people who are believed to have erected these monuments, and on their religious opinions, in explanation of some of the symbols or hieroglyphics which are found upon the ancient obelisks of Scotland.*

A reformer of the Sabaism, and Brahminical fire-worship of the ancient Asiatic races, gave rise to the present form of the Buddhist religion, in the north of India, in the sixth century before the Christian era. This religion inculcated a belief in a trinity, in the perfectibility of our nature, in the transmigration of souls, in the veneration for serpents, for certain trees, &c.; and such was the sanctity and zeal of the priests, and the benevolence of the doctrines they inculcated, that it rapidly spread over Hindostan. Some time after the invasion of India by Alexander the Great (B.C. 247), Asoka was the great Buddhist monarch of that country, who probably propagated the new doctrines by recording edicts upon rocks, and pillars (lâts), in different parts of Hindostan.† As the language was the vernacular dialect of the period, it varied according to the part of the country it was intended for; and it was only by the genius of the late JAMES PRINSEP that it was deciphered.‡ These most interesting monuments of antiquity are found in distant parts of India, and were most probably transcribed from copies obtained from the king, and such changes were made as were required for the better understanding of the people for whom they were intended. They inculcate the following benevolent precepts: honour to parents and kindred; never-tiring charity; respect and liberality for good men, and spiritual guides; temperance and moderation in every word and action; and the utmost humanity to man and the inferior animals. These precepts were not only to be inculcated in their own country, but ministers of religion, or missionaries, were appointed "to spread them, and to intermingle themselves among all unbelievers, to overwhelm them with the inundation of religion, and with the abundance of the sacred doctrine."—(5th Table.) They were directed to effect conversions in the dominions of Alexander (satrap of Persia), Antigonus (sovereign of Phrygia and Lycia), Magas (the son-in-law of Ptolemy Philadelphus), Ptolemy (either the first or all of the four first princes of Egypt), and Antiochus (the Great). These names were probably given, as Professor Wilson supposes, from their notoriety in India, as they were not contemporaries.§ The missionaries were to find their way into the uttermost limits of barbarous countries, for the benefit and pleasure of all, and for reducing the passions of the faithful, and for the regeneration of those bound in the fetters of sin; "intermingling

* See the beautiful drawings of the ancient sculptured stones of Angus by J. CHALMERS, Esq., and the more recent volume on those of Scotland by the Spalding Club.

† On the Kapur di Giri, the Girnar rock Gujerat, and on the Dhale rock of Cuttack; on the Pillars (lâts) of Delhi, Allahabad, &c. See Journal of Asiatic Society, Calcutta, vol. vii., pp. 156 and 219.

‡ See Asiatic Journal, Bengal, vol. vii., pp. 150 and 219.

§ Journal of the Royal Asiatic Society, vol. xii., p. 246.

equally among the dreaded and the respected, both in the very metropolis of religion (Pataliputa); and in foreign places," to teach them "righteousness, which passeth knowledge."

The Buddhists were then a powerful and a rich community; and in the fifth century, the Chinese traveller Fa Hean* found Buddhism the prevailing religion of India. But in the middle of the seventh century, another Chinese traveller (Hwan Thsang) found that the zealous Buddhists had degenerated into rich, selfish, and idle monks. This engendered a feeling of dislike and jealousy among the people, and as they rejected the sacred books of the Hindus, and many of their cherished dogmas, the Brahmins persecuted them with relentless fury, and after ages of sanguinary wars, they were expelled from India. The vanquished Buddhists fled to Ceylon, and other neighbouring countries; and the feeble remnant in Hindostan live in small communities, abstain from carnal pleasure, and pass a monotonous existence in the routine of a monastic life, without any of that fervid enthusiasm for which their predecessors were once so distinguished. But although so few remain in the original seat of the religion, so great was their success in other countries, that there are now upwards of three hundred millions of Asiatics who profess the Buddhist religion.

One cause of the almost total extinction of Buddhism in India was, that it required its followers to lead a life of charity, abstinence, and privation, with a long course of prayer, penance, and devout abstraction, in order to work out their escape from the circle of existence; or to attain what they considered the state of beatitude in another world. This produced constant disputes and divisions, which, with their wandering habits, the frequent and cruel persecutions, and the precepts of their religion, induced them to visit distant countries, to gain followers to their peculiar opinions, which they modified to suit the people they visited. At this period some intercourse was still maintained between the cognate, but widely separated races; and the new doctrines were carried westward by missionaries, who, finding some of the races they visited unprovided with a written language, had recourse to symbols, already used in the East, to express their fundamental doctrines. These were modified to suit the particular circumstances of the people they resided among. The Asiatics were idolaters from an early period, and have continued so; and from their power and riches, and a certain advancement in the arts, they constructed magnificent temples, and idols of Buddha; while they relied on symbols among the rude tribes of Britain. Such differences in the method of propagating the religion must be expected, and seem to strengthen the argument in favour of their identity; and an enumeration of the remains of the interesting race in India, will afford useful indications of their opinions. These are—

1. Magnificent *Cave Temples*, excavated out of the solid rock. Some of these are beautifully decorated with paintings and sculptures.

* See the admirable translation, and most interesting notes, by J. W. Laidlay, Esq. Calcutta, 1848.

2. *Lâts*, or sandstone obelisks, containing inscriptions of royal edicts regarding ceremonial observances, &c.

3. *Large Monasteries.* And,

4. *Topes*, or religious edifices. These are in the form of massive hemispherical domes, and are another name for regular built *Cairns*, signifying a solid mound or tumulus. These Topes are either funereal, memorial, or are dedicated to the deity.

a. The *Funereal Topes*, or *Cairns*, are the receptacles of the ashes and bones of saints, and are built in honour of mortal Buddhas. They are of all ages, and made of different materials; as they were the common form of tombs, even before the advent of the most celebrated mortal Buddha (the Sakya Muni), who died B.C. 543.

b. Topes were built as *Memorials*, in celebrated places.

c. Dedicatory Topes were intended as offerings to the Deity, the supreme invisible God. These religious edifices contained no deposits, and were typified on the outside by a pair of eyes on each of the four sides either of the base or crown of the edifice,* to indicate the all-seeing, all-powerful, divine spirit, who is light, and is supposed to occupy, in a special manner, the interior.†

The great doctrine of the Buddhist religion consists in a triad, "*tri-ratna*," or three jewels, or three precious ones; that is, *Buddha*, spirit or God, *Dharma*, the law, and *Sangha*, the Buddhist community or brotherhood. This was the genuine sense of the words, to certain of the initiated; but a more clear and intelligible explanation was, that *Buddha* signified the spiritual, or the divine intellectual essence of the world, or the efficient underived cause of all; *Dharma*, the material essence of the world, the plastic, underived cause; and *Sangha*, which was derived from and composed of the two others. This third member is, therefore, the collective energy of spirit and matter in the state of action; or "the embryotic creation, the type and sum of all specific forms, spontaneously evolved from the union of Buddha and Dharma." ‡ This is merely a modification of the opinion of so many of the ancient nations, and nearly all the Asiatic races of the present day. They believe in a spiritual deity and a fruitful earth; in a male and female principle, in mind and matter; Osiris and Isis; in Venus Genetrix and Phallos; Pater Æther and Mater Terra; Lingam and Yoni, Brahma and Sarsaswete, and other gods and their saktes or wives; Yang and Yin (Chinese), &c.

The Buddhist missionaries found it necessary to employ symbols in Asia and in the countries they visited, in order to direct and fix the attention of rude races to the spiritual objects of their worship. Those symbols of the deity were in Asia—

1. *Spirit* (Buddha), represented by a circle or wheel, typical of the passage of the soul through the circle of existence.

* See Plate, Fig. 10, CUNNINGHAM's Topes of Bhilsa, p. 8.

† Journal of the Asiatic Society, Bengal, vol. v., p. 81.

‡ Ibid., vol. v., p. 37.

2. *Inorganic Matter* (Dharma) was represented by a circle, or by a monogram formed of the initial letters of the names of the elements of matter.

3. Of *Organized Matter* (Sangha), by a third circle, which, when decorated, was often formed of the combination of the symbols of Spirit and Matter; or on coins by the representation of an organized body.*

The symbols of the Buddhist triad, when employed in India, are often of elegant forms, and gracefully decorated; and have undergone various modifications upon coins and sculptures, in different ages and countries, according to the fancy of the individual, and the particular Buddha, or saint, they worshipped. Buddha is represented by various-shaped wheels, Dharma by changes in the beautiful monogram, and Sangha by a combination of the two others, by organized bodies, or by other varied and graceful ornaments, to reach the understanding of those for whom they were intended. As the community increased in power and riches, they erected magnificent temples, or excavated them out of the "living rock," as the proper offering to the deity; amidst a profusion of ornaments and magnificence, idols, or dedicatory Topes (chaitya), were the objects of worship.

When the enthusiastic Buddhist missionaries reached the extreme west, they found themselves among a rude race, at enmity with their neighbours, and menaced by the great Roman power, which had subjugated their more powerful southern neighbours. These missionaries, with the Druids, many of whom had fled from the cruel persecutions of the Romans, would unite the different tribes to oppose their cruel invaders, and inculcate their religious doctrines. This could only be done by symbols, as they had no written language; and upon the erect stones, already probably venerated, they traced figures to explain their trinity, the great dogma of their religion. As their influence extended, other obelisks were erected, and adorned with devices to stimulate the pride of the Caledonians, while they awakened their fears, and kindled their zeal, for their religious opinions; and they were executed in a style which proved their intelligence, and their knowledge of the arts which they had brought from the east.

In examining these symbols, we must expect that it is in the simplest form we shall find the identity between the obelisks in Asia and those of this country. Upon Buddhist coins, the triad is represented as triple hemispheres, most probably intended for circles: In the great temples of Ellora, and several other Buddhist caves, Colonel Sykes found these circles traced in the same order as on the coins; two forming the basement, and one the apex (fig. 1).† This is the symbolical representation of the Buddhist triad; which is still more accurately traced on the Kinnellar standing-stone, in Aberdeenshire, which has the three

* On the sculptured stones of Scotland it is represented by some embryotic form of animal or vegetable life, or an imperfect circle.

† See annexed plate (No. IV.), in which *figures* designate the oriental symbols, and *letters* those on the engraved stones of Scotland. Journal of the Royal Asiatic Society, vol. vi., p. 451.

circles placed in the same order as in the temples in Hindostan (fig. *a*); and to mark still more intelligibly the trinity in unity, they are surrounded by another circle. This is the simplest form of the representation of the trinity in unity; and the "crescentic ornament" underneath the circles, in the Kinnellar stone, proves its identity with the other sculptured stones of Scotland.* The most frequent form, however, of the trinity on these stones, is two circles, symbols of spirit and matter, united by a belt, and crossed by a bar, to the extremities of which two sceptres were joined (fig. *d*), to indicate the supreme power, according to the Buddhist creed, the co-ordinate, and all-originating principle. This formed what has been called the "spectacled ornament" upon the stones of Scotland; while the third member of the trinity, organized matter (Sangha), was represented near the others, in the form of a crescent. Sometimes this third member is crossed by sceptres, to indicate the sovereignty of the laws which organic matter follows.

This interpretation of the "spectacled ornament" is further proved by the frequent appearance of an eye at the angles where the cross-bar joins the sceptres; which, like the eyes in the Buddhist temples dedicated to the deity, typify the spiritual intelligence which rules the universe. I may refer to the smaller Aberlemno stone, as a good example of these eyes, as they appear on the standing-stones of this country (fig. 10, *a*; and *k a*).

But neither the circles (fig. 1.), nor the crescent (fig. *A*, *a*), afforded a sufficiently clear idea of organized matter, the third member of the trinity, which they were intended to represent; and with that freedom which the Buddhist artists were allowed, and took advantage of, they explained their meaning more intelligibly by representing organized matter, in Scotland, as an embryo, or some rude representation of animal life.† Thus, on the Dunnichen stone, organized matter is represented as a two-headed figure of a flower (*c*) gracefully bent towards the symbol of God, from whence it was supposed to receive its spiritual emanation; on the silver ornament found at Norrie's Law, a dog-like embryo (*f*), and on the Kintore stone, the embryo of an elephant (*e*) is thus receiving the spiritual influence. The same idea is expressed in the same way, on Buddhist coins; as is represented on the annexed plate, where figs. 5 and 6 represent the union of the two members of the trinity, spirit and matter (α, β), with an embryo elephant or monster below (5); while organized matter was represented as a hand, an embryo (7), a serpent

* As the Hindus as well as the Buddhists suppose that the spiritual essence envelopes the earth, I was curious to see if there was any difference in the size of the circles. I found the figures on the Kinellar stone were sufficiently distinct to enable me to do so; and on carefully measuring the three circles, I found that the upper circle, and that on the right-hand side, were of the same diameter, while that on the left-hand was half an inch larger. I therefore consider that this circle represented Buddha, or spirit.

† According to this opinion, man is the union (Sangha) of matter (Dharma), with the soul or divine intelligence (Buddha).

(7′), a bull (8), &c., on other coins. This proves their identity with the symbols on the stones in Scotland.

Various other examples and modifications might be stated, but I shall now only mention the interesting and solitary example of such symbols upon a rock in Galloway. In this example, the sculptured "spectacle ornament," is near the top of the rock, with organized matter in the form of a horn, having an ornamented large extremity or mouth, turned to the symbol of the deity, and from what might be considered as its navel, two diverging lines proceed and terminate in a circle, or embryo head (fig. *m*); whilst lower down another (*β*), more formed and detached, is intended as a human head, with two feelers or antennæ, to communicate with the external world; by which means, the embryo was most probably fancied to be developed to its full size and figure.

The serpent is represented on several of the engraved stones of Scotland, as the symbol of the deity, or spirit; and this bore allusion to objects of a divine or intellectual nature.* It was therefore represented as transfixed by a cross-bar, uniting the extremities of two sceptres (fig. *n*); as on the stone of Belutheron and of Meigle, over which an embryo elephant is the representation of crude organized matter; which, like those near the circles, it was supposed to typify.

In some cases the trinity is represented in the form of a horse-shoe, with organized matter in the form of a fowl (fig. *x*).

A third variety of the symbol of the spiritual deity is a sculptured square, or oblong fork-like figure; a modification of the cross, or Buddhist sacred labyrinth. This is the complicated form of the Buddhist cross (fig. 9), which forms a curious subject of inquiry, as it is found on ancient Phœnician pottery, and on Gaza Coins; and is considered to be the Phœnician letter Tau (fig. *β*), the symbol of divine life.† It is also found on Christian monuments,‡ and on the dress of a gravedigger in the catacombs of Rome.§ In Scotland this cross occurs in the fourth line of the Newton stone (fig. *y*). Such examples of squares, or modifications of the Buddhist cross, are to be found on the Maiden stone, and on the Abernethy and other stones (fig. *o*, *p*).

On further examining these interesting sculptured stones of this country, we find other symbols of the faith of the Buddhists. The veneration which they had for certain trees affords a striking similarity, and appears to have given origin to their

* Physici vero serpentem spiritualissimum animal esse dicunt; itaque res divinas, per serpentis naturam notabant.—Eusebius, Prep. Evang., liii., c. 3.

† Raoul-Rochette, Mem. de l'Academie Royal des Ins. et Belles Lettres, tom. xvi., p. 312, et xvii., part 2, p. 329.

‡ *Loc. cit.*, p. 302. Boldetti Cosservazioni, pp. 87 and 350. Lupi. (Epitaph S. Sever. Mart., p. 11); and on a Christian sarcophagus described by Allegranza (Sacri Monumenti, Milan, 1577), tab. iv. and vi.

§ Louis Perrets sur les Catacombes de Rome, vol. i., p. 30.

western name, Druid.* In the East several trees were considered sacred by different Buddhists, according as their particular saint was supposed to have been born, done penance, preached, and died, under the sacred shade of a particular tree: Such are the Ficus indica, F. glomerata, F. religiosa, Mimosa serisha, &c. This explains the variety of trees which appear on the Buddhist coins, according as the dynasty or family who struck the coin were followers or disciples of the Buddha or saint whose emblem they adopted. The secluded spots, and the size of the tree, seemed to have decided the selection; while in Europe the oak was chosen for the same obvious reasons; and the secrecy of the forests of these fine trees was well adapted for the performance of their mysterious rites. Figure 12 is an enlarged copy of a tree on a coin springing out of a Buddhist pot or rail; and figure *q* is a similar tree copied from the Eassie stone. The Farnell stone (fig. *r*) has the representation of a sacred tree, with two priests, probably performing religious worship, standing on the rail, and between two serpents shedding their divine influence over the holy place; very much in the same manner as devotees are seen worshipping at a holy tree (fig. 4) springing out of the sacred rail, and each branch surrounded by a spiritual halo.†

Besides lions (fig. *u*), camels (fig. *v*), serpents, and marked sacrificial bulls (fig. *s*), centaurs appear several times upon the erect stones of Scotland; and they cannot be supposed to be an original idea of the artists of these stones, but point to Greece, their supposed original country; and perhaps they were derived from the mysterious Pelasgians, the druidical worshippers of the oak. The centaur was typical of a barbarous devastator; and he is represented at the side of the cross with other monsters, as on the Glammis stone. On the Meigle and Aberlemno stones, the centaurs hold a battle-axe in each hand (fig. *t*),‡ and are represented as dragging trees after them, being typical, probably, of the destruction of the druidical groves by the Roman troops. These sacred trees appear to have been respected by the Caledonians after the introduction of Christianity, which explains their appearing in honourable positions on the same stones with the cross, as in the Eassie stone; while the centaur is represented, in the lower and most degraded part of the stone; and in the Aberlemno stone, a bar separates him from the chiefs above.

These observations would be imperfect without a few words on the nature, probable age, and uses of these interesting monuments; which are so numerous in the eastern coast of Scotland, so remarkable for the peculiar symbols they bear, and the elegant manner in which these are often executed, in an age when the inhabitants of the country, from all we know of them, were in a state of ignorance and

* The name in the Sanscrit language is *dru*, in the Greek *drus*, Welsh *deru*, Erse *dair*, a tree, or oak-tree.

† From a bas-relief in the Museum of the Hon. East India Company, London.

‡ The artist of Mr Chalmers's beautiful drawings has erroneously represented these as crosses.

rudeness. A few slight notices of their existence, are found in some of the ancient national authors, and various absurd legends, in explanation of the origin of the engraved stones, are all that remain of the remarkable history of these antiquities, which can now only be obtained from examining the figures on the stones themselves.

These sculptured stones of Scotland are large in size, selected with skill, and often brought from great distances. They were erected at convenient central situations, the sacred symbols were traced upon them, and they were consecrated for worship. So various were their decorations, and so modified their sacred symbols, that out of nearly two hundred that are supposed to exist, there are not two exactly the same. Such varieties prove that the artists were allowed a considerable liberty in these illustrations of their belief, so as to suit their own fancy, or render them more acceptable to the people for whom they were intended. We might suppose the individual who traced the symbols on the rock of Galloway (fig. *m*) to be a zealous missionary, who availed himself of a rock cropping out of the soil, to trace the sacred symbols of the Deity, with such additions as might make them more intelligible to the ignorant people, for whom they were intended; as we find "rich and zealous Buddhists in Thibet, of the present day, who maintain at their own expense companies of priestly sculptors, who travel, chisel and mallet in hand, over the country, engraving their sacred formula"* upon the rocks and stones. Such a practice, among the ancient Buddhists of this country, would explain the number of the standing-stones in the east of Scotland—not with inscriptions, for the people they were intended for were ignorant of letters, but with the sacred symbols of the Deity engraved upon them.

It was probably after the persecution of the Roman General, Suetonius (A.D. 59), and the destruction of the sacred groves of Mona, and other places, that the Druids joined their brethren among the mountains of Scotland, when their superior intelligence, and hatred of the invaders, united the Caledonians in offering that obstinate resistance, and consolidating that Pictish kingdom, on the east coast of Scotland and north of the Firth of Forth, which the bravery and discipline of the Roman legions were never able to conquer. The extent and nature of the religious monuments prove that the Buddhists had enthusiastic followers; and as the severity of the climate, and proximity of their enemies, prevented their forming groves, they changed the manner of conveying instruction, in conformity with new opinions from the East, so as to render it more simple and less mysterious. This was accomplished by erecting standing-stones, which bore the symbols of the objects of their worship, sometimes by themselves, and at other places in connection with circles of stones; as was the case with the Kinnellar stone, the stones

* Om mani padme houm,—(Oh! the prescious lotus, Amen).—Oh! may I obtain perfection, and be absorbed into Buddha, Amen. See Travels in Tartary, Tibet, and China, vol. i., p. 194. KLAPROTH, Journ. Asiat., Second Series, vol. vii., p. 188.

of Kintore, of the wood of Crechie, and, most probably, of many others which have been destroyed. It is probable, therefore, that the erect stones, with the sacred pagan symbols of the Deity, were prepared during the first century.

At an early period, a remarkable change occurred in the religious opinions of the Caledonians. They became believers in Christianity. This we know from the crosses they erected still retaining peculiar ornaments, and pagan symbols; proving that they had not entirely rejected their ancient opinions; as druidical monuments were supposed to be purified from the contamination of heathenism, by being carved with the figure, or altered in the shape of the cross. This change of faith must have been facilitated by the Buddhist doctrine of the Trinity, and the liberality of their sentiments regarding other religious creeds, which is still so marked a peculiarity in Buddhist countries. "We find," writes M. Huc, "many of these Buddhist priests (lamas) attach the utmost importance to the study and knowledge of *truth*; and we find the same men coming, again and again, to seek instruction from us in our holy religion."* It appears to be this same liberality of sentiment which is now opening a way to the Christianizing of the great Chinese Empire, which produced a corresponding effect in the conversion of the Caledonians.

Such conversions must have been made at an early age, as TERTULLIAN, who wrote his celebrated Treatise against the Jews (A.D. 209), affirms, as a known truth, that "those parts of Britain where the Romans had no access were subjected to Christ," or had become Christians.† Those early converts could not communicate with their neighbours, in consequence of the constant warfare with the Romans and other tribes which were not able to conquer the great Pictish kingdom north of the Forth. Even the Mæatæ, or Midland Britons, were still idolaters when the Caledonians were Christians, which explains why the Scottish deputies, in the famous debate regarding the independence of their kingdom before Pope Boniface VIII., declared that the Christian missionaries, who converted the Caledonians in the primitive ages, came directly from the east;‡ and the account by BEDE of the dispute between Bishop COLMAN and WILFORD.§ When the Bishop alleged the example of St John the Evangelist, with all the churches over which he presided, for adhering to the Jewish custom of keeping Easter, WILFORD declared that wherever the Church of Christ is spread abroad the western form was kept, "except only these and their accomplices in obscurity; I mean," said he, "the Picts and the Britons, who foolishly, in these two remote islands of the world, and only a part even of them, oppose all the rest of the universe."‖ Their missionaries seem to have proceeded directly from one of the then congregations of Asia Minor, which were most probably in-

* Travels in Tartary, Tibet, and China, vol. i., p. 65.
† Britannorum inaccessa Romanis loca Christo vero subdida. Contra Judæos, c. vii.
‡ Innes' Critical Essay, p. 620, and Civil and Eccl. Hist., p. 14.
§ A.D. 662.
‖ BEDE, Eccl. Hist., b. iii., ch. 25.

timately connected with Spain, which explains St Paul's remark in his Epistle to the Romans,* "whensoever I take my journey to Spain I will come to you" (chap. xv. ver. 24); "and I will come to you into Spain" (ver. 28). This proves that congregations of Christians existed in Spain in the first century, from whence, most probably, the spirit of conversion early found its way to Britain. It is in vain to conjecture by what means the Christian missionaries reached the northern part of Scotland. The enthusiasm of these primitive Christians was quite sufficient to overcome these obstacles; and the liberal Buddhists, open to reason, and eager for the acquisition of truth, would be easily converted, and become enthusiastic followers of the new faith. But, retaining their liberality of sentiment, they did not immediately reject the symbols of the Deity which their more ignorant followers held in veneration; and we may still mark the changes which their feelings underwent by those on the engraved stones. In all those erected after this period, the pagan symbols were subservient to the Cross, and this became more and more marked, until the symbols at length disappeared from the emblem of the Christian faith.

According to that able antiquary, Dr D. Wilson, "the interlaced patterns, and figures of dragons, serpents, and nondescript monsters, bearing a close and unmistakeable resemblance to the decorations of some of the most ancient Irish manuscripts, and several of the beautiful initials from the Book of Kells, an Irish MS. of the sixth century, as engraved in Mr Westwood's Palæographia, bear a close resemblance to the style of ornament of these sculptures; while the interlaced network on the case of the shield of St Maido, which Dr Petrie conceives cannot be later than the eighth century, though less distinctly characteristic, and by no means peculiar to Ireland, very nearly corresponds in its details to the ornamentation frequently introduced on the Scottish monuments." †

There is much difficulty in determining on the age in which these crosses were erected, as we have no direct evidence on the subject. From the peculiarity of their form, of their decorations, and locality, they must have been prepared before the dissolution of the great Pictish kingdom in the ninth century; and the adoration of the cross appears to have been practised in the ancient churches, for which reason the heathen, particularly Julian, reproached the primitive Christians with this species of idolatry. This may be traced to a misapprehension of the expressions of the apostles and fathers "taking up the cross and following Christ—of enduring the cross—suffering persecutions for the cross—should not glory save in the cross." The custom of making the sign of the cross may be traced to the third, but most probably was used at a much earlier period. Constantine the Great first used the cross, or token under which he fought and conquered, and is supposed to have first erected crosses in public places. Others believe that it was not until the Empress Helena found the true cross that it became an object of

* A.D. 60. † See his Archæology and Pre-historic Annals of Scotland, p. 497.

adoration. This was A.D. 326, in the twenty-first year of the reign of her son Constantine, the thirteenth of the Pontificate of St Silvester, and the first after the Council of Nice. Previous to a battle or great enterprise, an anticipatory offering to heaven was presented by the erection of a cross, as we find OSWALD did previous to the battle he fought with CADWALLO in the seventh century. In this case it was a cross of wood, OSWALD holding it till it was fixed in the earth, while his soldiers kneeled around.* It was in the eighth century that, in compliance with the teaching of JOHN OF DAMASCUS, the crucifix was considered the principal object of worship, so that it is probable those stone crosses in the north of Scotland were erected before this period; and, from their being so different in their form and ornament from those of Iona, they were probably erected by a distinct set of missionaries from the east, at central situations, for affording instruction, by the piety or remorse of individuals. From a consideration of all the circumstances known regarding them, I am inclined to suppose that these peculiar engraved stones of the Pictish kingdom, with crosses, were erected in that native transition period, from the fourth to the eighth century, when Pagan and Christian relics were so curiously mingled.

The large obelisk of Meigle (fig. *w*) may be instanced as an example of a beautiful cross which occupies the upper part of the face, with monsters, and unseemly objects on the lower and outer compartments. On the back is the armed centaur dragging away the sacred tree, with the figures of monsters on the lower parts; above which is the representation of some local tradition, with chiefs on horseback, accompanied with dogs, to indicate the state and rank of the individuals, which they were intended to represent. Upon the St Orleans stone, a beautiful cross (fig. *y*), turned to the east, occupies the face, while the pagan symbols are on the upper part of the back of the stone; whereas on the Fordoun stone, they are at the bottom of the cross, without the crescentic ornament, proving the discredit into which the emblems had fallen. This is still more marked in the obelisk of Golspie, near Dunrobin, where the face represents a graceful and chastely-ornamented cross, and the back a curious collection of pagan emblems (see Drawing, by the Spalding Club). At the top, Providence is represented as hovering over organized matter in the form of an embryo elephant. Under this is a man armed with an uplifted battle-axe, threatening an animal marked for a bloody sacrifice, as is still done in India. In his left hand he holds an open knife over a fish—another emblem; under which is the pyramidal form of organized matter, the third member of the trinity, and the usual crescentic sceptred ornament, which he is in the act of kicking away from their position over the "spectacled ornament," now in the lowest and most degraded part of the stone, and without the sceptres, the emblems of sovereignty. At the bottom of the stone is a nondescript animal, upon the tail of which the armed man rests.

* BEDE, Eccl. Hist., iii., 2.

It is probable that soon after the erection of this obelisk the heathen emblems disappeared from the stones as no longer respected, and may have given place to the simple cross, one of which is built into the wall of the churchyard of Meigle, with no pagan symbols or grotesque additions (fig. z). These sculptured crosses were erected on the side of highways, and in central and convenient situations. As Christianity extended we find St Columba erected them, at a later period, both in stone and wood, in the Christianized part of the island, where the priest afforded instruction to the people, and offered up prayers, before there were churches.* This explains the old Gaelic word "clachan," which signified then the stone, and not, as now, the church.

The great Pictish kingdom north of the Firth of Forth, in which these ancient obelisks are found in such numbers, remained independent till the middle of the ninth century; but from there not being a succession of missionaries, and no provision made to enlighten the Caledonians, they declined in religious knowledge; so that when COLUMBA visited that country in the sixth century in order to convert them, he found their dialect so peculiar that he was obliged to employ an interpreter.† This explains why the names of so many old places in that part of the country, and of the Pictish kings, are neither Irish nor Gaelic. It also explains certain peculiarities in the forms of the cross they used as compared with those of Ireland and Iona.

Were it considered necessary, other facts might be added in proof that in ancient times the same pagan opinions existed in this country as in India, and were supplanted by the Christian faith from the east, to which religion and civilization are again flowing back from the west.

These ancient "standing stones" and beautiful crosses of Scotland have, until lately, been totally neglected, and many owe their preservation to their having been buried by accident or design. In other cases I found they had been, from ignorance, designedly mutilated, or, through carelessness, had been allowed to be destroyed; and a still larger proportion had been removed from their original position, and placed in exposed situations, without any protection. The consequence is that the wet insinuates itself into the interstices, especially of the red sandstone, and in the process of freezing and thawing, the fissures are increased, and the surface of the stone crumbles away. I may instance the large and beautiful cross of Meigle, and that at Golspie, as examples; and, unless measures are speedily taken, they will soon be destroyed by the influence of the weather. Were the cracks filled up with Roman cement, and the whole stone, when dry, saturated with boiled oil, it would arrest the destruction of these most interesting monuments of antiquity.

* Innes' Eccl. Hist. of Scotland, p. 212.

† This is expressly stated in more than one place by his biographer ADOMNAN.

Explanation of PLATE IV.

The Eastern symbols are marked by numbers, and the Western, or those of Scotland, by letters.

Figs. 1 and 2 represent the Trinity; fig. 1 occurring in the great Buddhist temples of India (SYKES), and fig. 2 on the Topes of Bhilsa (CUNNINGHAM).

These are the same as the Trinity in unity, fig. a, on the Kinnellar standing-stone in Aberdeenshire, and of x on the Dingwall stone.

Figs. 3 and 4 represent the members of the Trinity separate; α, β, γ, representing spirit (Buddha), inorganic matter (Dharma), and organized matter (Sangha).

The same is repeated in figs. b and c, on the stones of Scotland, in a form less decorated.

Figs. 5 and 6 are two of the many varieties of the two members of the Trinity, Buddha and Dharma, as represented on coins;* and Sangha, or organized matter, as an animal, generally not completely formed—as an embryo elephant, a hand, an embryo (7), a serpent (7′), a bull (8), &c.

These symbols are very slightly changed in the stones of Scotland. The two members of the Trinity are represented as circles, united by a belt, which is crossed by a bar uniting two sceptres, the ensigns of sovereignty; and the third member, organized matter, is represented as an embryo elephant, a flower (e), an embryo dog (f), a serpent (g), a fish (h), a bird (x).

Fig. m is the only example of the Trinity traced on a rock of Galloway. In this case, organized matter is in the form of a horn, from the navel of which embryo heads proceed. When detached, they apparently communicate with the external world by means of organ-like insect antennæ.

Fig. n represents a serpent, transfixed by a bar united to two sceptres, with an embryo elephant, receiving its influence from the symbol of the Deity.

Fig. 9 is the Buddhist cross.

l is the same cross, as it appears upon the ancient Phœnician pottery and coins ($\dot{\alpha}\ \alpha\ \alpha\ \alpha$), is considered to be the symbol of divine life, and to be the Phœnician letter *Tau* (β). This symbol appears on the fourth line of the Newton stone inscription (γ).

Fig. 11 is a more complicated form of the Buddhist cross, and is called the Buddhist labyrinth—the symbol of providence; of which p is a modification, as it appears on the stones of Scotland.

Fig. o is another variety of the symbol of Providence. There are several others.

Fig. 10 is the representation of a Buddhist temple, dedicated to the Deity; which is indicated by the two eyes (at α).

The same is represented on the stones of Scotland by two eyes (fig. $k\,\alpha$), where the bar crossing the belt, unites the two members of the Trinity, and joins the two sceptres: In this the resemblance is complete.

Fig. 12. The sacred Buddhist tree, represented as rising out of a pot; which resembles that (q) on the Eassie stone of Scotland.

Fig. 13. The sacred Buddhist tree, surrounded by a sacred halo, with two persons worshipping.

The same sacred tree is upon the Farnell stone, Scotland (fig. r), with a sacred serpent on each side, and two figures standing on the pot.

Fig. s represents the sacred bull on the Eassie stone, marked like those still found in India.

Fig. t. The centaur on the Meigle stone, armed with two battle-axes, and dragging a sacred tree after him.

Figs. u and v. The figures of a lion, and a stooping camel, on one of the Meigle stones.

Figs. w, y, z. Specimens of crosses as they appear upon the ancient sculptured stones of Scotland.

* See Journal of Asiatic Society of Bengal, particularly vol. vii., plate 61.

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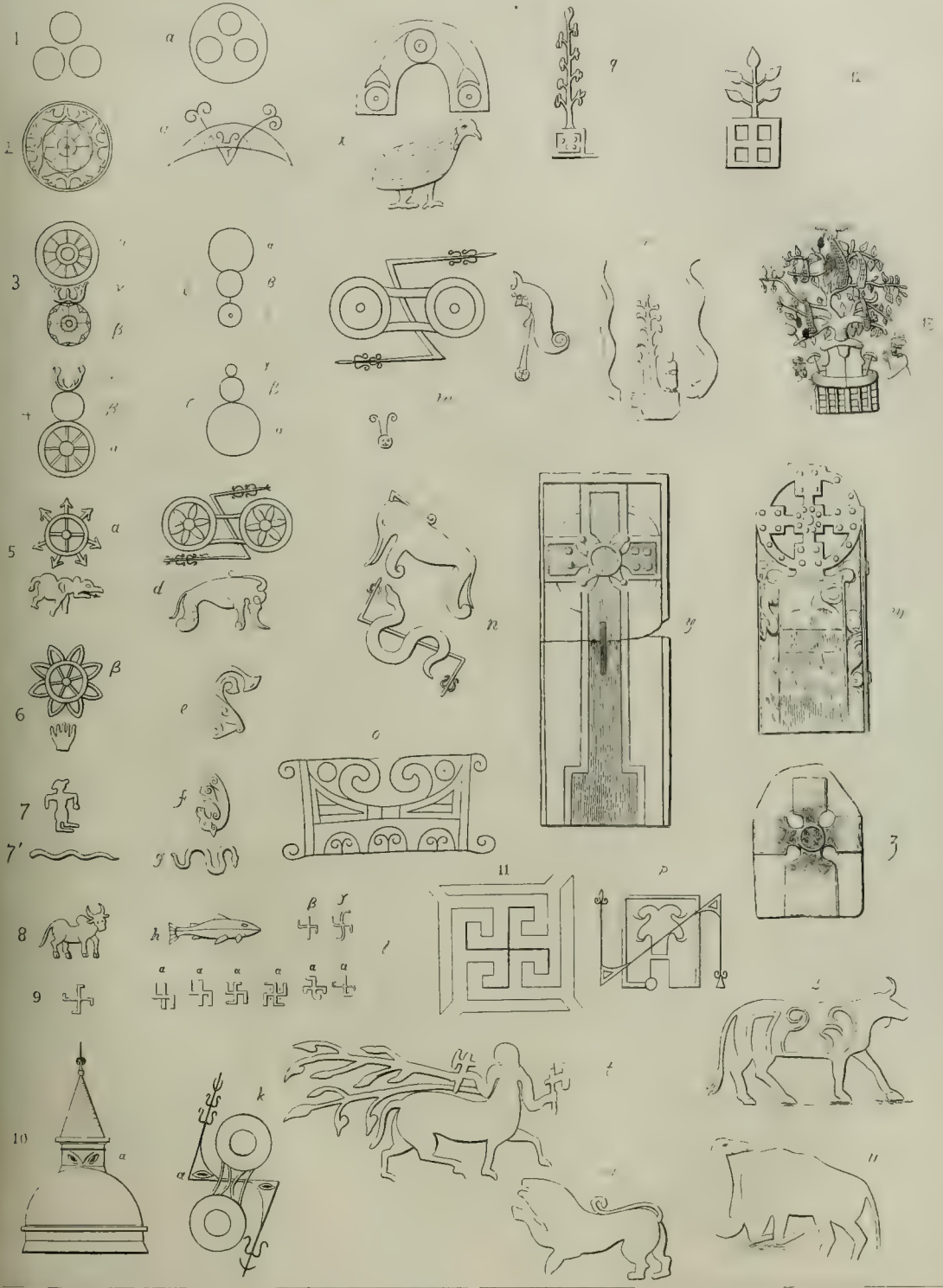


T. Bridgeman del.

Ric. B. Miller sculp.

NATURAL HISTORY.

XVII.—*On Superposition.* By the Rev. Philip Kelland, M.A., Professor of Mathematics in the University of Edinburgh. (With a Plate.)

(Read 19th February 1855.)

The subject which I propose to discuss in this paper is the value of the method of demonstration by superposition. I am satisfied that it has been much underrated, and in some cases misunderstood. It may be stated, that the essential characteristic of this method of demonstration, is the mental comparison of two magnitudes, by placing one of them upon the other. Euclid's axiom of equality (which, perhaps, is rather a definition) is this: "Magnitudes which coincide with one another, that is, which exactly fill the same space, are equal." Accordingly, in his first four books, Euclid never regards two magnitudes as equal, except under circumstances wherein it can be shown that this condition is satisfied. Only in one proposition has he avoided the labour which a strict attention to this requirement necessarily imposes; and perhaps, even in that case, it is hypercritical to object to what he has done. All that he assumes is this: it being admitted that when A fills the same space as B, A is equal to B; it must therefore be admitted, that when A and C together fill the same space as B and C together, A is also equal to B.

In the 6th book, as depending on the 5th, Euclid makes another step in the assumptions necessary, and, in the 2d proposition, he admits the test of equality to be this: When two magnitudes can be multiplied equally to any extent, and when it can be shown, that on every occasion in which one exceeds or falls short of a magnitude, the other does the same exactly; then the two cannot fill other than the same space.

For the next step in this line of argument, we must advance to Newton's Principia. In the 7th Lemma will be found a beautiful process, by which it is shown that two inconceivably small magnitudes are, as they get smaller, approaching to a ratio of equality. The process is simply that of applying a minute magnifying power, so that one of the things to be compared shall always be magnified up to a fixed standard.

This is the *method* of demonstration in geometry. It is not too much to say that geometry owes nine-tenths of its value, as an educational agent, to its being a consistent and systematic exemplification of this method. The treatises of Legendre, in French, and Sir John Leslie, in English, in which *method* is set at defiance, however valuable as introductory to other branches of science, are com-

paratively useless as elements of training. The loose way in which many of these treatises are compiled, causes us to be astonished at the celebrity they have attained. In the treatise of LEGENDRE, for instance, the author attempts, in his first edition, to place the doctrine of parallels on a more simple basis,—at that time, it may be presumed he was tolerably ignorant of the real state of the question,—but when his book became pretty generally adopted, he gave his close attention to the subject, and became convinced that his emendation required amendment. New editions were called for, and new amendments succeeded the old ones, until, in the tenth edition, we are presented with the following significant words as an advertisement: "By the advice of several distinguished professors, I have determined to re-establish, in this tenth edition, the theory of parallels nearly on the same basis as EUCLID'S."

The author has thus sailed round nearly all the points of the compass, and agreeably to his own confession arrives nearly at the point at which he started. *Nearly*, he says; and, truth to tell, not so very nearly at all. There is certainly an abandonment of all novelty in the exposition of the doctrine; but any one may see how very far the author is behind EUCLID, even in his tenth edition. If we could examine the last edition, we should probably find EUCLID'S method completely restored. At any rate, LEGENDRE tells us, in his note, that he is not satisfied with the theory as it stands, and he attributes its imperfection to the definition of a straight line; but whether he means his own definition, which is imperfect enough, no doubt, or EUCLID'S, does not appear.

But it is time we should leave LEGENDRE, and offer one positive argument in favour of the method of demonstration by superposition. It is admitted, I think, that a chain of reasoning upon an abstract definition is the most healthful exercise of the mental powers, at least in the days of youth. Viewed as such, EUCLID'S Elements stand above all other writings; but a class of objectors of a totally different order from LEGENDRE has arisen, who, with considerable show of reason, urge against geometry, as based on superposition, that it excludes all exercise of ingenuity, inasmuch as it only demands a uniform and unvaried march, to deviate from which is to wander into error. There is some truth in this,—but it is not altogether true; and I have here exhibited, in reply to it, some of the different solutions of a single problem where we are bound down by a specific requirement; and it will be seen that the solutions present themselves in tolerable variety, and that their discovery must have brought out some ingenuity.

The problem was proposed to me by the late lamented Secretary of this Society, Sir JOHN ROBISON. I gave him the first solution. The others have arisen out of it, partly from my own suggestions, partly from my students' exercises. It is certainly a very remarkable problem. I have never met with one which presents such a variety of altogether independent solutions. There can be no doubt that the problem has appeared in the Ladies' Diary, or elsewhere, but I

have never met with it, and I do not suppose, at any rate, that half the solutions have been dreamt of before. About these solutions I have not many words to say. It may suffice if I describe the first, and indicate the others by a letter or two placed in the figures. (See Plate V.)

PROBLEM. *From a given square one quarter is cut off, to divide the remaining gnomon into four such parts that they shall be capable of forming a square.*

Let ABE be the gnomon. Let AX be a side of the square which is equal to this gnomon (EUC., ii, 14): call it x, and call AB a. In Figure I. draw HK parallel to AB and equal to $x-a$.

Then (1), (2), and (4) will unite as in Figure II. Also (3) will fit in between (1), (2), and (4). Now, if the four do not make up a square, either (3) will reach below P, or (2) will not reach so low as P, or *vice versâ*, seeing that the area is *equal* to the square of MP. But neither of these circumstances can happen, because then HK + GE would be unequal to MR or x, which it is not. It follows that the four parts exactly make up a square.

The *second* method of making the sections differs from the first only in the different method of producing the portions (2) and (4).

The *third* differs from the first in the way of producing (2) and (3). This method may be modified *ad libitum*. It is only necessary that the portion which (3) takes from (2) shall be symmetrical with respect to C and K.

The *fourth*, like the second, is a slight modification of (2) and (4) in the first. An isosceles triangle is taken out of (4) into (2).

The *fifth* is a modification of the second. A figure equal to (4) is cut out of (2) and remains in (3), whilst (4) itself replaces that figure in (2).

The *sixth* is a new form: (3) and (4) instead of being, as in the first method, cut from the right side, are cut from the left and reversed.

The *seventh* is again a new form.

The *eighth* has two pieces in common with the first method. In other respects it is new.

The *ninth* is a slight modification of the eighth, the piece (4) being cut from the top instead of from the bottom.

The *tenth* is a modification of the seventh.

The *eleventh* is another modification of the eighth; the piece is now cut out of the top of (3).

The *twelfth* is new; and the demonstration is effected by showing that the figures form a rectangular parallelogram, of which one side is x.

I must not omit to add, that the major part of these solutions are due to the ingenuity of my students.

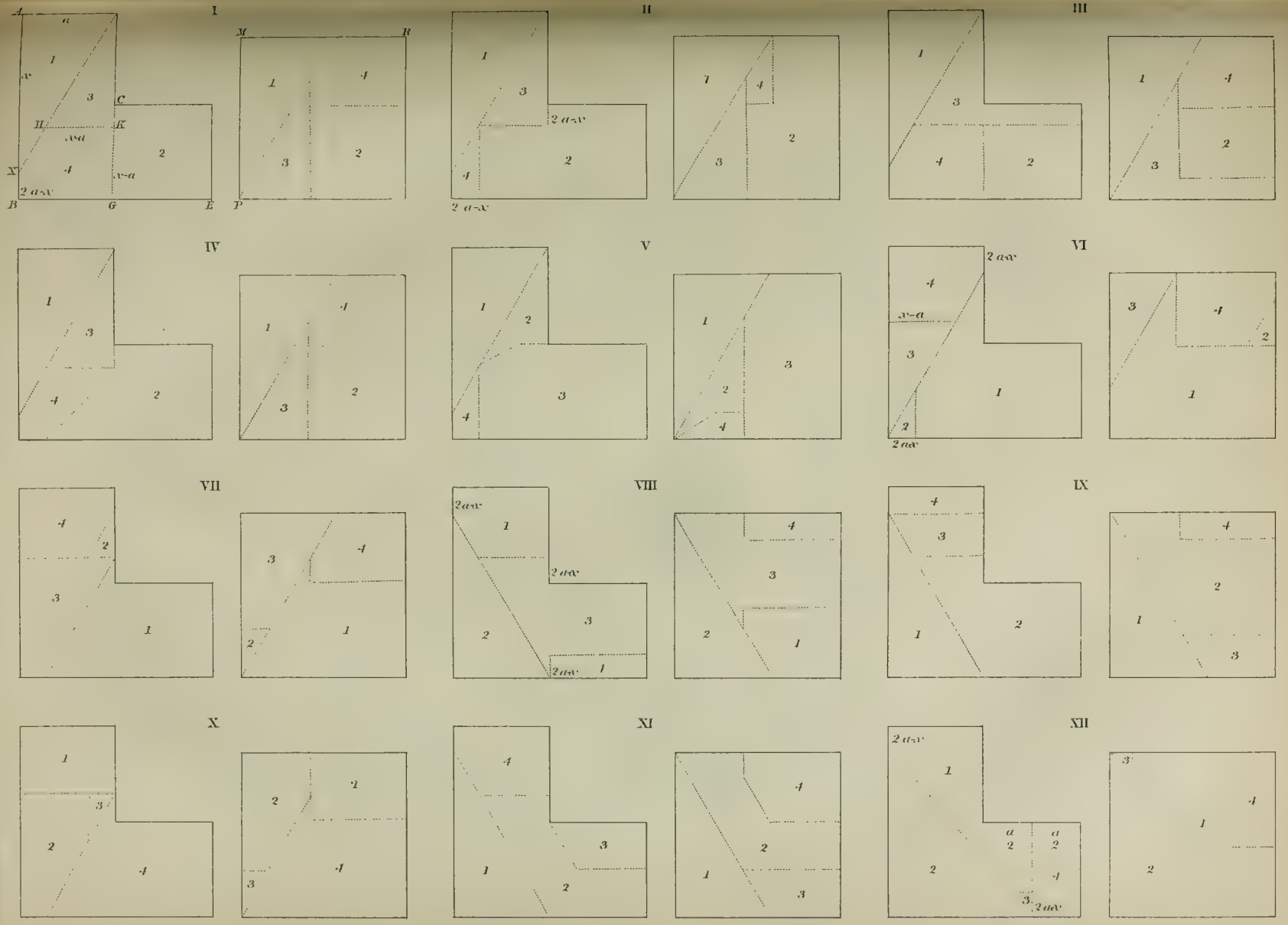


PLATE V Royal Soc Trans Edin Vol XXI

NATURAL HISTORY

XVIII.—*Experiments on Colour, as perceived by the Eye, with remarks on Colour-Blindness.* By JAMES CLERK MAXWELL, B.A., Trinity College, Cambridge. Communicated by Dr GREGORY. (With a Plate.)

(Read 19th March 1855.)

The object of the following communication is to describe a method by which every variety of visible colour may be exhibited to the eye in such a form as to admit of accurate comparison; to show how experiments so made may be registered numerically; and to deduce from these numerical results certain laws of vision.

The different tints are produced by means of a combination of discs of paper, painted with the pigments commonly used in the arts, and arranged round an axis, so that a sector of any required angular magnitude of each colour may be exposed. When this system of discs is set in rapid rotation, the sectors of the different colours become indistinguishable, and the whole appears of one uniform tint. The resultant tints of two different combinations of colours may be compared by using a second set of discs of a smaller size, and placing these over the centre of the first set, so as to leave the outer portion of the larger discs exposed. The resultant tint of the first combination will then appear in a ring round that of the second, and may be very carefully compared with it.

The form in which the experiment is most manageable is that of the common top. An axis, of which the lower extremity is conical, carries a circular plate, which serves as a support for the discs of coloured paper. The circumference of this plate is divided into 100 equal parts, for the purpose of ascertaining the proportions of the different colours which form the combination. When the discs have been properly arranged, the upper part of the axis is screwed down, so as to prevent any alteration in the proportions of the colours.

The instrument used in the first series of experiments (at Cambridge, in November 1854) was constructed by myself, with coloured papers procured from Mr D. R. HAY. The experiments made in the present year were with the improved top made by Mr J. M. BRYSON, Edinburgh, and coloured papers prepared by Mr T. PURDIE, with the unmixed pigments used in the arts. A number of Mr BRYSON's tops, with Mr PURDIE's coloured papers has been prepared, so as to afford different observers the means of testing and comparing results independently obtained.

The colours used for Mr PURDIE's papers were—

Vermilion,	.	V	Ultramarine,	.	U	Emerald Green,	EG
Carmine,	.	C	Prussian Blue,	.	PB	Brunswick Green,	BG
Red Lead,	.	RL	Verditer Blue,	.	VB	Mixture of Ultramarine and Chrome,	UC
Orange Orpiment,	.	OO					
Orange Chrome,	.	OC					
Chrome Yellow,	.	CY					
Gamboge,	.	Gam					
Pale Chrome,	.	PC					
			Ivory Black,	.	Bk		
			Snow White,	.	SW		
			White Paper (Pirie, Aberdeen).				

The colours in the first column are reds, oranges, and yellows; those in the second, blues; and those in the third, greens. Vermilion, ultramarine, and emerald green, seem the best colours to adopt in referring the rest to a uniform standard. They are therefore put at the head of the list, as types of three convenient divisions of colour, red, blue, and green.

It may be asked, why some variety of yellow was not chosen in place of green, which is commonly placed among the secondary colours, while yellow ranks as a primary? The reason for this deviation from the received system is, that the colours on the discs do not represent primary colours at all, but are simply specimens of different kinds of paint, and the choice of these was determined solely by the power of forming the requisite variety of combinations. Now, if red, blue, and yellow, had been adopted, there would have been a difficulty in forming green by any compound of blue and yellow, while the yellow formed by vermilion and emerald green is tolerably distinct. This will be more clearly perceived after the experiments have been discussed, by referring to the diagram.

As an example of the method of experimenting, let us endeavour to form a neutral gray by the combination of vermilion, ultramarine, and emerald green. The most perfect results are obtained by two persons acting in concert, when the operator arranges the colours and spins the top, leaving the eye of the observer free from the distracting effect of the bright colours of the papers when at rest.

After placing discs of these three colours on the circular plate of the top, and smaller discs of white and black above them, the operator must spin the top, and demand the opinion of the observer respecting the relation of the outer ring to the inner circle. He will be told that the outer circle is too red, too blue, or too green, as the case may be, and that the inner one is too light or too dark, as compared with the outer. The arrangement must then be changed, so as to render the resultant tint of the outer and inner circles more nearly alike. Sometimes the observer will see the inner circle tinted with the complementary colour of the outer one. In this case the operator must interpret the observation with respect to the outer circle, as the inner circle contains only black and white.

By a little experience the operator will learn how to put his questions, and

how to interpret their answers. The observer should not look at the coloured papers, nor be told the proportions of the colours during the experiments. When these adjustments have been properly made, the resultant tints of the outer and inner circles ought to be perfectly indistinguishable, when the top has a sufficient velocity of rotation. The number of divisions occupied by the different colours must then be read off on the edge of the plate, and registered in the form of an equation. Thus, in the preceding experiment we have vermilion, ultramarine, and emerald green outside, and black and white inside. The numbers, as given by an experiment on the 6th March 1855, in daylight without sun, are—

$$\cdot 37\ \mathrm{V} + \cdot 27\ \mathrm{U} + \cdot 36\ \mathrm{EG} = \cdot 28\ \mathrm{SW} + \cdot 72\ \mathrm{Bk} \quad . \quad . \quad . \quad . \quad . \quad (1).$$

The method of treating these equations will be given when we come to the theoretical view of the subject.

In this way we have formed a neutral gray by the combination of the three standard colours. We may also form neutral grays of different intensities by the combination of vermilion and ultramarine with the other greens, and thus obtain the quantities of each necessary to neutralize a given quantity of the proposed green. By substituting for each standard colour in succession one of the colours which stand under it, we may obtain equations, each of which contains two standard colours, and one of the remaining colours.

Thus, in the case of pale chrome, we have, from the same set of experiments,

$$\cdot 34\ \mathrm{PC} + \cdot 55\ \mathrm{U} + \cdot 12\ \mathrm{EG} = \cdot 37\ \mathrm{SW} + \cdot 63\ \mathrm{Bk} \quad . \quad . \quad . \quad . \quad (2).$$

We may also make experiments in which the resulting tint is not a neutral gray, but a decided colour. Thus we may combine ultramarine, pale chrome, and black, so as to produce a tint identical with that of a compound of vermilion and emerald green. Experiments of this sort are more difficult, both from the inability of the observer to express the difference which he detects in two tints which have, perhaps, the same hue and intensity, but differ in purity; and also from the complementary colours which are produced in the eye after gazing too long at the colours to be compared.

The best method of arriving at a result in the case before us, is to render the *hue* of the red and green combination something like that of the yellow, to reduce the *purity* of the yellow by the admixture of blue, and to diminish its *intensity* by the addition of black. These operations must be repeated and adjusted, till the two tints are not merely varieties of the same colour, but absolutely the same. An experiment made 5th March gives—

$$\cdot 39\ \mathrm{PC} + \cdot 21\ \mathrm{U} + \cdot 40\ \mathrm{Bk} = \cdot 59\ \mathrm{V} + \cdot 41\ \mathrm{EG} \quad . \quad . \quad . \quad . \quad . \quad (3).$$

That these experiments are really evidence relating to the constitution of the eye, and not mere comparisons of two things which are in themselves identical, may be shown by observing these resultant tints through coloured glasses, or by using

gas-light instead of day-light. The tints which before appeared identical will now be manifestly different, and will require alteration, to reduce them to equality.

Thus, in the case of carmine, we have by day-light,

$$\cdot 44\ C + \cdot 22\ U + \cdot 34\ EG = \cdot 17\ SW + \cdot 83\ Bk$$

while by gas-light (Edinburgh)

$$\cdot 47\ C + \cdot 08\ U + \cdot 45\ EG = \cdot 25\ SW + \cdot 75\ Bk$$

which shows that the yellowing effect of the gas-light tells more on the white than on the combination of colours. If we examine the two resulting tints which appeared identical in experiment (3), observing the whirling discs through a blue glass, the combination of yellow, blue, and black, appears redder than the other, while through a yellow glass, the red and green mixture appears redder. So also a red glass makes the first side of the equation too dark, and a green glass makes it too light.

The apparent identity of the tints in these experiments is therefore not real, but a consequence of a determinate constitution of the eye, and hence arises the importance of the results, as indicating the laws of human vision.

The first result which is worthy of notice is, that the equations, as observed by different persons of ordinary vision, agree in a remarkable manner. If care be taken to secure the same kind of light in all the experiments, the equations, as determined by two independent observers, will seldom show a difference of more than three divisions in any part of the equation containing the bright standard colours. As the duller colours are less active in changing the resultant tint, their true proportions cannot be so well ascertained. The accuracy of vision of each observer may be tested by repeating the same experiment at different times, and comparing the equations so found.

Experiments of this kind, made at Cambridge in November 1854, show that of ten observers, the best were accurate to within $1\frac{1}{2}$ division, and agreed within 1 division of the mean of all; and the worst contradicted themselves to the extent of 6 degrees, but still were never more than 4 or 5 from the mean of all the observations.

We are thus led to conclude—

1*st*, That the human eye is capable of estimating the likeness of colours with a precision which in some cases is very great.

2*d*, That the judgment thus formed is determined, not by the real identity of the colours, but by a cause residing in the eye of the observer.

3*d*, That the eyes of different observers vary in accuracy, but agree with each other so nearly as to leave no doubt that the law of colour-vision is identical for all ordinary eyes.

Investigation of the Law of the Perception of Colour.

Before proceeding to the deduction of the elementary laws of the perception of colour from the numerical results previously obtained, it will be desirable to point out some general features of the experiments which indicate the form which these laws must assume.

Returning to experiment (1), in which a neutral gray was produced from red, blue, and green, we may observe, that, while the adjustments were incomplete, the difference of the tints could be detected only by one circle appearing more red, more green, or more blue than the other, or by being lighter or darker, that is, having an excess or defect of all the three colours together. Hence it appears that the nature of a colour may be considered as dependent on *three* things, as, for instance, redness, blueness, and greenness. This is confirmed by the fact, that any tint may be imitated by mixing red, blue, and green alone, provided that tint does not exceed a certain brilliancy.

Another way of showing that colour depends on *three* things is, by considering how two tints, say two lilacs, may differ. In the first place, one may be *lighter* or *darker* than the other, that is, the tints may differ in *shade*. Secondly, one may be more *blue* or more *red* than the other, that is, they may differ in *hue*. Thirdly, one may be more or less *decided* in its colour; it may vary from purity on the one hand, to neutrality on the other. This is sometimes expressed by saying that they may differ in *tint*.

Thus, in shade, hue, and tint, we have another mode of reducing the elements of colour to three. It will be shown that these two methods of considering colour may be deduced one from the other, and are capable of exact numerical comparison.

On a Graphical Method of Exhibiting the Relations of Colours.

The method which exhibits to the eye most clearly the results of this theory of the three elements of colour, is that which supposes each colour to be represented by a point in space, whose distances from three co-ordinate planes are proportional to the three elements of colour. But as any method by which the operations are confined to a plane is preferable to one requiring space of three dimensions, we shall only consider for the present that which has been adopted for convenience, founded on NEWTON's Circle of Colours and MAYER and YOUNG's Triangle.

Vermilion, ultramarine, and emerald green, being taken (for convenience) as standard colours, are conceived to be represented by three points, taken (for convenience) at the angles of an equilateral triangle. Any colour compounded of these three is to be represented by a point found by conceiving masses propor-

tional to the several components of the colour placed at their respective angular points, and taking the centre of gravity of the three masses. In this way, each colour will indicate by its position the proportions of the elements of which it is composed. The total intensity of the colour is to be measured by the whole number of divisions of V, U, and EG, of which it is composed. This may be indicated by a number or coefficient appended to the name of the colour, by which the number of divisions it occupies must be multiplied to obtain its mass in calculating the results of new combinations.

This will be best explained by an example on the diagram (No. 1). We have, by experiment (1),

$$\cdot 37\ \mathrm{V} + \cdot 27\ \mathrm{U} + \cdot 36\ \mathrm{EG} = \cdot 28\ \mathrm{SW} + \cdot 72\ \mathrm{Bk}$$

To find the position of the resultant neutral tint, we must conceive a mass of ·37 at V, of ·27 at U, and of ·36 at EG, and find the centre of gravity. This may be done by taking the line UV, and dividing it in the proportion of ·37 to ·27 at the point a, where

$$a\,\mathrm{V} : a\,\mathrm{U} :: \cdot 27 : \cdot 37$$

Then, joining a with EG, divide the joining line in W in the proportion of ·36 to (·37 + ·27), W will be the position of the neutral tint required, which is not white, but 0·28 of white, diluted with 0·72 of black, which has hardly any effect whatever, except in decreasing the amount of the other colour. The total intensity of our white paper will be represented by $\frac{1}{0\cdot 28} = 3\cdot 57$; so that, whenever white enters into an equation, the number of divisions must be multiplied by the coefficient 3·57 before any true results can be obtained.

We may take, as the next example, the method of representing the relation of pale chrome to the standard colours on our diagram, by making use of experiment (2), in which pale chrome, ultramarine, and emerald green, produced a neutral gray. The resulting equation was

$$\cdot 33\ \mathrm{PC} + \cdot 55\ \mathrm{U} + \cdot 12\ \mathrm{EG} = \cdot 37\ \mathrm{SW} + \cdot 63\ \mathrm{Bk} \quad . \quad . \quad . \quad . \quad (2).$$

In order to obtain the total intensity of white, we must multiply the number of divisions, ·37, by the proper coefficient, which is 3·57. The result is 1·32, which therefore measures the total intensity on both sides of the equation.

Subtracting the intensity of ·55 U + ·12 EG, or ·67 from 1·32, we obtain ·65 as the *corrected* value of ·32 PC. It will be convenient to use these corrected values of the different colours, taking care to distinguish them by small initials instead of capitals.

Equation (2) then becomes

$$\cdot 65\ \mathrm{p\,c} + \cdot 55\ \mathrm{U} + \cdot 12\ \mathrm{EG} = 1\cdot 32\ \mathrm{w}$$

Hence p c must be situated at a point such that w is the centre of gravity of ·65 p c + ·55 U + ·12 EG.

To find it, we begin by determining β the centre of gravity of ·55 U + ·12 EG, then, joining β w, the point we are seeking must lie at a certain distance on the other side of w from c. This distance may be found from the proportion,

$$\cdot 65 : (\cdot 55 + \cdot 12) :: \overline{\beta\, w} : \overline{w\, p\, c}$$

which determines the position of p c. The proper coefficient, by which the observed values of PC must be corrected is $\frac{65}{33}$, or 1·97.

We have thus determined the position and coefficient of a colour by a single experiment, in which it was made to produce a neutral tint along with two of the standard colours. As this may be done with every possible colour, the method is applicable wherever we can obtain a disc of the proposed colour. In this way the diagram (No. 1) has been laid down from observations made in daylight, by a good eye of the ordinary type.

It has been observed that experiments, in which the resultant tint is neutral, are more accurate than those in which the resulting tint has a decided colour, as in experiment (3), owing to the effects of accidental colours produced in the eye in the latter case. These experiments, however, may be repeated till a very good mean result has been obtained.

But since the elements of every colour have been already fixed by our previous observations and calculations, the agreement of these results with those calculated from the diagram forms a test of the correctness of our method.

By experiment (No. 3), made at the same time with (1) and (2), we have

$$\cdot 39\ PC + \cdot 21\ U + \cdot 40\ Bk = \cdot 59\ V + \cdot 41\ EG \quad . \quad . \quad . \quad (3).$$

Now, joining U with p c, and V with EG, the only common point is that at which they cross, namely γ.

Measuring the parts of the line $\overline{V\, EG}$, we find them in the proportion of

$$\cdot 58\ V \text{ and } \cdot 42\ EG = 1 \cdot 00\ \gamma$$

Similarly, the line $\overline{U\, p\, c}$ is divided in the proportion

$$\cdot 78\ p\, c \text{ and } \cdot 22\ U = 1 \cdot 00\ \gamma$$

But ·78 p c must be divided by 1·97, to reduce it to PC, as was previously explained. The result of calculation is, therefore,

$$\cdot 39\ PC + \cdot 22\ U + \cdot 39\ Bk = \cdot 58\ V + \cdot 42\ EG$$

the black being introduced simply to fill up the circle.

This result differs very little from that of experiment (3), and it must be recollected that these are single experiments,made independently of theory, and chosen at random.

Experiments made at Cambridge, with all the combinations of five colours, show that theory agrees with calculation always within 0·012 of the whole, and sometimes within 0·002. By the repetition of these experiments at the numerous opportunities which present themselves, the accuracy of the results may be rendered still greater. As it is, I am not aware that the judgments of the human eye with respect to colour have been supposed capable of so severe a test.

Further consideration of the Diagram of Colours.

We have seen how the composition of any tint, in terms of our three standard colours, determines its position on the diagram and its proper coefficient. In the same way, the result of mixing any other colours, situated at other points of the diagram, is to be found by taking the centre of gravity of their *reduced masses*, as was done in the last calculation (experiment 3).

We have now to turn our attention to the general aspect of the diagram.

The standard colours, V, U, and EG, occupy the angles of an equilateral triangle, and the rest are arranged in the order in which they participate in red, blue, and green, the neutral tint being at the point w within the triangle. If we now draw lines through w to the different colours ranged round it, we shall find that, if we pass from one line to another in the order in which they lie from red to green, and through blue back again to red, the order will be—

	Coefficient.		Coefficient.
Carmine,	0·4	Pale Chrome	2·0
Vermilion,	1·0	Mixed Green (U C),	0·4
Red Lead,	1·3	Brunswick Green,	0·2
Orange Orpiment,	1·0	*Emerald Green*,	1·0
Orange Chrome,	1·6	Verditer Blue,	0·8
Chrome Yellow,	1·5	Prussian Blue,	0·1
Gamboge,	1·8	*Ultramarine*,	1·0

It may be easily seen that this arrangement of the colours corresponds to that of the prismatic spectrum; the only difference being that the spectrum is deficient in those fine purples which lie between ultramarine and vermilion, and which are easily produced by mixture. The experiments necessary for determining the exact relation of this list to the lines in the spectrum are not yet completed.

If we examine the colours represented by different points in one of these lines through w, we shall find the purest and most decided colours at its outer extremity, and the faint tints approaching to neutrality nearer to w.

If we also study the coefficients attached to each colour, we shall find that the brighter and more luminous colours have higher numbers for their coefficients than those which are dark.

In this way, the qualities which we have already distinguished as hue, tint, and shade, are represented on the diagram by angular position with respect to w, distance from w, and coefficient; and the relation between the two methods of reducing the elements of colour to three becomes a matter of geometry.

Theory of the Perception of Colour.

Opticians have long been divided on this point; those who trusted to popular notions and their own impressions adopting some theory of three primary colours, while those who studied the phenomena of light itself proved that no such theory could explain the constitution of the spectrum. NEWTON, who was the first to demonstrate the actual existence of a series of kinds of light, countless in number, yet all perfectly distinct, was also the first to propound a method of calculating the effect of the mixture of various coloured light; and this method was substantially the same as that which we have just verified. It is true, that the directions which he gives for the construction of his circle of colours are somewhat arbitrary, being probably only intended as an indication of the general nature of the method, but the method itself is mathematically reducible to the theory of three elements of the colour-sensation.*

YOUNG, who made the next great step in the establishment of the theory of light, seems also to have been the first to follow out the necessary consequences of NEWTON's suggestion on the mixture of colours. He saw that, since this triplicity has no foundation in the theory of light, its cause must be looked for in the constitution of the eye; and, by one of those bold assumptions which sometimes express the result of speculation better than any cautious trains of reasoning, he attributed it to the existence of three distinct modes of sensation in the retina, each of which he supposed to be produced in different degrees by the different rays. These three elementary effects, according to his view, correspond to the three sensations of red, green, and violet, and would separately convey to the sensorium the sensation of a red, a green, and a violet picture; so that by the superposition of these pictures, the actual variegated world is represented.†

In order fully to understand YOUNG's theory, the function which he attributes to each system of nerves must be carefully borne in mind. Each nerve acts, not, as some have thought, by conveying to the mind the knowledge of the length of an undulation of light, or of its periodic time, but simply by being *more* or *less* affected by the rays which fall on it. The sensation of each elementary nerve is capable only of increase and diminution, and of no other change. We must also observe, that the nerves corresponding to the red sensation are affected chiefly by the red rays, but in some degree also by those of every other part of the spectrum; just as red glass transmits red rays freely, but also suffers those of other colours to pass in smaller quantity.

This theory of colour may be illustrated by a supposed case taken from

* See Note III. For a confirmation of NEWTON's analysis of Light, see HELMHOLTZ. POGG. Ann. 1852; and Phil. Mag. 1852, Part II.

† YOUNG's Lectures, p. 345, KELLAND's Edition. See also HELMHOLTZ's statement of YOUNG's Theory, in his Paper referred to in Note I.; and HERSCHEL's Light, Art. 518.

the art of photography. Let it be required to ascertain the colours of a landscape, by means of impressions taken on a preparation equally sensitive to rays of every colour.

Let a plate of red glass be placed before the camera, and an impression taken. The positive of this will be transparent wherever the red light has been abundant in the landscape, and opaque where it has been wanting. Let it now be put in a magic lantern, along with the red glass, and a red picture will be thrown on the screen.

Let this operation be repeated with a green and a violet glass, and, by means of three magic lanterns, let the three images be superimposed on the screen. The colour of any point on the screen will then depend on that of the corresponding point of the landscape; and, by properly adjusting the intensities of the lights, &c., a complete copy of the landscape, as far as visible colour is concerned, will be thrown on the screen. The only apparent difference will be, that the copy will be more subdued, or less pure in tint, than the original. Here, however, we have the process performed twice—first on the screen, and then on the retina.

This illustration will show how the functions which YOUNG attributes to the three systems of nerves may be imitated by optical apparatus. It is therefore unnecessary to search for any direct connection between the lengths of the undulations of the various rays of light and the sensations as felt by us, as the threefold partition of the properties of light may be effected by physical means. The remarkable correspondence between the results of experiments on different individuals would indicate some anatomical contrivance identical in all. As there is little hope of detecting it by dissection, we may be content at present with any subsidiary evidence which we may possess. Such evidence is furnished by those individuals who have the defect of vision which was described by DALTON, and which is a variety of that which Dr G. WILSON has lately investigated, under the name of Colour-Blindness.

Testimony of the Colour-blind with respect to Colour.

Dr GEORGE WILSON has described a great number of cases of colour-blindness, some of which involve a general indistinctness in the appreciation of colour, while in others, the errors of judgment are plainly more numerous in those colours which approach to red and green, than among those which approach to blue and yellow. In these more definite cases of colour-blindness, the phenomena can be tolerably well accounted for by the hypothesis of an insensibility to red light; and this is, to a certain extent, confirmed by the fact, that red objects appear to these eyes decidedly more obscure than to ordinary eyes. But by experiments made with the pure spectrum, it appears that though the red appears much more obscure than other colours, it is not wholly invisible, and, what is more curious, resembles the green more than any other colour. The spectrum to them

appears faintly luminous in the red; bright yellow from orange to yellow, bright but not coloured from yellow-green to blue, and then strongly coloured in the extreme blue and violet, after which it seems to approach the neutral obscure tint of the red. It is not easy to see why an insensibility to red *rays* should deprive the green rays, which have no *optical* connection with them, of their distinctive appearance. The phenomena seem rather to lead to the conclusion that it is the red *sensation* which is wanting, that is, that supposed system of nerves which is affected in various degrees by all light, but chiefly by red. We have fortunately the means of testing this hypothesis by numerical results.

Of the subjects of my experiments at Cambridge, four were decided cases of colour-blindness. Of these two, namely, Mr R. and Mr S., were not sufficiently critical in their observations to afford any results consistent within 10 divisions of the colour-top. The remaining two, Mr N. and Mr X., were as consistent in their observations as any persons of ordinary vision can be, while the results showed all the more clearly how completely their sensations must differ from ours.

The method of experimenting was the same as that adopted with ordinary eyes, except that in these cases the operator can hardly influence the result by yielding to his own impressions, as he has no perception whatever of the similarity of the two tints as seen by the observer. The questions which he must ask are two, Which circle appears most blue or yellow? Which appears lightest and which darkest? By means of the answers to these questions he must adjust the resulting tints to equality in these respects as it appears to the observer, and then ascertain that these tints now present no difference of colour whatever to his eyes. The equations thus obtained do not require five colours including black, but four only. For instance, the mean of several observations gives—

$$\cdot 19\ G + \cdot 05\ B + \cdot 76\ Bk = 100\ R \qquad . \quad . \quad . \quad . \quad . \quad (4).$$

[In these experiments R, B, G, Y, stand for red, blue, green, and yellow papers prepared by Mr D. R. Hay. I am not certain that they are identical with his standard colours, but I believe so. Their relation to vermilion, ultramarine, and emerald-green is given in diagram (1). Their relations to each other are very accurately given in diagram (2).]

It appears, then, that the dark blue-green of the left side of the equation is equivalent to the full red of the right side.

Hence, if we divide the line BG in the proportion of 19 to 5 at the point β, and join Rβ, the tint at β will differ from that at R (to the colour-blind) only in being more brilliant in the proportion of 100 to 24, and all intermediate tints on the line Rβ will appear to them of the same hue, but of intermediate intensities.

Now, if we take a point D, so that RD is to Rβ in the proportion of 24 to 100−24, or 76, the tint of D, if producible, should be invisible to the colour-blind. D, therefore, represents the pure sensation which is unknown to the colour-blind,

and the addition of this sensation to any others cannot alter it in their estimation. It is for them equivalent to black.

Hence, if we draw lines through D in different directions, the colours belonging to the line ought to differ only in intensity as seen by them, so that one of them may be reduced to the other by the addition of black only. If we draw DW and produce it, all colours on the upper side of DW will be varieties of blue, and those on the under side varieties of yellow, so that the line DW is a boundary line between their two kinds of colour, blue and yellow being the names by which they call them.

The accuracy of this theory will be evident from the comparison of the experiments which I had an opportunity of making on Mr N. and Mr X. with each other, and with measurements taken from the diagram No. 2, which was constructed from the observations of ordinary eyes only, the point D alone being ascertained from a series of observations by Mr N.

Taking the point γ, between R and B, it appears, by measurement of the lines R γ and B γ, that γ corresponds to

$$\cdot 07\ \mathrm{B} + \cdot 93\ \mathrm{R}$$

By measurement of W γ and D γ, and correction by means of the coefficient of W, and calling D black in the colour-blind language, γ corresponds to

$$\cdot 105\ \mathrm{W} + \cdot 895\ \mathrm{Bk}$$

Therefore,

By measurement, . .	·93 R + ·07 B = ·105 W + ·895 Bk	(5).
By observation N. & X. together,	·94 R + ·06 B = ·10 W + ·90 Bk	
By X. alone, . . .	·93 R + ·07 B = ·10 W + ·90 Bk	

The agreement here is as near as can be expected.

By a similar calculation with respect to the point δ, between B and G,

By measurement, .	·43 B + ·57 G = ·335 W + ·665 Bk	(6).
Observed by N. and X.,	·41 B + ·59 G = ·34 W + ·66 Bk	
By X. alone, . .	·42 B + ·58 G = ·32 W + ·68 Bk	

We may also observe, that the line GD crosses RY. At the point of intersection we have—

By calculation, . .	·87 R + ·13 Y = ·34 G + ·66 Bk	(7).
Observed by N. and X.,	·86 R + ·14 Y = ·40 G + ·60 Bk	
... ... X.,	·84 R + ·16 Y = ·31 G + ·69 Bk	
... ... X.,	·90 R + ·10 Y = ·27 G + ·73 Bk	

Here observations are at variance, owing to the decided colours produced affecting the state of the retina, but the mean agrees well with calculation.

Drawing the line BY, we find that it cuts lines through D drawn to every

colour. Hence all colours appear to the colour-blind as if composed of blue and yellow. By measurement on the diagram, we find for red

$$\left.\begin{array}{lll} \text{Measured,} & \cdot138\ Y + \cdot123\ B + \cdot749\ Bk = 100\ R \\ \text{Observed by N.,} & \cdot15\ Y + \cdot11\ B + \cdot74\ Bk = 100\ R \\ \text{...} \quad \text{X.,} & \cdot13\ Y + \cdot11\ B + \cdot76\ Bk = 100\ R \end{array}\right\} \quad (8).$$

For green we have in the same way—

$$\left.\begin{array}{lll} \text{Measured,} & \cdot705\ Y + \cdot295\ B = \cdot95\ G + \cdot05\ Bk \\ \text{Observed by N.,} & \cdot70\ Y + \cdot30\ B = \cdot86\ G + \cdot14\ Bk \\ \text{...} \quad \text{X.,} & \cdot70\ Y + \cdot30\ B = \cdot83\ G + \cdot17\ Bk \end{array}\right\} \quad (9).$$

For white—

$$\begin{array}{lll} \text{Measured,} & \cdot407\ Y + \cdot593\ B = \cdot326\ W + \cdot674\ Bk \\ \text{Observed by N.,} & \cdot40\ Y + \cdot60\ B = \cdot33\ W + \cdot67\ Bk \\ \text{...} \quad \text{X.,} & \cdot44\ Y + \cdot56\ B = \cdot33\ W + \cdot67\ Bk \end{array}$$

The accuracy of these results shows that, whether the hypothesis of the want of one element out of three necessary to perfect vision be actually true or not, it affords a most trustworthy foundation on which to build a theory of colour-blindness, as it expresses completely the observed facts of the case. They also furnish us with a datum for our theory of perfect vision, namely, the point D, which points out the exact nature of the colour-sensation, which must be added to the colour-blind eye to render it perfect. I am not aware of any method of determining by a legitimate process the nature of the other two sensations, although YOUNG's reasons for adopting something like green and violet appear to me worthy of attention.

The only remaining subject to which I would call the attention of the Society is the effect of coloured glasses on the colour-blind. Although they cannot distinguish reds and greens from varieties of gray, the transparency of red and green glasses for those kinds of light is very different. Hence, after finding a case such as that in equation (4), in which a red and a green appear identical, on looking through a red glass they see the red clearly and the green obscurely, while through a green glass the red appears dark and the green light.

By furnishing Mr X. with a red and a green glass, which he could distinguish only by their shape, I enabled him to make judgments in previously doubtful cases of colour with perfect certainty. I have since had a pair of spectacles constructed with one eye-glass red and the other green. These Mr X. intends to use for a length of time, and he hopes to acquire the habit of discriminating red from green tints by their different effects on his two eyes. Though he can never acquire our sensation of red, he may then discern for himself what things are red, and the mental process may become so familiar to him as to act unconsciously like a new sense.

In one experiment, after looking at a bright light, with a red glass over one eye and a green over the other, the two tints in experiment (4) appeared to him altered, so that the outer circle was lighter according to one eye, and the inner according to the other. As far as I could ascertain, it appeared as if the eye which had used the red glass saw the red circle brightest. This result, which seems at variance with what might be expected, I have had no opportunity of verifying.

This paper is already longer than was originally intended. For further information I would refer the reader to NEWTON's Opticks, Book I. Part II., to YOUNG's Lectures on Natural Philosophy, page 345, to Mr D. R. HAY's works on Colours, and to Professor FORBES on the Classification of Colours (Phil. Mag., March 1849).

The most remarkable paper on the subject is that of M. HELMHOLTZ, in the Philosophical Magazine for 1852, in which he discusses the different theories of primary colours, and describes his method of mixing the colours of the spectrum. An examination of the results of M. HELMHOLTZ with reference to the theory of three elements of colour, by Professor GRASSMANN, is translated in the Phil. Mag., April 1854.

References to authors on colour-blindness are given in Dr G. WILSON's papers on that subject. A valuable Letter of Sir J. F. W. HERSCHEL to DALTON on his peculiarity of vision, is to be found in the Life of DALTON by Dr HENRY.

I had intended to describe some experiments on the propriety of the method of mixing colours by rotation, which might serve as an extension of Mr SWAN's experiments on instantaneous impressions on the eye. These, together with the explanation of some phenomena which seem to be at variance with the theory of vision here adopted, must be deferred for the present. On some future occasion, I hope to be able to connect these simple experiments on the colours of pigments with others in which the pure hues of the spectrum are used. I have already constructed a model of apparatus for this purpose, and the results obtained are sufficiently remarkable to encourage perseverance.

NOTE I.

On different Methods of Exhibiting the Mixtures of Colours.

(1.) *Mechanical Mixture of Coloured Powders.*

By grinding coloured powders together, the differently-coloured particles may be so intermingled that the eye cannot distinguish the colours of the separate powders, but receives the impression of a uniform tint, depending on the nature and proportions of the pigments used. In this way, NEWTON mixed the powders of orpiment, purple, bise, and *viride æris*, so as to form a gray, which, in sunlight, resembled white paper in the shade. (NEWTON's Opticks, Book I. Part II. Exp.

XV.) This method of mixture, besides being adopted by all painters, has been employed by optical writers as a means of obtaining numerical results. The specimens of such mixtures given by D. R. Hay in his works on Colour, and the experiments of Professor J. D. Forbes on the same subject, show the importance of the method as a means of classifying colours. There are two objections, however, to this method of exhibiting colours to the eye. When two powders of unequal fineness are mixed, the particles of the finer powder cover over those of the coarser, so as to produce more than their due effect in influencing the resultant tint. For instance, a small quantity of lamp-black, mixed with a large quantity of chalk, will produce a mixture which is nearly black. Although the powders generally used are not so different in this respect as lamp-black and chalk, the results of mixing given weights of any coloured powders must be greatly modified by the mode in which these powders have been prepared.

Again, the light which reaches the eye from the surface of the mixed powders consists partly of light which has fallen on one of the substances mixed without being modified by the other, and partly of light which, by repeated reflection or transmission has been acted on by both substances. The colour of these rays will not be a mixture of those of the substances, but will be the result of the absorption due to both substances successively. Thus, a mixture of yellow and blue produces a neutral tint tending towards red, but the remainder of white light, after passing through both, is green; and this green is generally sufficiently powerful to overpower the reddish gray due to the separate colours of the substances mixed. This curious result has been ably investigated by Professor Helmholtz of Königsberg, in his Memoir on the Theory of Compound Colours, a translation of which may be found in the Annals of Philosophy for 1852, Part 2.

(2.) *Mixture of differently-coloured Beams of Light by Superposition on an Opaque Screen.*

When we can obtain light of sufficient intensity, this method produces the most beautiful results. The best series of experiments of this kind are to be found in Newton's Opticks, Book I. Part II. The different arrangements for mixing the rays of the spectrum on a screen, as described by Newton, form a very complete system of combinations of lenses and prisms, by which almost every possible modification of coloured light may be produced. The principal objections to the use of this method are—(1.) The difficulty of obtaining a constant supply of uniformly intense light; (2.) The uncertainty of the effect of the position of the screen with respect to the incident beams and the eye of the observer; (3.) The possible change in the colour of the incident light due to the *fluorescence* of the substance of the screen. Professor Stokes has found that many substances, when illuminated by homogeneous light of one refrangibility, become themselves luminous, so as to emit light of lower refrangibility. This phenomenon must be carefully attended to when screens are used to exhibit light.

(3.) *Union of Coloured Beams by a Prism so as to form one Beam.*

The mode of viewing the beam of light directly, without first throwing it on a screen, wasnot much used by the older experimenters, but it possesses the advantage of saving much light, and admits of examining the rays before they have been stopped in any way. In NEWTON's 11th proposition of the 2d Book, an experiment is described, in which a beam is analysed by a prism, concentrated by a lens, and recombined by another prism, so as to form a beam of white light similar to the incident beam. By stopping the coloured rays at the lens, any proposed combination may be made to pass into the emergent beam, where it may be received directly by the eye, or on a screen, at pleasure.

The experiments of HELMHOLTZ on the colours of the spectrum were made with the ordinary apparatus for directly viewing the pure spectrum, two oblique slits crossing one another being employed to admit the light instead of one vertical slit. Two pure spectra were then seen crossing each other, and so exhibiting at once a large number of combinations. The proportions of these combinations were altered by varying the inclination of the slits to the plane of refraction, and in this way a number of very remarkable results were obtained,—for which see his Memoir, before referred to.

In experiments of the same kind made by myself in August 1852 (independently of M. HELMHOLTZ), I used a combination of three moveable vertical slits to admit the light, instead of two cross slits, and observed the compound ray through a slit made in a screen on which the pure spectrum is formed. In this way a considerable field of view was filled with the mixed light, and might be compared with another part of the field illuminated by light proceeding from a second system of slits, placed below the first set. The general character of the results agreed with those of M. HELMHOLTZ. The chief difficulties seemed to arise from the defects of the opticala pparatus of my own eye, which rendered apparent the compound nature of the light, by analysing it as a prism or an ordinary lens would do, whenever the lights mixed differed much in refrangibility.

(4.) *Union of two beams by means of a transparent surface, which reflects the first and transmits the second.*

The simplest experiment of this kind is described by M. HELMHOLTZ. He places two coloured wafers on a table, and then, taking a piece of transparent glass, he places it between them, so that the reflected image of one apparently coincides with the other as seen through the glass. The colours are thus mixed, and, by varying the angle of reflection, the relative intensities of the reflected and transmitted beams may be varied at pleasure.

In an instrument constructed by myself for photometrical purposes two reflecting plates were used. They were placed in a square tube, so as to polarize

the incident light, which entered through holes in the sides of the tubes, and was reflected in the direction of the axis. In this way two beams oppositely polarized were mixed, either of which could be coloured in any way by coloured glasses placed over the holes in the tube. By means of a NICOL's prism placed at the end of the tube, the relative intensities of the two colours as they entered the eye could be altered at pleasure.

(5.) *Union of two coloured beams by means of a doubly-refracting Prism.*

I am not aware that this method has been tried, although the opposite polarization of the emergent rays is favourable to the variation of the experiment.

(6.) *Successive presentation of the different Colours to the Retina.*

It has long been known, that light does not produce its full effect on the eye at once, and that the effect, when produced, remains visible for some after the light has ceased to act. In the case of the rotating disc, the various colours become indistinguishable, and the disc appears of a uniform tint, which is in some sense the resultant of the colours so blended. This method of combining colours has been used since the time of NEWTON, to exhibit the results of theory. The experiments of Professor J. D. FORBES, which I witnessed in 1849, first encouraged me to think that the laws of this kind of mixture might be discovered by special experiments. After repeating the well-known experiment in which a series of colours representing those of the spectrum are combined to form gray, Professor FORBES endeavoured to form a neutral tint, by the combination of three colours only. For this purpose, he combined the three so-called primary colours, red, blue, and yellow, but the resulting tint could not be rendered neutral by any combination of these colours; and the reason was found to be, that blue and yellow do not make green, but a pinkish tint, when neither prevails in the combination. It was plain, that no addition of *red* to this, could produce a neutral tint.

This result of mixing blue and yellow was, I believe, not previously known. It directly contradicted the received theory of colours, and seemed to be at variance with the fact, that the same blue and yellow paint, when ground together, do make green. Several experiments were proposed by Professor FORBES, in order to eliminate the effect of motion, but he was not then able to undertake them. One of these consisted in viewing alternate stripes of blue and yellow, with a telescope out of focus. I have tried this, and find the resultant tint pink as before.* I also found that the beams of light coloured by transmission through blue and yellow glasses appeared pink, when mixed on a screen, while a beam of light, after passing through both glasses, appeared green. By the help of the theory of absorption, given by HERSCHEL,† I made out the complete explanation

* See however Encyc. Metropolitana, Art. "Light," section 502. † Ib. sect. 516.

of this phenomenon. Those of pigments were, I think, first explained by HELMHOLTZ in the manner above referred to.*

It may still be asked, whether the effect of successive presentation to the eye is identical with that of simultaneous presentation, for if there is any action of the one kind of light on the other, it can take place only in the case of simultaneous presentation. An experiment tending to settle this point is recorded by NEWTON (Book I. Part II, Exp. 10). He used a comb with large teeth to intercept various rays of the spectrum. When it was moved slowly, the various colours could be perceived, but when the speed was increased the result was perfect whiteness. For another form of this experiment, see NEWTON's Sixth Letter to OLDENBURG (HORSLEY's Edition, vol. iv., page 335.)

In order more fully to satisfy myself on this subject, I took a disc in which were cut a number of slits, so as to divide it into spokes. In a plane, nearly passing through the axis of this disc, I placed a blue glass, so that one half of the disc might be seen by transmitted light—blue, and the other by reflected light—white. In the course of the reflected light I placed a yellow glass, and in this way I had two nearly coincident images of the slits, one yellow and one blue. By turning the disc slowly, I observed that in some parts the yellow slits and the blue slits appeared to pass over the field alternately, while in others they appeared superimposed, so as to produce alternately their mixture, which was pale pink, and complete darkness. As long as the disc moved slowly I could perceive this, but when the speed became great, the whole field appeared uniformly coloured pink, so that those parts in which the colours were seen successively were indistinguishable from those in which they were presented together to the eye.

Another form in which the experiment may be tried requires only the colour-top above described. The disc should be covered with alternate sectors of any two colours, say red and green, disposed alternately in four quadrants. By placing a piece of glass above the top, in the plane of the axis, we make the image of one half seen by reflection coincide with that of the other seen by transmission. It will then be seen that, in the diameters of the top which are parallel and perpendicular to the plane of reflection, the transmitted green coincides with the reflected green, and the transmitted red with the reflected red, so that the result is always either pure red or pure green. But in the diameters intermediate to these, the transmitted red coincides with the reflected green, and *vice versa*, so that the pure colours are never seen, but only their mixtures. As long as the top is spun slowly, these parts of the disc will appear more steady in colour than those in which the greatest alternations take place; but when the speed is sufficiently

* I have lately seen a passage in MOIGNO's Cosmos, stating that M. PLATEAU, in 1819, had obtained gray by whirling together gamboge and Prussian blue.—*Correspondence Math. et Phys.*, de M. QUETELET, vol. v., p. 221.

increased, the disc appears perfectly uniform in colour. From these experiments it appears, that the apparent mixture of colours is not due to a mechanical superposition of vibrations, or to any mutual action of the mixed rays, but to some cause residing in the constitution of the apparatus of vision.

(7.) *Presentation of the Colours to be mixed one to each Eye.*

This method is said not to succeed with some people; but I have always found that the mixture of *colours* was perfect, although it was difficult to conceive the *objects* seen by the two eyes as identical. In using the spectacles, of which one eye is green and the other red, I have found, when looking at an arrangement of green and red papers, that some looked metallic and others transparent. This arises from the very different relations of brightness of the two colours as seen by each eye through the spectacles, which suggests the false conclusion, that these differences are the result of reflection from a polished surface, or of light transmitted through a clear one.

Note II.

Results of Experiments with Mr Hay's *Papers, at Cambridge, November* 1854.

The mean of ten observations made by six observers gave—

$$\cdot 449\,R + \cdot 299\,G + \cdot 252\,B = \cdot 224\,W + \cdot 776\,Bk \quad . \quad . \quad . \quad . \quad (1).$$

$$\cdot 696\,R + \cdot 304\,G = \cdot 181\,B + \cdot 327\,Y + \cdot 492\,Bk \quad . \quad . \quad . \quad . \quad (2).$$

These two equations served to determine the positions of white and yellow in diagram No. 2. The coefficient of W is 4·447, and that of yellow 2·506.

From these data we may deduce three other equations, either by calculation, or by measurement on the diagram (No. 2).

Eliminating green from the equations, we find—

$$\cdot 565\,B + \cdot 435\,Y = \cdot 307\,R + \cdot 304\,W + \cdot 389\,Bk \quad . \quad . \quad . \quad . \quad (3).$$

The mean of three observations by three different observers, gives—

$$\cdot 573\,B + \cdot 477\,Y = \cdot 313\,R + \cdot 297\,W + \cdot 390\,Bk$$

Errors of calculation, $-\cdot 008\,B + \cdot 008\,Y - \cdot 006\,R + \cdot 007\,W - \cdot 001\,Bk$

The point on the diagram to which this equation corresponds is the intersection of the lines BY and RW, and the resultant tint is a pinkish-gray.

Eliminating red from the equations, we obtain—

Calculation, .	·533 B +	·150 G +	·317 Y =	·337 W +	·663 Bk
By 10 observations,	·537 B +	·146 G +	·317 Y =	·337 W +	·663 Bk
Errors, . .	−·004	+·004	—	—	—

. . . . (4).

Eliminating blue,	·660 R +	·340 G =	·218 Y +	·108 W +	·682 Bk
By 5 observations,	·672 R +	·328 G =	·224 Y +	·094 W +	·672 Bk
Errors, . .	−·012	+·012	−·006	+·014	+·008

. . . . (5).

Note III.

On the Theory of Compound Colours.

Newton's theorem on the mixture of colours is to be found in his Opticks, Book I., Pt. II., Prop. VI.

In a mixture of primary colours, the quantity and quality of each being given, to know the colour of the compound.

He divides the circumference of a circle into parts proportional to the seven musical intervals, in accordance with his opinion of the divisions of the spectrum. He then conceives the colours of the spectrum arranged round the circle, and at the centre of gravity of each of the seven arcs he places a little circle, the area of which represents the number of rays of the corresponding colour which enter into the given mixture. He takes the centre of gravity of all these circles to represent the colour formed by the mixture. The *hue* is determined by drawing a line through the centre of the circle and this point to the circumference. The position of this line points out the colour of the spectrum which the mixture most resembles, and the distance of the resultant tint from the centre determines the fulness of its colour.

Newton, by this construction (for which he gives no reasons), plainly shows that he considered it possible to find a place within his circle for every possible colour, and that the entire nature of any compound colour may be known from its place in the circle. It will be seen that the same colour may be compounded from the colours of the spectrum in an infinite variety of ways. The apparent identity of all these mixtures, which are optically different, as may be shown by the prism, implies some law of vision not explicitly stated by Newton. This law, if Newton's method be true, must be that which I have endeavoured to establish, namely, the threefold nature of sensible colour.

With respect to Newton's construction, we now know that the proportions of the colours of the spectrum vary with the nature of the refracting medium. The only *absolute* index of the kind of light is the *time* of its vibration. The *length* of its vibration depends on the medium in which it is; and if any proportions are to be sought among the wave-lengths of the colours, they must be determined for those tissues of the eye in which their physical effects are supposed to terminate. It may be remarked, that the apparent colour of the spectrum changes most rapidly at three points, which lie respectively in the yellow, between blue and green, and between violet and blue. The wave-lengths of the corresponding rays *in water* are in the proportions of three geometric means between 1 and 2 very nearly. This result, however, is not to be considered established, unless confirmed by better observations than mine.

The only safe method of completing Newton's construction is by an examina-

tion of the colours of the spectrum and their mixtures, and subsequent calculation by the method used in the experiments with coloured papers. In this way I hope to determine the relative positions in the colour-diagram of every ray of the spectrum, and its relative intensity in the solar light. The spectrum will then form a curve not necessarily circular or even re-entrant, and its peculiarities so ascertained may form the foundation of a more complete theory of the colour-sensation.

On the relation of the pure rays of the Spectrum to the three assumed Elementary Sensations.

If we place the three elementary colour-sensations (which we may call, after YOUNG, red, green, and violet) at the angles of a triangle, all colours which the eye can possibly perceive (whether by the action of light, or by pressure, disease, or imagination), must be somewhere within this triangle, those which lie farthest from the centre being the fullest and purest colours. Hence the colours which lie at the middle of the sides are the purest of their kind which the eye can see, although not so pure as the elementary sensations.

It is natural to suppose that the pure red, green, and violet rays of the spectrum produce the sensations which bear their names in the highest purity. But from this supposition it would follow that the yellow, composed of the red and green of the spectrum, would be the most intense yellow possible, while it is the result of experiment, that the yellow of the spectrum itself is much more full in colour. Hence the sensations produced by the pure red and green rays of the spectrum are not the pure sensations of our theory. NEWTON has remarked, that no two colours of the spectrum produce, when mixed, a colour equal in fulness to the intermediate colour. The colours of the spectrum are all more intense than any compound ones. Purple is the only colour which must be produced by combination. The experiments of HELMHOLTZ lead to the same conclusion; and hence it would appear that we can find no part of the spectrum which produces a pure sensation.

An additional, though less satisfactory evidence of this, is supplied by the observation of the colours of the spectrum when excessively bright. They then appear to lose their peculiar colour, and to merge into pure whiteness. This is probably due to the want of capacity of the organ to take in so strong an impression; one sensation becomes first saturated, and the other two speedily follow it, the final effect being simple brightness.

From these facts I would conclude, that every ray of the spectrum is capable of producing all three pure sensations, though in different degrees. The curve, therefore, which we have supposed to represent the spectrum will be quite within the triangle of colour. All natural or artificial colours, being compounded of the colours of the spectrum, must lie within this curve, and, therefore, the colours corresponding to those parts of the triangle beyond this curve must be for ever

unknown to us. The determination of the exact nature of the pure sensations, or of their relation to ordinary colours, is therefore impossible, unless we can prevent them from interfering with each other as they do. It may be possible to experience sensations more pure than those directly produced by the spectrum, by first exhausting the sensibility to one colour by protracted gazing, and then suddenly turning to its opposite. But if, as I suspect, colour-blindness be due to the absence of one of these sensations, then the point D in diagram (2), which indicates their absent sensation, indicates also our pure sensation, which we may call red, but which we can never experience, because all kinds of light excite the other sensations.

Newton has stated one objection to his theory, as follows:—"*Also, if only two of the primary colours, which in the circle are opposite to one another, be mixed in an equal proportion, the point Z*" (the resultant tint) "*shall fall upon the centre O*" (neutral tint); "*and yet the colour compounded of these two shall not be perfectly white, but some faint anonymous colour. For I could never yet, by mixing only two primary colours, produce a perfect white.*" This is confirmed by the experiments of Helmholtz; who, however has succeeded better with some pairs of colours than with others.

In my experiments on the spectrum, I came to the same result; but it appeared to me that the very peculiar appearance of the neutral tints produced was owing to some optical effect taking place in the transparent part of the eye on the mixture of two rays of very different refrangibility. Most eyes are by no means achromatic, so that the images of objects illuminated with mixed light of this kind appear divided into two different colours; and even when there is no distinct object, the mixtures become in some degree analysed, so as to present a very strange, and certainly "anonymous" appearance.

Additional Note on the more recent experiments of M. Helmholtz.*

In his former memoir on the Theory of Compound Colours,† M. Helmholtz arrived at the conclusion that only one pair of homogeneous colours, orange-yellow and indigo-blue, were strictly complementary. This result was shown by Professor Grassmann‡ to be at variance with Newton's theory of compound colours; and although the reasoning was founded on intuitive rather than experimental truths, it pointed out the tests by which Newton's theory must be verified or overthrown. In applying these tests, M. Helmholtz made use of an apparatus similar to that described by M. Foucault,§ by which a screen of white paper is illuminated by the mixed light. The field of mixed colour is much larger than

* Poggendorff's Annalen, Bd. xciv. (I am indebted for the perusal of this Memoir to Professor Stokes.)

† Ib. Bd. lxxxvii. Annals of Philosophy, 1852, Part II.

‡ Ib. Bd. lxxxix., Ann. Phil., 1854, April.

§ Ib. Bd. lxxxviii. Moigno, Cosmos, 1853, Tom. ii., p. 232.

in M. HELMHOLTZ's former experiments, and the facility of forming combinations is much increased. In this memoir the mathematical theory of NEWTON's circle, and of the curve formed by the spectrum, with its possible transformations, is completely stated, and the form of this curve is in some degree indicated, as far as the determination of the colours which lie on opposite sides of white, and of those which lie opposite the part of the curve which is wanting. The colours between red and yellow-green are complementary to colours between blue-green and violet, and those between yellow-green and blue-green have no homogeneous complementaries, but must be neutralized by various hues of purple, *i.e.*, mixtures of red and violet. The names of the complementary colours, with their wave-lengths in air, as deduced from FRAUNHOFER's measurements, are given in the following table:—

Colour.	Wave-length.	Complementary Colour.	Wave-length.	Ratio of Wave-lengths.
Red, . . .	2425	Green-blue, .	1818	1·334
Orange, . .	2244	Blue, . . .	1809	1·240
Gold-yellow,	2162	Blue, . . .	1793	1·206
Gold-yellow,	2120	Blue, . . .	1781	1·190
Yellow, . .	2095	Indigo-blue, .	1716	1·221
Yellow, . .	2085	Indigo-blue, .	1706	1·222
Green-yellow,	2082	Violet, . .	1600—	1·301

(The wave-lengths are expressed in millionths of a Paris inch.)

(In order to reduce these wave-lengths to their actual length in the eye, each must be divided by the index of refraction for that kind of light in the medium in which the physical effect of the vibrations is supposed to take place.)

Although these experiments are not in themselves sufficient to give the complete theory of the curve of homogeneous colours, they determine the most important element of that theory in a way which seems very accurate, and I cannot doubt that when a philosopher who has so fully pointed out the importance of general theories in physics turns his attention to the theory of sensation, he will at least establish the principle that the laws of sensation can be successfully investigated only after the corresponding physical laws have been ascertained, and that the connection of these two kinds of laws can be apprehended only when the distinction between them is fully recognised.

Note IV.

Description of the Figures. Plate VI.

No. 1. is the colour-diagram already referred to, representing, on Newton's principle, the relations of different coloured papers to the three standard colours—vermilion, emerald-green, and ultramarine. The initials denoting the colours are explained in the list at page 276, and the numbers belonging to them are their coefficients of intensity, the use of which has been explained. The initials H.R., H.B., and H.G., represent the red, blue and green papers of Mr Hay, and serve to connect this diagram with No. (2), which takes these colours for its standards.

No. 2. represents the relations of Mr Hay's red, blue, green, white, and yellow papers, as determined by a large number of experiments at Cambridge.—(See Note II.). The use of the point D, in calculating the results of colour-blindness, is explained in the Paper.

Fig. 3. represents a disc of the larger size, with its slit.

Fig. 4. shows the mode of combining two discs of the smaller size.

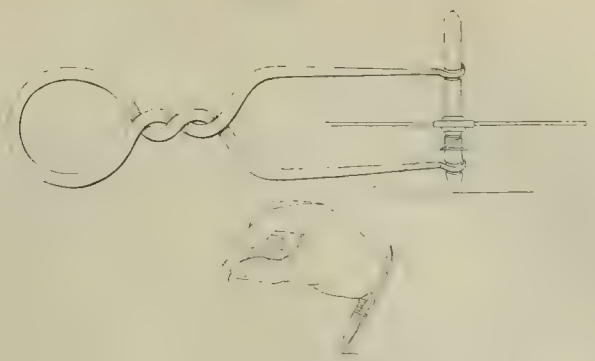
Fig. 5. shows the combination of discs, as placed on the top, in the first experiment described in the Paper.

Fig. 6. represents the method of spinning the top, when speed is required.

The last four figures are half the actual size.

Colour-tops of the kind used in these experiments, with paper discs of the colours whose relations are represented in No. 1, are to be had of Mr J. M. Bryson, Optician, Edinburgh.

FIG. 6



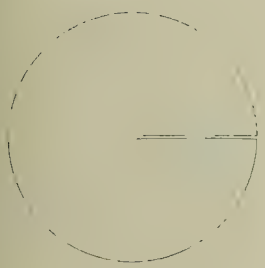


FIG. 4

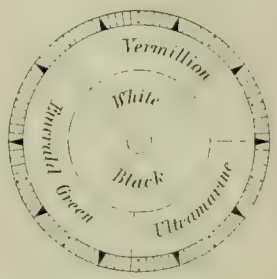
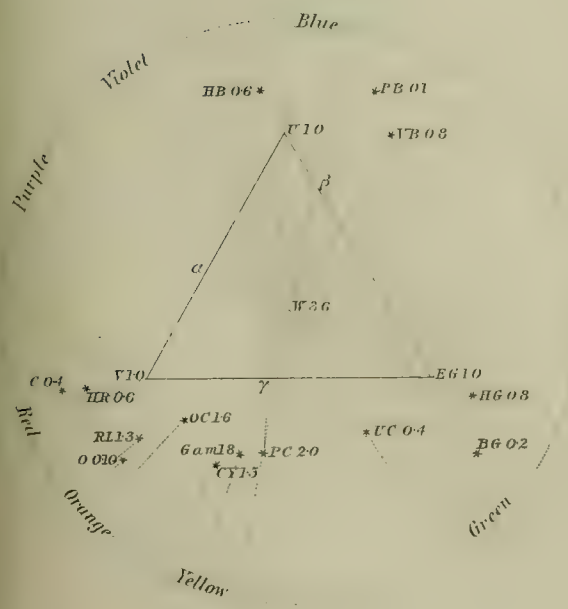
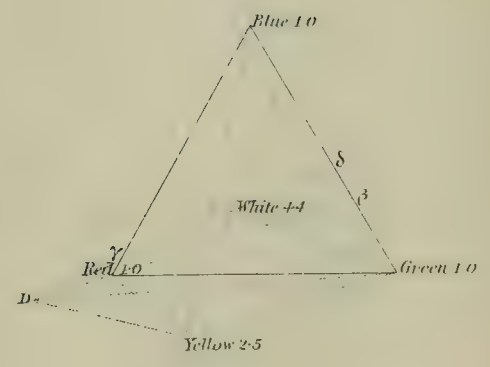


FIG. 3

FIG. 5



(N° 1)



(N° 2)

XIX.—*Researches on the Amides of the Fatty Acids.* By Thomas H. Rowney, Ph. D., F.C.S., Assistant in the College Laboratory, Glasgow.

(Read 2d April 1855.)

The following paper contains the details of some experiments upon the action of ammonia on the oils and fats, of which a preliminary notice was published in the Quarterly Journal of the Chemical Society of London.* The production of a soapy emulsion by the action of ammonia on these substances has long been familiar to chemists, but comparatively few accurate experiments have been made upon the compounds formed. Boullay† long since examined the crystalline substance obtained from olive-oil, which he called Margaramide, and mentioned that similar compounds were obtained with the other oils, although he did not examine them. More lately,‡ M. Bouis produced ricinolamide from castor-oil, and showed that, by fusion with potash, it yielded caprylic alcohol and sebacic acid; and still more recently,§ he has obtained another fatty amide, which he calls Isocetamide, by the action of ammonia on the fat of the purging nut of the West Indies.

From these experiments, it is obvious that the Glycerides of the fatty acids suffer the same decomposition when acted upon by ammonia, as their ethyl and methyl compounds undergo when exposed to the action of this reagent, amides being formed, and glycerine, methylic and ethylic alcohols being set free.

As the present communication contains a description of a considerable number of compounds obtained from different oils and fats; and as the method followed to obtain and purify them was nearly the same in all cases, I shall here describe it once for all, merely adding, under each oil, any particular remarks it may be necessary to make.

The compounds were obtained by mixing one part by measure of oil, two parts of alcohol, and four parts of strong solution of ammonia, in a stoppered bottle, capable of holding twice the quantity, and placed in a moderately warm situation, the stopper being tied with string, in order to prevent its being blown out. The materials require occasional shaking, as, after standing some time, they separate into two layers, the oil depositing, and the alcoholic and ammoniacal fluid rising to the surface. After a certain time, varying with the nature of the oil employed, it becomes covered with a whitish solid matter, which gradually

* Quarterly Journal, Chemical Society London, vol. vii., p. 200; 1855.
† Comptes Rendus, December 26, 1843, p. 1346.
‡ Ibid., August 11, 1851, p. 141.
§ Ibid., November 6, 1854, p. 923.

increases in quantity. The oil at the same time becomes thicker, diminishes in bulk, and finally both oil and ammoniacal liquor become nearly solid.

To purify the compounds, the pasty mass was collected upon a cloth filter, washed with water, and then squeezed, to free it as much as possible from the ammoniacal mother liquor. The squeezed mass was then dissolved in warm alcohol, and allowed to crystallize, again filtered through cloth, and washed, first with dilute alcohol (made with equal parts of alcohol and water), then with water, and the residue again expressed; and this was repeated until it was obtained free from a resinous matter which adheres obstinately to it. When pure, these amides are perfectly white, and undergo no change by exposure to the air; but if any of the resinous matter adheres to them, they speedily change colour, becoming yellow and resinous. The alcoholic mother liquors from which they have been crystallized retain a considerable quantity of the substances dissolved in them, which may be separated by the addition of water.

The original ammoniacal fluids, when evaporated on a water-bath, yield a considerable quantity of a dark oily or resinous matter, mixed with some of the amide.

The quantity of the crystalline compound obtained from each oil varied very much, some yielding a considerable, and others a very small quantity. The time required for the completion of the action also differs; with some the change is readily and rapidly effected, but with others, and particularly with the drying oils, long-continued digestion is required. The latter oils yield a very small proportion of the crystalline compound, but a considerable quantity of resinous matter is obtained by evaporating the ammoniacal mother liquors.

The quantities of substance employed for analysis were dried *in vacuo* over sulphuric acid, and the combustions were made with chromate of lead. The nitrogen determinations were principally made by Mr Mitchell's modification of Peligot's method, viz., caustic soda, to neutralize the excess of sulphuric acid, infusion of logwood being used as the colouring matter.

The fusing point was taken by placing a small quantity of the substance in a thin glass tube suspended in water contained in a metal vessel, which was heated by a gas flame, and the temperature ascertained by immersing a thermometer in the water. When the fusing point was found, the gas flame was removed, the water allowed to cool, and the temperature at which solidification took place was also observed. In the case of the amide which fused at 103° C., common salt was added, in small quantities at a time, so as to raise the temperature gradually until the fusing point was obtained.

The following are the oils that I have already examined, and several others are in course of examination.

Almond oil, linseed oil, poppy oil, cod-liver oil, seal oil, and croton oil; also almond oil and castor oil, after solidification by nitrous acid.

Almond Oil.

Almond oil is readily acted upon by ammonia, and yields a large proportion of the crystalline compound. This amide is very soluble in warm alcohol, and is deposited from the solution in mammilated groups of crystals. It is insoluble in water, and is separated from an alcoholic solution by dilution with water. It is not decomposed by boiling with a solution of potassa, but fused potassa decomposes it, with the evolution of ammonia.

It commences to fuse at 79° C., and is completely fused at 81° C.; when allowed to cool after fusion it becomes solid at 78° C., but remains semitransparent.

The following numbers were obtained by analysis:—

I. ·2612 grammes of substance gave
·7303 ... of carbonic acid, and
·2905 ... of water.

II. ·2612 grammes of substance gave
·7340 ... of carbonic acid, and
·2877 ... of water.

III. ·2455 grammes of substance gave
·6910 ... of carbonic acid.

IV. ·2493 grammes of substance gave
·6970 ... of carbonic acid, and
·2830 ... of water.

V. ·2740 grammes of substance, burnt with soda lime, gave
·1980 ... of ammonio-chloride of platinum.

VI. ·3090 grammes of substance, burnt with soda lime, gave
·2300 ... of ammonio-chloride of platinum.

	I.	II.	III.	IV.	V.	VI.
Carbon,	76·25	76.64	76.76	76·25	...	...
Hydrogen,	12·35	12·19	...	12·61	...	...
Nitrogen,	...	...	...	...	4·52	4·67

	Theory.		Mean.
C_{36}	216	76·86	76·47
H_{35}	35	12·45	12·38
N	14	4·98	4·59
O_2	16	5·71	6·56
	281	100·00	100·00

The ammoniacal mother liquors, when concentrated, did not yield much resinous matter; but upon the addition of hydrochloric acid to the residue, an oil separated, which was collected upon a moist filter, and washed with water. It was then dissolved in ammonia, and chloride of barium was added; the precipitate obtained was filtered, and washed with water. I endeavoured to crystallize this salt from alcohol, but as it fuses into a resinous mass under the alcohol, and adheres to the sides of the vessel, it could not be satisfactorily accomplished. This property corresponds to that of oleate of baryta, and the analyses of it are sufficient to show that it was in fact this salt, although in an impure state.

The following are the numbers obtained:—

I. { ·2550 grammes of substance gave
·0865 ... of sulphate of baryta.

II. { ·2227 grammes of substance gave
·0765 ... of sulphate of baryta.

III. { ·2472 grammes of substance gave
·5485 ... of carbonic acid, and
·2113 ... of water.

	I.	II.	III.	Mean.			
Carbon,	...	...	60·51	60·51	C_{36}	216	61·78
Hydrogen,	...	...	9·49	9·49	H_{33}	33	9·44
Ba O .	22·28	22·56	...	22·42	Ba O	76·6	21·91
Oxygen, .	...	...	...	7·58	O_3	24	6·87
				100·00		349·6	100·00

Elaidine.

The solid fat or elaidine, obtained by passing nitrous acid into almond oil, is readily acted upon by ammonia, and yields a large quantity of a crystalline amide. Upon the addition of this reagent a deep red colour is produced; and when the action is completed, the whole is transformed into a pasty mass, which is collected on a filter, washed with water, and expressed, by which means most of the colouring matter is removed, the mass is purified by several crystallizations from alcohol; and the amide is then obtained in brilliant groups of perfectly colourless crystals, which are very soluble in alcohol but insoluble in water. Elaidamide is decomposed by fusion with potassa, ammonia being evolved, but a solution of potassa is entirely without action upon it. In this property it resembles oleamide; indeed, all the fatty amides which I have examined agree in this respect.

It begins to fuse at 92° C., and is completely fluid at 94° C.; upon cooling it solidifies at 91° C. and becomes opaque.

The analysis gave the following results:—

I. { ·2035 grammes of substance gave
·5725 ... of carbonic acid, and
·2323 ... of water.

II. { ·2100 ... of substance gave
·5865 ... of carbonic acid, and
·2405 ... of water.

III. { ·4790 ... of substance gave
·0234 ... of nitrogen.

	I.	II.	III.
Carbon, . . .	76·72	76.16	...
Hydrogen, . .	12·68	12.72	...
Nitrogen, . .	...	...	4·88

		Theory.		Mean.
C_{36}	.	216	76·86	76·44
H_{35}	.	35	12·45	12·70
N	.	14	4·98	4·88
O_2	.	16	5·71	5·98
		281	100·00	100·00

Castor Oil.

I had commenced the examination of the action of ammonia upon castor oil previous to the publication of M. Bouis' paper upon the same subject, but as he has described ricinolamide, and some of the products of its decomposition, I did not continue my own experiments in that direction, though I made a partial examination of the mother liquor filtered from the amide.

When this filtrate is concentrated by evaporation, the addition of hydrochloric acid to it causes the separation of an oil which was collected upon a moistened filter, washed with water, and then dissolved in ammonia. The addition of chloride of barium to this solution gave a precipitate, which was purified by repeated crystallizations from alcohol. It fuses under alcohol, and adheres to the bottom of the vessel, but is more readily soluble in alcohol than oleate of baryta. By analysis it was found to be ricinolate of baryta.

I. { ·3120 grammes of substance gave
·0995 ... of sulphate of baryta.

II. { ·2444 grammes of substance gave
·0785 ... of sulphate of baryta.

III. { ·3333 grammes of substance gave
·1070 ... of sulphate of baryta.

IV. { ·3108 grammes of substance gave
·6675 ... of carbonic acid, and
·2533 ... of water.

	I.	II.	III.	IV.
Carbon, . .	...	...	...	58·57
Hydrogen, . .	...	...	...	9·05
Baryta, . .	20·95	21·10	21·09	...

		Theory.		Mean.
C_{36}	. .	216	59·08	58·57
H_{33}	. .	33	9·02	9·05
O_5	. .	40	10·95	11·33
BaO	. .	76·6	20·95	21·05
		365·6	100·00	100·00

The cause of the presence of ricinoleic acid in the mother liquor is explained by a remark made by M. Bouis in his paper, viz., that ricinolamide is decomposed by acids even in the cold. A small quantity of this amide must have been

present in the mother liquor, and been decomposed by the addition of the hydrochloric acid. The presence of oleic acid in the mother liquor from oleamide may be accounted for in a similar manner.

Palmine.

Palmine, obtained from castor oil by nitrous acid, when submitted to the action of ammonia, behaves in a similar manner to elaidine. A large quantity of amide is obtained, which is easily purified by crystallization from alcohol; and, when pure, it closely resembles elaidamide in appearance and properties.

It commences to fuse at 91° C., is completely fused at 93° C., and solidifies at 89° C.

By analysis the following results were obtained:—

I. ·3295 grammes of substance gave
·8760 ... of carbonic acid, and
·3840 ... of water.

II. ·2100 grammes of substance gave
·5642 ... of carbonic acid, and
·2277 ... of water.

III. ·2115 grammes of substance gave
·5630 ... of carbonic acid, and
·2280 ... of water.

IV. ·4709 grammes of substance gave
·0226 ... of nitrogen.

	I.	II.	III.	IV.
Carbon, . .	72·50	73·27	72·59	...
Hydrogen, . .	11·73	12·04	11·97	...
Nitrogen, . .	...	...	...	4·79

	Theory.		Mean.
C_{36}	216	72·72	72·79
H_{35}	35	11·78	11·91
N	14	4·71	4·79
O_4	32	10·79	10·41
	297	100·00	100·00

Linseed Oil.

Linseed oil requires long digestion with ammonia before it is acted upon, and it yields only a small quantity of amide, the greater portion of the oil being converted into a resinous matter, from which it is exceedingly difficult to purify the amide. It is insoluble in water, but very soluble in warm alcohol; and is deposited from this solution on cooling in mammillated groups of crystals, which, when dry, form a light, bulky, colourless, crystalline substance. The crystals soon acquire colour if they have not been thoroughly separated from the resin-

ous matter. The fusing point was found to be 100° C., and it became solid again when cooled to 97° C.

The following results were obtained by analysis:—

I. ·2400 grammes of substance gave
·6628 ... of carbonic acid, and
·2815 ... of water.

II. ·2200 grammes of substance gave
·6065 ... of carbonic acid, and
·2577 ... of water.

III. ·2128 grammes of substance gave
·1698 ... of ammonio-chloride of platinum.

IV. ·2423 grammes of substance gave
·1948 ... of ammonio-chloride of platinum.

	I.	II.	III.	IV.
Carbon, . .	75·31	75·18	...	...
Hydrogen, . .	13·03	13·01	...	...
Nitrogen, . .	...	...	5·01	50·4

	Theory.		Mean.
C_{34}	204	75·83	75·25
H_{35}	35	13·01	13·02
N	14	5·20	5·03
O_2	16	5·96	6·70
	269	100·00	100·00

These results correspond with the formula of margaramide, with which the properties of the substance also agree.

Poppy Oil.

Poppy oil is more readily acted upon than linseed oil, and yields a considerable portion of amide, but also much resinous matter. The amide obtained is more easily purified than that from linseed oil, but still repeated crystallizations from alcohol are necessary in order to free it from resin.

It is very soluble in alcohol, and crystallizes in mammillated groups. Its fusing point was found to be 103° C.

The analysis of this compound shows that it also was margaramide:—

I. ·1918 grammes of substance gave
·5300 ... of carbonic acid, and
·2248 ... of water.

II. ·2100 grammes of substance gave
·5805 ... of carbonic acid, and
·2445 ... of water.

III. ·3215 grammes of substance gave
·0168 ... of nitrogen.

	I.	II.	III.	Mean.
Carbon, . .	75·36	75·38	...	75·37
Hydrogen, .	13·01	12·93	...	12·97
Nitrogen, .	...	...	5·24	5·24

Croton Oil.

Croton oil yields a small quantity of amide, and the mother liquor when concentrated by evaporation contains a dark oily substance. The amide requires several crystallizations from alcohol to render it pure. It crystallizes in mammillated groups, and when dry is light and bulky. Its fusing point is 100° C.

The analysis of this substance gave the following results:—

I. ·2258 grammes of substance gave
·6235 ... of carbonic acid, and
·2598 ... of water.

II. ·2153 grammes of substance gave
·5962 ... of carbonic acid, and
·2528 ... of water.

III. ·2900 grammes of substance gave
·0142 ... of nitrogen.

	I.	II.	III.	Mean.
Carbon, . .	75·30	75·52	...	75·41
Hydrogen, . .	12·78	13·04	...	12·91
Nitrogen, . .	...	...	4·83	4·83

The nitrogen in this analysis is rather too low, and unfortunately I had no more substance with which to make another determination; but, however, the results altogether are sufficient to show that the substance was margaramide.

Seal Oil.

Seal oil is readily acted upon by ammonia, and yields a considerable quantity of amide; the mother liquor contains a large amount of resinous and oily matter, which separates by evaporation. The amide is readily purified by crystallization from alcohol, in which it is very soluble. When pure and dry, it forms a rather dense and crystalline powder, fusing at 82° C., and is transparent when cold.

By analysis the following numbers were obtained, which correspond with those for oleamide, though the hydrogen is somewhat too high.

I. ·2125 grammes of substance gave
·5980 ... of carbonic acid, and
·2475 ... of water.

II. ·2133 grammes of substance gave
·5995 ... of carbonic acid, and
·2475 ... of water.

III. ·3145 grammes of substance gave
·0158 ... of nitrogen.

IV. ·3255 grammes of substance gave
·0158 ... of nitrogen.

	I.	II.	III.	IV.	Mean.
Carbon, .	76·74	76·65	...	...	76·70
Hydrogen, .	12·94	12·89	...	...	12·92
Nitrogen, .	...	...	5·02	4·85	4·94

Cod-Liver Oil.

Cod-liver oil requires to be digested for a considerable time with ammonia before it is acted upon. It does not yield very much amide, but a considerable quantity of an oily and resinous matter is obtained from the mother liquor. It is very soluble in alcohol, and when dry presents similar appearances to the amides previously described; but its analysis gives results which at present I cannot explain, and on that account I have not given it either a name or formula. It fuses at 93° C., and becomes solid and transparent at 91° C.

The following are the numbers obtained:—

I. { ·2150 grammes of substance gave
·5960 ... of carbonic acid, and
·2534 ... of water.

II. { ·2114 grammes of substance gave
·5875 ... of carbonic acid, and
·2465 ... of water.

III. { ·4740 grammes of substance gave
·0205 ... of nitrogen.

IV. { ·4360 grammes of substance gave
·0193 ... of nitrogen.

V. { ·3305 grammes of substance gave
·0142 ... of nitrogen.

	I.	II.	III.	IV.	V.	Mean.
Carbon, .	75·60	75·79	...	...	...	76·70
Hydrogen, .	13·03	12·95	...	...	...	12·99
Nitrogen, .	...	...	4·32	4·42	4·29	4·34

On comparing these results with those obtained from the other amides, it will be seen that the carbon and hydrogen would correspond with the formula for margaramide, but the nitrogen is nearly one per cent. too low; the fusing point is also much lower than that of this amide. It likewise remains transparent when cold, which is a property belonging to oleamide, whilst the margaramide becomes opaque and somewhat crystalline when cold. It is my intention to make another examination of this oil, in order to ascertain the cause of these unsatisfactory results.

As it is my intention to continue this investigation, and also to examine the products of decomposition of these amides, I shall at present confine myself to a few remarks.

From the results of the analyses it appears that linseed, poppy, and croton oils yield margaramide, whilst oleamide is obtained from almond and seal oils; and, according to M. Bouis' experiments, ricinolamide is obtained from castor

oil; also from almond and castor oils, after solidification by nitrous acid, we obtain elaidamide and palmamide, these amides being isomeric with oleamide and ricinolamide.

The only other point to which I shall refer is the temperature at which these amides fuse. M. Boullay states that the fusing point of margaramide is 60° C., but, according to my own experiments, I have found it to be as high as 103° C., though in the cases of the amides from linseed and croton oils, it was as low as 100° C., but this arose from these substances not being perfectly pure; and it is probably a similar cause that occasions the error of M. Boullay. I am also inclined to think that the fusing points of ricinolamide, 66° C., and isocetamide, 67° C., are too low. Isocetamide only differs by $2C_2H_2$ from margaramide, the fusing point of which is 103°; and ricinolamide is closely related to oleamide, which has a fusing point of 82° C., and the isomeric amides, palmamide and elaidamide, both fuse at about 94° C.

In conclusion, I have to express my thanks to Dr Anderson for the use of his laboratory, and also for many suggestions during the progress of this investigation.

XX.—*On the Volatile Bases produced by Destructive Distillation of Cinchonine.* By C. Greville Williams, Assistant to Dr Anderson, University of Glasgow.

(Read 16th April 1855.)

The history of the organic bases has now become as ardent an object of study among chemists as that of the as yet far more numerous group of acids; and it is generally admitted that the information obtained has been no less important and interesting. Of all organic alkaloids, those of opium and the cinchona barks doubtless occupy the first rank, whether we regard their value as remedial agents, or the remarkable facts which have been ascertained connected with their atomic relations, and the influence exerted by the latter, upon the theory of the science.

It has long been known, that many of these fixed oxidized alkaloids may, by destructive distillation and analogous processes, be made to yield an entirely different, and no less interesting class, distinguished from the first by their volatility, and the absence of oxygen.

None of the bases produced by such decompositions have attracted more attention than chinoline; not that, like aniline, it has yielded any compounds which, from their perfect analogy with those of ammonia, the beauty of their salts, or any peculiarities in their coloured reactions, are interesting *per se;* on the contrary, the highness of its atomic weight and boiling point, the small tendency of its salts to form well-defined crystals, and the absence of any specialties in its behaviour with reagents, make it entirely dependent on its stöchiometrical relations, and on its supposed intimate connection with quinine, cinchonine, and strychnine, for the interest it possesses. While chemists believed that those alkaloids, minus a certain amount of hydrogen and carbonic acid, yielded chinoline, a very simple relation appeared to exist between them, a simplicity that gave interest to the subject; but when it was announced that the leukoline of coal tar was identical with chinoline; and still more, that the product of the action of iodide of methyl on leukol, when converted into a hydrated oxide was isomeric with quinine, if not the same substance, many were found who believed that the artificial production of the chinchona alkaloids would soon become a common process. But the evidence of the identity of leukol with chinoline was contradictory; Hofmann first asserted, in his Researches upon the Bases of Coal Naphtha, that their behaviour with chromic acid demonstrated their dissimilarity, while subsequently,* Liebig announced, upon the authority of

* Chemical Gazette, vol. iii., p. 251, 1845. Proceedings of Chem. Soc., April 7, 1845.

experiments made by HOFMANN, that they had been ascertained, by crucial experiments upon perfectly pure substances, to be the same. In the meantime, chinoline is found to be produced from other bodies, among which may be mentioned thialdine and trigenic acid.

But if we examine the published analyses of chinoline, great discrepancies appear, so much so, that GERHARDT places two formulæ by the side of the analytical results, namely, C_{18} H_7 N and C_{20} H_9 N. On looking at the experimental as compared with the theoretical results, it appears that the analyses, if we except HOFMANN'S, which were made upon the base from coal naphtha, agree with neither view. BROMEIS, in a paper on chinoline, published about ten years since,* gave a formula which is not admissible, and his numbers, recalculated according to the new atomic weight of carbon, do not agree with either of the formulæ given by GERHARDT, the carbon being two per cent. too low. The analysis of the bases themselves in the present case, is by no means a satisfactory method of establishing their constitution, for when entirely freed from other substances, they are extremely incombustible; and when it is considered that the two formulæ only cause a difference of ·2 of a per cent. of carbon, it will be seen that the platinum salts afford a far better means of distinguishing them, the addition of C_2 H_2 causing a rise of two per cent. in the carbon. But even this method could not afford a result, where a mixture of substances was present, unless the salt had been fractionally crystallized, which does not appear to have been done by either of the chemists who have worked on chinoline; BROMEIS obtaining far too much carbon for the 18, and as much too little for the 20 C base; the same remark applying, though less strongly, to the results of GERHARDT, whose numbers approach, with regard to the carbon, nearer to the formula C_{18} H_7 N, than C_{20} H_9 N, although he obtained greatly too much hydrogen for either.

Before I had seen the discordant analyses in the *Traité de Chimie Organique* of the last-named chemist, I had suspected, upon theoretical grounds, that chinoline was not a homogeneous substance. In the first place, it appeared unlikely that in an operation with so powerful a reagent as caustic potash, acting at an elevated temperature upon so complex an organic molecule as the base alluded to, that only one substance, and that of so high an atomic weight as chinoline, would be found. I had ascertained, by a great number of experiments on nitrogenized substances, both animal and vegetable, that in no case could they be distilled, either alone or with alkalies, without formation of the pyrrol of RUNGE, and on trying the same experiment with cinchonine, a similar result presented itself; and this alone was a strong evidence of the truth of the supposition alluded to. But the chief reason which led me to doubt the homogeneity of chinoline was, that I was unable to obtain it with a constant boiling point. I will not lay stress upon an

* LIEBIG'S Annalen, Bd. 52, p. 130.

idea which often forced itself upon me, namely, that there might be some basic product besides pyrrol, characteristic of the destructive distillation of nitrogenized organic bodies, because I had no decisive proof; but I may mention that there is a most intimate connection between the volatile bases best known, produced at very high temperatures. For with all the substances as yet examined, such as bones, shale, &c., the complete series of bases homologous with pyridine has been found, and though, in the case of coal naphtha, only picoline has as yet been detected, I believe I shall shortly be able to prove, that other members of that series exist in it. It will be seen that, if these views are correct, indigo and piperine ought to yield more than aniline in the one case, and piperidine in the other, which, in the present state of our knowledge, does not appear to be the case; while, on the other hand, if we study the experimental results yet obtained from coniine, it will scarcely be too much to conclude that it is a mixture. In order to give a decisive answer to the question raised by these facts, it becomes necessary to examine, somewhat minutely, several of the bases said to be the sole product of the destructive distillation of certain alkaloids, and other nitrogenous bodies; and the following may be regarded in the light of a small contribution to the subject.

It being evident that a considerable amount of material would be required, I subjected 100 ounces of cinchonine to destructive distillation with potash, by small portions at a time, in an iron alembic, the products being collected in a well cooled recipient, but notwithstanding the large scale on which the experiments were carried on, some difficulty was found in procuring enough of certain portions for examination.

There were several phenomena observed during the preparation of the crude chinoline, which are not described in the works to which I have had access; but as these are not immediately connected with the subject under consideration, they need not be further alluded to, except to mention that whatever precautions were taken in the distillation, pyrrol was, nevertheless, constantly found, and adhered to the crude chinoline with such tenacity, that it required two days' boiling of the acid solution to effect its complete removal.

The base was separated from the simultaneously-produced water, by means of caustic potash, which was added in sufficient quantity to prevent any remaining dissolved. After separation, by means of a tap funnel, from the dense alkaline solution, it was again digested with sticks of the potash, until no more water was removed. The general process for separating these volatile bases from non-basic and other impurities, has been so often described, that it becomes unnecessary to dwell upon it further.

On distilling perfectly dry chinoline, with a thermometer in the tubulature of the retort, ebullition was found to begin at about 300° F. (149°* C.), although no

* I have inserted the centigrade degrees (in round numbers), for convenience of those accustomed to that scale.

distillate could be procured for nearly fifty degrees above that point, until after several rectifications, when it was found that fractions could be obtained at every ten degrees from about 300° to 500° F. (183° to 260° C.) It was therefore determined to submit the whole of the chinoline in my possession to a systematic fractionation; and by the use of very small retorts, and tolerably perfect condensation, I was enabled to effect ten fractionations even of the smaller portions; and with the distillates of the higher boiling points, which were larger in quantity than the others, twelve, and in some cases even thirteen were effected. Altogether this procedure involved, with those afterwards found necessary, at least 200 distillations. By this means, fractions were obtained as low as between 310° and 320° F. (154° to 160° C.), and as high as 520° F. (271° C.). When it is considered that the boiling point of chinoline is 460° F. (238° C.), it will be seen that it was not going too far to conclude that evidence was obtained of the correctness of the suspicion previously mentioned; and it is submitted, that the following experiments prove that cinchonine by distillation with potash yields at least seven bases instead of one, as has been generally believed. It might be imagined that they would all be found to consist of homologues of chinoline, but it will be shown that this is not the case, and that two distinct series are present, one homologous with chinoline, and the other isomeric with the aniline series, and identical in composition with the group found by Dr Anderson in bone oil,* and afterwards by myself in the naphtha, from the bituminous shale of Dorsetshire.†

Before proceeding to the details of the experiments, it is proper to mention, that although the researches were commenced in London, and afterwards partially carried on in my own laboratory in Glasgow, that on becoming assistant to Dr Anderson, he not only permitted me to make comparative experiments with his bases, but gave me every possible opportunity of pursuing the investigation. In my first experiments, and before I had any considerable quantity of chinoline at my disposal, I endeavoured to obtain some insight into the nature of the fluid by fractionally crystallizing the platinum salts according to the method described in a paper on the presence of pyridine in the basic portion of the naphtha from the Dorsetshire shale.‡

The results, which are given below, were too remarkable not to be carried out more in detail; but the large quantity of platinum required to effect a perfect separation, prevented me from availing myself of this method of working, and I was forced to fall back upon the tedious and wasteful process of fractional distillation.

To obtain the platinum salts, a certain quantity of hydrochlorate of chinoline, freed from pyrrol, by boiling its acid solution for a long time, was evaporated on the water-bath, as long as water came off, but it could not be obtained in a per-

* Trans. Royal Soc. Edin., vol. xvi., part iv. † Quart. Jour. Chem. Soc., Lond., July 1854.
‡ Phil. Mag., September 1854.

fectly dry state. The mass dissolved immediately in strong alcohol, showing the absence of ammonia. To the solution, a slight excess of bichloride of platinum was added, when a voluminous precipitate (a) was obtained; the mother liquor of a, on evaporation, gave a crop of crystals b, the fluid from which gave another crop c.

The precipitate a was the first examined; it was dissolved in hot water, and on cooling deposited a crop of crystals, which was evidently a mixture, for it consisted of large orange crystals a^3, and exceedingly small needles a^4; a^3 was capable of being perfectly freed from a^4 by sifting through muslin; the latter was not, however, absolutely free from small fragments of a^3, and was therefore further examined, as will be presently seen.

5·295 grains of a^3 dried at 212° gave
1·625 ... platinum,

or 30·69 per cent., a number intermediate between lutidine and collidine.

The mother liquid of the crops a^3 and a^4 gave a crystalline deposit a^2, which had the same crystalline form and appearance as a^3, but a little more brilliant in colour.

4·955 grains of a^2 dried at 212° gave
1·520 ... platinum,

or, 30·67 per cent. It will be seen, therefore, that a^2 and a^3 agree not only in form but also in composition.

The mother liquid of a^2 on evaporation over sulphuric acid, gave another crop a^{2b}, which was, therefore, the third crop from a.

2·245 grains of a^{2b} dried at 212° gave
·740 ... platinum,

or 32·96 per cent., which is almost exactly the theoretical percentage in picoline for

Experiment.	Theory. (Picoline.)
32·96	32·94.

It has been said that a^4 contained a little of a^3 intermixed; it was therefore treated with hot water, which separated it into two portions, one less soluble a^5, and a solution which on cooling deposited a crop of crystals a^6. The portion of a^3 which was present was very minute, and remaining in solution did not effect the results given by a^5 and a^6.

3·205 grains of a^5 platinum salt gave
1·010 .... platinum,

or 31·51 per cent.

Experiment.	Theory. (Lutidine.)
31·51	31·51

3·970 grains of a^6 platinum salt gave
1·150 ... ... platinum,

or 28·96 per cent., a number intermediate between chinoline and lepidine, presently

to be mentioned. The mother liquid, on treatment in the manner described in my former paper, namely, exposure to a desiccating surface gave a crop a^7,

2·780 grains of platinum salt a^7 gave
·880 ... platinum,

or 31·65 per cent.

Experiment.	Theory. (Lutidine.)
31·65	31·50

The mother liquor of a gave b on evaporation,

5·327 grains of platinum salt b gave
1·635 ... platinum,

or 30·70 per cent., being almost exactly the same result as a^2 and a^3.

The mother liquor of b gave a crop of white needles, the nature of which I have not yet been able perfectly to comprehend. I have, however, observed them to exist in the very last crops of many platinum salts of different kinds, more especially when evaporated by the aid of heat. They are soluble in hot water, and at a red heat leave metallic platinum. They do not deflagrate when thrown into melted nitre. Solution of potash does not decompose them, the aqueous solution is not precipitated by alcohol; the solution in boiling water precipitates nitrate of silver.

2·182 grains gave on ignition
1·672 ... of platinum,

or 73·50 per cent.

When it is considered that even protochloride of platinum requires 76·6, it will be seen that this experiment does not throw much light on their nature.* The quantity I have as yet been able to obtain has been too small to allow of a further development of their history.

The mother liquid of these crystals yielded a resin not further examined, which was the last product of the mother liquid of a.

These results,—although the numbers obtained are as close as could be expected to the theoretical values, and prove that the bases to be described more fully further on, were not *produced* during the distillations,—were, of course, quite insufficient to settle the points sought to be determined: it became necessary therefore to examine minutely each of the fractions obtained by the distillations previously alluded to. It may be mentioned here, that in order to remove any objections that might be urged as to the bases being produced from the decomposition of nitrogenous impurities existing in the cinchonine employed, an analysis was made with the following results,—

I. 7·25 grains of cinchonine dried at 212° gave
20·56 ... carbonic acid and
5·28 ... water.

II. 8·03 ... cinchonine dried at 212° gave
5·64 ... water.

* It was probably a salt analogous to the bases of Reiset or Gros, mixed with some impurity.

	I.	II.	Calculation.		
Carbon,	77·34	...	77·92	C_{40}	240
Hydrogen,	8·09	7·80	7·79	H_{24}	24
Nitrogen,	...	...	9·09	N_2	28
Oxygen,	...	...	5·13	O_2	16
					308

The fact of the two series of bases present not being homologous with each other, rendered a considerable amount of labour necessary before they could be purified sufficiently for analysis, the presence, in very small quantity, of a base of the one series altering to such an extent the composition of the other, that no confidence could be placed in an analysis, unless extreme care was taken in the purifications, and the small amount of material at my command naturally added considerably to the difficulties with which I had to contend.

Lutidine.—It soon became evident that the third base discovered by Dr ANDERSON in the animal oil of Dippel, and to which he gave the name of lutidine, was that which most prominently presented itself in the first fractions. It was subsequently ascertained that pyridine and picoline were present in exceedingly small quantities, and to attempt their isolation would have been useless. The only evidence I have to show of the presence of pyridine is an isolated result, being the composition of the second crop of crystals of platinum salt obtained from the first fraction during the earlier rectifications, and before the pyridine could have escaped, as being present in such small quantity it was sure to do eventually, from the difficulty of effecting perfect condensation in an operation involving the changing of the receiver every few minutes.

2·740 grains of platinum salt, second crop, from fraction boiling below 330° F. (165° C.) (fourth distillation) gave
·948 grains of platinum,

or 34·6 per cent.

Experiment.	Theory. (Pyridine.)
34·6	34·6

The difficulty experienced in obtaining the lutidine sufficiently free from the bases above it, to enable a good result to be obtained, will appear from the analyses below, which were made upon platinum salts from fractions which had been rectified the number of times prefixed to each result.

	1st Rect.	2d Rect.	4th Rect.	
Carbon, . .	29·40	28·89	27·10	27·20
Hydrogen, . .	3·73	3·84	3·30	3·28
Platinum, . .	30·85	30·63	30·83	...

It was evident, therefore, that although the numbers were gradually becoming nearer to those required for lutidine, that, nevertheless, many more rectifications, would be necessary before any close approximation could be expected. The small quantity of fluid was therefore, with careful management, made to pass

through nine perfect fractionations, at the end of which a base was obtained, if not absolutely free from the more highly carburetted bases, at least so nearly, that, on analysis—

5·265 grains of base, boiling between 320° and 330° F. (160°–165° C.), gave
15·190 ... carbonic acid, and
4·040 ... water,

or per cent.

	Experiment.	Theory.		
Carbon, . .	78·68	78·50	C_{14}	84
Hydrogen, . .	8·52	8·41	H_9	9
Nitrogen, . .	12·80	13·09	N	14
	100·00	100·00		107

This, then, is the third occasion in which lutidine has been observed, the other two being, first, in bone oil, where it was discovered;* and, secondly, among the bases produced by destructive distillation of the bituminous shale of Dorsetshire, where I found it among others of the same series.†

The analysis of the base from those sources yielded the numbers following, where they are compared with the same base from cinchonine.

	(Dr Anderson, from Bone-Oil, *mean.*)	(Grev. Williams from Shale Naphtha, *mean.*)	(Grev. Williams from Cinchonine.)
Carbon, . .	78·45	78·68	78·68
Hydrogen, . .	8·81	8·55	8·52
Nitrogen, . .	12·54	12·77	12·80
	100·00	100·00	100·00

Before converting the fraction which gave the analysis mentioned above into platinum salt, in order to confirm the result, it was once more distilled, that portion only being received which came over between the same points; the double salt then obtained gave the following numbers:—

9·025 grains platinum salt, boiling between 320° and 330° F. (160°–165° C.), tenth rectification, gave
8·915 ... carbonic acid, and
2·730 ... water.

5·715 grains platinum salt gave
1·780 ... platinum,

corresponding to—

	Experiment.	Calculation.		
Carbon, . .	26·94	26·81	C_{14}	84
Hydrogen, . .	3·36	3·19	H_{10}	10
Nitrogen, . .	...	4·49	N	14
Chlorine, . .	...	34·00	Cl_3	106·5
Platinum, . .	31·14	31·51	Pt	98·7
	100·00	100·00		313·2

* Trans. Royal Soc. Edin., vol. xx., part ii. † Quart. Journ. Chem. Soc. Lond., 1854.

The following analyses of the platinum salts of lutidine relate to the mean results of Dr ANDERSON from Dippel's oil, and to a salt obtained by me from shale naphtha; the latter contained, however, a little picoline, which lowered the carbon.

	(Dr ANDERSON from Dippel.)	(GREV. WILLIAMS from Shale Naphtha.)
Carbon, . . .	26·35	26·14
Hydrogen, . . .	3·23	3·16
Platinum, . . .	31·50	31·76

Although the results detailed leave no doubt as to the identity of this base, it was, nevertheless, determined to place the fact beyond dispute, by obtaining a methyl compound. When the base is mixed with twice its bulk of iodide of methyl, it becomes heated, boils, and almost immediately solidifies into a mass of crystals of hydriodate of methyl-lutidine-ammonium. This substance, as obtained from the chinoline bases, is excessively soluble in water and alcohol, but insoluble, or nearly so, in ether. On evaporating the spirituous solution of the hydriodate to a syrupy consistence, it retains that state for a considerable time, if not disturbed; but immediately it is touched, long and beautiful needles shoot quite across the vessel, and finally the whole becomes a mass of crystals.

As some difficulty presented itself in purifying the crystals from a brown product which contaminates it, in common with analogous salts from almost all volatile oily bases subjected to the same treatment, it was converted into a platinum salt. To effect this the crystals were dissolved in water, the iodine precipitated by nitrate of silver, excess of hydrochloric acid was then added, and the solution filtered; the filtrate, after addition of chloride of platinum, yielded a crop of fine crystals.

4·490 grains of platinum salt of methyl-lutidine gave
1·360 ... platinum.

Experiment.	Theory.
30·29	30·16

On treating the iodide of methyl-lutidine with potash, no odour of a volatile base is evolved, showing it to agree in constitution with HOFMANN's fourth class, and serving also as corroborative evidence of the identity of it with the bone-oil alkaloid.

Collidine.—It now became desirable to ascertain if the next base of the picoline series was present, and the following experiments leave no doubt that collidine exists among the products of the distillation of cinchonine with potash.

Collidine is one of the bases discovered by Dr ANDERSON in Dippel's oil,* and found a few weeks subsequently by me in shale naphtha. At the time I examined the latter I was unacquainted with Dr ANDERSON's experiments; but when I became his assistant, abundant opportunities were afforded me of comparing

* Trans. Royal Soc. Edin., vol. xxi., part i.

the bases I had obtained from both sources with the originals discovered by him, and the result is, that no doubt remains in my mind of their identity, and I have the satisfaction of knowing that Dr ANDERSON is of the same opinion.

The quantity of collidine in the crude chinoline, from 100 ounces of cinchonine, was found to be so small that it became impossible to analyse the base itself; the platinum salt, however, was obtained nearly in a state of purity.

The boiling point of collidine is stated, in the paper before adverted to, to be 354° F. (179° C.); and on converting the fraction between 350° and 360° F. (177°–182° C.), of the tenth rectification into platinum salt, the following numbers were obtained,—

I. { 9·290 grains of platinochloride of collidine gave
9·895 ... carbonic acid and
2·960 ... water.

II. { 3·405 grains of platinochloride of collidine gave
1·020 ... platinum.

III. { 2·400 grains of platinochloride of collidine gave
·725 ... platinum.

	I. and II.	III.	Calculation.		
Carbon, . .	29·04	...	29·33	C_{16}	96
Hydrogen, . .	3·54	...	3·66	H_{12}	12
Nitrogen, . .	...	...	4·31	N	14
Chlorine, . .	...	...	32·54	Cl_3	106·5
Platinum, . .	29·97	30·2	30·16	Pt	98·7
			100·00		327·2

Collidine was found to exist also in fractions boiling at higher points; for the next fraction to that last analysed gave a salt which yielded, in a platinum determination, the annexed numbers:—

7·095 grains of platinochloride of collidine from fraction boiling between 360° and 370° F. (182° to 187° C.), tenth rectification, gave
2·145 ... platinum,

or 30·23 per cent.

Experiment.	Theory. (Collidine).
30·23	30·16

One cause of the difficulty of obtaining the Dippel's oil series from cinchonine in a state of purity, was the presence of some basic substance decomposable by nitric acid of moderate strength. It was only towards the end of the investigation that this was ascertained. If it had been known at the outset, many of the distillations, and consequently much loss of material, might in all probability have been saved.

The fraction boiling even as high as 390° F. (199° C.) contained a large proportion of collidine; but it was necessary to act upon it with rather weak nitric acid, and then reobtain the base by distillation with potash before converting it into plati-

num salt. After proceeding in this manner, a fine crop of crystals was obtained, which, on combustion with chromate of lead, gave the annexed numbers:—

9·046 grains of platinum salt from fraction boiling between 380° and 390° F. (193°–198° C.), after treatment with nitric acid (ninth rectification), gave
9·805 ... carbonic acid and
2·900 ... water.

5·110 grains of platinochloride of collidine, same as last, gave
1·525 ... platinum.

corresponding to

	Experiment.	Calculation.		
Carbon,	29·56	29·33	C_{16}	96
Hydrogen,	3·56	3·66	H_{12}	12
Nitrogen,	...	4·31	N	14
Chlorine,	...	32·54	Cl_3	106·5
Platinum,	29·84	30·16	Pt	98·7
		100·00		327·2

The numbers obtained in the analyses of the platinum salt of collidine from Dippel's oil, is in the following table compared with those given above and the theoretical ones.

	Dippel's Oil, Dr Anderson.		Grev. Williams, from Cinchonine.			
	I.	II.	I.	II.	III.	Theory.
Carbon,	28·77	29·00	29·04	29·56	...	29·33
Hydrogen,	3·57	3·63	3·54	3·56	...	3·66
Nitrogen,	...	...	...	...	...	4·31
Chlorine,	...	...	...	...	...	32·54
Platinum,	30·33	30·03	29·97	29·80	30·2	30·16
						100·00

The collidine thus obtained, treated in the usual manner with iodide of methyl, yields a finely crystallized hydriodate of the ammonium base, although the reaction is less energetic than in the case of lutidine.

Chinoline.—In examining the fractions at temperatures above those already described, it appeared that the series which then presented itself was not homologous with that of which lutidine and collidine are members. In fact, about 400° F. (204° C.) the proportion of hydrogen in the platinum salts began to lower so rapidly, that it was evident that the chinoline of Gerhardt was the next base.

In the course of the rectifications, the relative positions of the bases undergo considerable changes, for while, in the eighth rectification, the portion of fluid boiling about 420° F. contained some lepidine (the 20 carbon base, to be described further on), after four more distillations, it had gone higher up, and nearer to its correct boiling point, and then the position in the series of fractions formerly occupied by a mixture of chinoline and lepidine became held entirely by the former.

Chinoline is the chief constituent of all the fractions boiling between 420° and 470° F. (216°–243° C.), in the twelfth rectification; it is also contained in small quantity in the fractions a little below and above those points.

In a case like the present, where the chief means of separation was distillation, it was, of course, impossible to obtain the 18 and 20 carbon bases perfectly free from each other, and in bodies of so high an atomic weight, it was useless to attempt to prove their constitution by analyses of the bases themselves, as they only differ by ·2 of a per cent. in the carbon, and when freed from those of the picoline series are extremely difficult to burn.

As no doubt exists of the constitution of the double salt formed with chloride of platinum, and as the salts of the two bases differ in the amount of carbon they contain by two per cent., and, moreover, are readily obtained pure, I availed myself of them to determine the fact of the existence of the two homologous bases in the fractions.

The properties of chinoline and lepidine approach so nearly, that one description will serve for both, less distinction being observable between them than is found to occur with a difference of $C_2 H_2$ in the other volatile bases.

It is remarkable to find bases boiling at such extremely high temperatures give such well crystallized salts as those of chinoline and lepidine; even the portion boiling at 520° F. (271° C.) affords a fine platinum salt, almost insoluble in water, and without the slightest tendency to assume a resinous or oily condition, as the salts obtained from bases with such high boiling points are so liable to do.

As chinoline has so long been known, although not in a state of purity, I was satisfied, for the purposes of the present investigation, with determining the composition of its platinum salt by the following analyses:—

I.
10·325 grains of platinochloride of chinoline boiling between 410° and 420° F. (210°–215° C.), tenth rectification, gave
12·090 ... carbonic acid and
2·880 ... water.
4·280 ... platinochloride of chinoline gave
1·260 ... platinum.

II.
3·530 grains of platinochloride of chinoline gave
1·045 ... platinum.

III.
6·560 grains of platinochloride of chinoline gave
7·755 ... carbonic acid and
1·550 ... water.

IV.
9·900 grains of platinochloride of chinoline gave
11·805 ... carbonic acid and
2·300 ... water.

3·265 grains of platinochloride of chinoline boiling between 420° and 430° F. (216°–221° C.), eleventh rectification, gave
·960 ... platinum.

	I.	II.	III.	IV.	V.	Calculation.		
Carbon,	31·93	...	32·24	32·52	....	32·22	C_{18}	108
Hydrogen,	3·09	...	2·62	2·58	....	2·39	H_8	8
Nitrogen,	...	...	...	...	....	4·18	N	14
Chlorine,	...	...	...	...	...	31·77	Cl_3	106·5
Platinum,	29·44	29·60	...	...	29·40	29·44	Pt	98·7
						100·00		335·2

Lepidine.—After repeated rectifications it was found that the fraction boiling about 510° contained another base, to which I give the name of lepidine.* It was only, however, when the rectifications had been very frequently repeated, that it was obtained pure. My reason for giving a new name to the base containing twenty equivalents of carbon, and retaining that of chinoline for the other was, that C_{18} H_7 N, is almost universally received as the formula of the latter. It has been said that the positions of the bases in the fractions greatly alter as the rectifications proceed. This is nowhere more strikingly illustrated than with lepidine, which in the eighth rectification was met with as low down as 420° F. (216° C.). By fractional crystallization, without heat, a crop was obtained, which yielded the annexed numbers:—

{ 5·905 grains of platinum salt gave
{ 1·685 ... platinum.

Experiment.	Theory.
28·53	28·27

But this salt formed only a small portion of the original fraction, for upon further evaporation crystals were obtained, which the subjoined platinum determination proved to consist of platinochloride of chinoline,—

{ 6·345 grains of platinochloride of chinoline gave
{ 1·885 ... platinum.

or per cent. 29·71.

Experiment.	Theory (Chinoline.)
29·71	29·44

The real boiling point of lepidine is probably as high as 500° F. (260° C.), or even a little higher, and after four more rectifications, no sign of it could be found in the fraction at 420° F., and the nearest approach to a pure base was obtained at 510° F. (265° C.). But by so many distillations, at such an elevated temperature, it becomes slightly decomposed, a little pyrrol and carbonate of ammonia being formed; this in addition to the incombustibility of the fluid rendered its purification and analysis difficult. Fortunately, however, the same remark does not apply to its salts.

{ 3·615 grains of lepidine gave
{ 11·040 ... of carbonic acid and
{ 2·140 ... of water.

* From λεπος.

	Experiment.	Calculation.		
Carbon, . .	83·29	83·91	C_{20}	120
Hydrogen, . .	6·57	6·29	H_9	9
Nitrogen, . .	10·15	9·80	N	14
		100·00		143

The above analysis, as regards the carbon, would be useless as evidence of the existence of lepidine, because, as has been said, the 18 and 20 carbon formulæ only cause a difference of ·2 of a per cent., but the hydrogen is so much higher in lepidine, that some slight judgment may be formed from the numbers obtained.

The platinum salt of lepidine contains two per cent. more carbon, and more than one per cent. less platinum than chinoline, and the analysis of that obtained from the fraction boiling between 510° and 520° F. (265°, 271° C.), which had been rectified no less than twelve times, gave the following result,—

I. { 10·265 grains of platinochloride of lepidine gave
12·740 ... carbonic acid, and
2·720 ... water.

II. { 3·335 grains of platinochloride of lepidine gave
·940 ... platinum.

III. { 9·125 grains of platinochloride of lepidine gave
11·455 ... carbonic acid, and
2·430 ... water.

IV. { 3·330 grains of platinochloride of lepidine gave
·935 ... platinum.

	Experiment.		Calculation.		
	I. & II.	III. & IV.			
Carbon, . . .	33·85	34·23	34·36	C_{20}	120
Hydrogen, . . .	2·94	2·96	2·86	H_{10}	10
Nitrogen, . . .	...	...	4·01	N	14
Chlorine, . . .	...	...	30·50	Cl_3	106·5
Platinum, . . .	28·18	28·08	28·27	Pt	98·7
			100·00		349·2

As a further confirmation of the constitution of lepidine, the density of its vapour was ascertained with the following result,—

Temperature of balance case, ...	58° Fah.
... vapour	551° ...
Excess of weight of balloon and vapour over balloon and air, .	10·050 grains.
Capacity of balloon, ...	325·5 cub. Cent.
Barometer, ...	29·852 inches.
Residual air, ...	0·

Theoretical Density of Vapour. $C_{20}H_9N = 4$ vols.	Experiment.
4·94	5·14

Nitrate of Lepidine.—When the fraction boiling from 500° to 510° F. (260°-266° C.) is dissolved in nitric acid of moderate strength, a solution of a pale red colour is obtained, which, on evaporation, yields a deep brownish-red deliquescent

crystalline mass, which on solution in water and re-evaporation becomes lighter in colour. The nitrate in this state cannot be obtained in the form of a dry or pulverulent salt, in consequence of the presence of an impurity which renders it readily fusible at 212°. But if the salt is pressed repeatedly between folds of blotting paper, and then crystallized from alcohol, fine hard prisms are obtained, quite infusible at 212°, and without the slightest tendency to deliquesce. As a little colouring matter was still retained, rendering the salt yellow, the crystals were pulverized and washed with ether, in which they were nearly insoluble; the purified salt gave, on combustion, the numbers annexed. It should be stated, that the first analysis was made upon a mixture of two crops, the second of which was obtained by evaporating the mother liquor of the first, the second and third analyses were made upon the first crop of crystals.

I. 6·840 grains of nitrate of lepidine (mixture of two crops of crystals) gave
14·470 ... carbonic acid and
3·035 ... water.

II. 6·855 grains of nitrate of lepidine (first crop of crystals) gave
14·680 ... carbonic acid, and
3·025 ... water.

III. 6·340 grains of nitrate of lepidine (first crop of crystals) gave
13·540 ... carbonic acid, and
2·845 ... water.

	Experiment.				Calculation.		
	I.	II.	III.	Mean.			
Carbon, .	57·69	58·40	58·24	58·11	58·25	C_{20}	120
Hydrogen, .	4·93	4·90	4·98	4·93	4·86	H_{10}	10
Nitrogen, .	...	...	...	...	13·59	N_2	28
Oxygen, .	...	...	...	...	23·30	O_6	48
					100·00		206

Hydrochlorate of Lepidine.—This salt is obtainable without difficulty in small white needles. The reason that former experimenters found so much time and trouble necessary to obtain crystals, was the presence of the more volatile bases, which, strange to say, yield crystalline salts with far greater difficulty than either chinoline or lepidine. The crystals of hydrochlorate of lepidine are quite infusible at 212°. An analysis gave the following result :—

7·630 grains of hydrochlorate of lepidine, dried at 212°, gave
6·060 ... chloride of silver.

giving a percentage of 19·64 of chlorine, being exactly the quantity required for the hydrochlorate of lepidine, as will appear from the annexed comparison with the theoretical percentages.

	Experiment.	Calculation.		
Carbon, . .	...	66·85	C_{20}	120
Hydrogen, . .	...	5·57	H_{10}	10
Nitrogen, . .	...	7·80	N	14
Chlorine, . .	19·65	19·78	Cl	35·5
		100·00		179·5

Bichromate of Lepidine.—This extremely beautiful salt is easily obtained by adding an excess of a rather dilute solution of chromic acid to lepidine. For the first few seconds the chromate, on touching with a glass-rod, appears resinous, but the instant it is stirred the salt becomes gritty and crystalline. On filtering off the crystalline powder obtained in this manner, and dissolving it in hot water, the salt crystallizes out on cooling, in needles nearly an inch long, extremely brilliant, and of a rich golden yellow. The mother liquors, on evaporation, yield a fresh crop. It does not appear to be at all decomposed by moderate boiling with excess of dilute chromic acid. It decomposes at no very elevated temperature, and if suddenly heated to 212° when slightly damp, it becomes converted into a mixture of green oxide of chromium and charcoal. On one occasion, this took place with a very curious phenomenon. About four grains in powder having been placed on a water-glass, on the upper shelf of the water-bath, it was not observed for about one hour, during which time it had become converted into a mass of flattened rods, reaching to the top of the water-bath, a distance of about $1\frac{1}{4}$ inches, and had turned down again on all sides, in a manner which gave the whole much the appearance of the capital of a Corinthian column. In general, however, it may be dried at 212° with perfect safety; if retained more than an hour or two in the bath it begins to turn slightly brown, but experiences no further change during five hours' exposure. On ignition, the salt behaves like the bichromate of ammonia and the chromate of strychnine, inasmuch as it leaves pure green oxide of chromium. This fact enables the constitution of the salt to be determined with accuracy. The following experiments were made upon three different specimens.

I. { 4·105 grains of bichromate lepidine, dried at 212°, gave
1·260 ... green oxide of chromium.

II. { 6·290 grains of bichromate lepidine, dried at 212° until slightly brown, gave
1·940 ... green oxide of chromium.

III. { 6·850 grains of bichromate lepidine, dried at 212° for five hours, gave
2·120 ... green oxide of chromium.

IV. { 7·315 grains of bichromate lepidine gave
12·620 ... carbonic acid, and
2·560 ... water.

	Experiment.				Calculation.		
	I.	II.	III.	IV.			
Carbon, .	...	...	...	47·05	47·36	C_{20}	120·0
Hydrogen, .	...	...	...	3·89	3·95	H_{10}	10·0
Nitrogen, .	...	...	...	...	5·52	N	14·0
Chromium, .	21·17	21·28	21·35	...	21·07	Cr_2	53·4
Oxygen, .	...	...	...	...	22·10	O_7	56·0
					100·00		253·4

Consequently the formula, is

$$C_{20}H_9N + 2CrO_3 + HO.$$

The salt analysed agrees therefore with the bichromate of ammonia in only containing one atom of water.

Hydriodate of Amyl-Lepidine.—On treating lepidine with iodide of amyl in a pressure-tube at 212° for some hours, a finely crystallized salt is obtained, rather sparingly soluble in water; it was analysed by determining the percentage of iodine.

8·685 grains of hydriodate of amyl-lepidine gave
6·025 ... iodide of silver.

corresponding to 37·49 per cent.; hydriodate of amyl-lepidine requires the following numbers:—

	Experiment.	Calculation.		
Carbon, ...		52·79	C_{30}	180
Hydrogen, ...		5·87	H_{20}	20
Nitrogen, ...		4·10	N	14
Iodine, . . .	37·49	37.24	I	127
		100·00		341

Hydriodate of Methyl-Lepidine is a finely crystallized body, but its history, and that of the analogous salt C_2 H_2 below it, will be given in another paper, in which the leukoline of HOFMANN will be compared with pure chinoline.

The experiments detailed prove, therefore, that cinchonine, by distillation with potash, yields pyrrol, pyridine, picoline, lutidine, collidine, chinoline, and lepidine, a result which indicates a total breaking up of the cinchonine, and of this the appearance of pyrrol may be considered as a further confirmation, my experiments having shewn that that substance is, in general, characteristic of the complex decomposition of nitrogenous substances.

When feathers, wool, or hair, are distilled *per se*, sufficient pyrrol is evolved to give a reaction instantly, with deal wood moistened with hydrochloric acid, and as the experiment can be made in a test-tube, it serves very well for lecture illustration. Feathers yield a very large quantity of bases and carbonate of ammonia when distilled. The former appear, from my experiments (which, as yet, have only been on a very small scale), to contain some different from those at present known. Pyrrol possesses perhaps, as high an interest as any basic substance obtained by destructive distillation; and it is singular, that most nitrogenized bodies, when burnt with soda-lime, by WILL and VARRENTRAP'S process for determining the nitrogen, evolve pyrrol, which, passing through the acid in the bulbs, may be recognised by the reaction with deal wood and hydrochloric acid. Among the bodies tried in this manner, and found to give unequivocal reactions, may be mentioned, guanos, dried turnips, oil-cake, hay, and para grass. Whether these facts prove a loss of nitrogen, is at present doubtful, but the question will probably be solved, when Dr ANDERSON'S researches on the pyrrol series of bases from Dippel's oil are completed.

The following is a list of the substances analysed in this paper,—

Platinum salt of pyridine,	$C_{10}\,H_5\,N,\,H\,Cl,\,Pt\,Cl_2$
... ... picoline,	$C_{12}\,H_7\,N,\,H\,Cl,\,Pt\,Cl_2$
Lutidine,	$C_{14}\,H_9\,N,$
Platinum salt of lutidine,	$C_{14}\,H_9\,N,\,H\,Cl,\,Pt\,Cl_2$
... ... methyl-lutidine,	$C_{16}\,H_{11}\,N,\,H\,Cl,\,Pt\,Cl_2$
... ... collidine,	$C_{16}\,H_{11}\,N,\,H\,Cl,\,Pt\,Cl_2$
... ... chinoline,	$C_{18}\,H_7\,N,\,H\,Cl,\,Pt\,Cl_2$
Lepidine,	$C_{20}\,H_9\,N,$
Platinum salt lepidine,	$C_{20}\,H_9\,N,\,H\,Cl,\,Pt\,Cl_2$
Hydrochlorate lepidine,	$C_{20}\,H_9\,N,\,H\,Cl$
Nitrate lepidine,	$C_{20}\,H_9\,N,\,N\,O_5\,HO$
Bichromate lepidine,	$C_{20}\,H_9\,N,\,2\,Cr\,O_3,\,HO$
Hydriodate amyl-lepidine,	$C_{30}\,H_{19}\,N,\,HI$

XXI.—*On the Extent to which the received Theory of Vision requires us to regard the Eye as a Camera Obscura.* By George Wilson, M.D., F.R.S.E., Director of the Industrial Museum of Scotland.

(Read 2d April 1855.)

In the course of those researches on Colour-Blindness, which, at intervals, I have recently been engaged in prosecuting, I have encountered some phenomena connected with normal vision, which I am desirous to submit to the consideration of the Society. Those phenomena I have already in part detailed, in the account of the researches referred to,* and I shall not, accordingly, repeat the description of them here, to a greater extent than is essential to rendering intelligible the question which I wish to submit for discussion.

I venture to assume, that without adducing a lengthened series of authorities, I may take for granted, that, on the received theory of vision, the eye of man, as well as that of most of the lower animals, is regarded as essentially realizing, during the performance of its function of sight, the condition of a darkened chamber, or *camera obscura*. In more precise words: the theory in question teaches, that those rays of light, which reach the eye from the objects which they render visible, and entering at its front traverse all its transparent humours and membranes, last of all pierce the retina, and after making that impression upon it which is supposed to be the most important physical element of vision, are stopped, or absorbed by the dark pigment lining the choroid coat, and suffer extinction as visible rays. The dark surface of the choroid is thus held to abolish all the light which reaches it, so that none of the luminous rays return through the retina, or retrace their course across the chamber of the eye.

The doctrine thus taught appears, in the present state of our knowledge, to be in great part beyond dispute. It may suffice on this point to notice:—

(1.) That no other use has been assigned, or readily suggests itself, for the existence of a dark lining to the deepest membrane of the eye on which light falls, than the one referred to.

(2.) That, as theory indicates, and the experience of our artificial cameræ obscuræ teaches, the darker all their internal walls are, the more marked and sharply defined is the picture which light produces upon the screen at the back of the camera.

(3.) That, apart from the sharpness of definition secured by the contrast between the darkness of the ground, and the brightness of the picture in all ca-

* Researches on Colour-Blindness. Sutherland and Knox, Edinburgh, 1855.

meræ, there is a further necessity for a black lining to the living camera, or eye-chamber; for were that lining wanting, and the internal surface of the choroid highly reflective, the same ray of light might return many times across the chamber of the eye, producing multiple images of single objects on the retina, exhausting its sensibility, and confusing vision.

(4.) That this, moreover, is no hypothetical case, is held to be demonstrated by the experience of the Albinoes of our race, in whom the pigment of the choroid is wanting, and who see with pain and effort, unless by dim light.

The views stated above have till very recently been universally held by physiologists and natural philosophers; in proof of which it may suffice to quote the following passage from the Elements of Physiology of Professor Müller, who has written very fully on the theory of vision, and is one of the highest living authorities among physiologists on this subject.

"The interior of the eye, namely, the posterior surface of the iris and ciliary processes, and the inner surface of the choroid, immediately external to the retina itself, is coated with black pigment, which has the same effect as the black colour given to the inner surface of the walls of optical instruments. It absorbs any rays of light which may be reflected within the eye, and prevents their being thrown again upon the retina, so as to interfere with the distinctness of the images there formed. This is the use of the pigment on the posterior surface of the iris and ciliary processes. But the coating of the outer surface of the retina by the pigment of the choroid is also important in the same respect; for the retina is very transparent, and if the surface behind it were not of a dark colour, but capable of reflecting the light, the luminous rays which had already acted on the retina would be reflected back again through it, and would fall upon other parts of the same membrane, the consequence of which would be, not merely dazzling from the excessive action of light, but also indistinctness of the images. Animals in which the choroid is destitute of pigment, and human Albinoes suffer in this way; they are dazzled by daylight, and see best by twilight."*

Thus far, then, there does not appear to be room for two opinions concerning the internal darkness of the human eye being a condition of perfect sight. But recent discoveries require us to look at the theory of vision from an opposite point of view. It is now beyond question, that even in the darkest human eye, there is reflection through or across its chamber, from the surface of the retina, as well as from that of the choroid; and the observation is a very old one, that in a large number of animals, a part, and sometimes the whole of the retinal surface is covered, or replaced† by a reflector rivalling in brilliancy a sheet of polished silver.

* Müller's Elements of Physiology, vol. ii., p. 1133. 1842. Translated by Baly.

† I do not intend by the words "covered" or "replaced," to imply any opinion on the anatomical relation of the *tapetum lucidum* to the other structures of the eye. In an optical point of view, it is the substitution of a highly reflective, for a partially absorptive surface.

That the eyes of living men and women emitted light, and shone like those of the cat, had been occasionally noticed and recorded from an early time, but the phenomenon was supposed to be an exceptional, and indeed very rare one, and was either credulously magnified into a highly marvellous occurrence, or despised as of questionable accuracy, and of little real significance. In (or about) 1847, however, Mr CUMMING, an English medical practitioner, pointed out that the phenomenon in question might be witnessed in every human eye, if looked for in the right way; and a little later and independently, BRÜCKE made the same discovery in Germany, through the curious circumstance, that occasionally when looking through his spectacles, at the face of another, he saw his neighbour's eye glare like a cat's.

In 1851, the accomplished physiologist and natural philosopher of Koenigsberg, HELMHOLTZ, showed how the observation which BRÜCKE had made accidentally, might be repeated at will; and carrying out the principle thus established, constructed an eye-speculum which soon proved a most valuable addition to the diagnostic apparatus of the oculist. Other eye-specula or ophthalmoscopes were devised or improved by RUETE and COCCIUS of Leipsic, ANAGNOSTAKIS of Athens, and the English opticians, and are now in use in the hospitals of this country and the continent. It will be sufficient for me, therefore, to give in a note, the names of some of the chief works from which those to whom the subject is new may obtain information regarding eye-specula; especially as no more complex instrument than a fragment of polished glass, or of perforated polished metal is required to show that light is reflected from the bottom of the eye; and even this is only needed to facilitate the observation; for by following Mr CUMMING'S directions the fundamental phenomenon may be witnessed without the employment of any reflector.*

The demonstrability of the proposition, that the eye is not a camera obscura,

* CUMMING'S observations are contained in Medico-Chir. Trans., Lond., vol. xxix., p. 283; BRÜCKE'S, in MÜLLER'S Archiv., 1847, p. 225.

HELMHOLTZ'S description of his speculum occurs in a little work, entitled, "Beschreibung eines Augen-Spiegels zur Untersuchung der Netzhaut in lebenden Auge. Berlin, 1851." An excellent abstract of this paper by Dr W. R. SANDERS, accompanied by comments of his own, will be found in the Edinburgh Monthly Journal of Medical Science, July 1852, p. 40. I am indebted to this gentleman for my knowledge of HELMHOLTZ'S instrument, and for the opportunity of using it.

The eye-specula of RUETE and COCCIUS, as well as of HELMHOLTZ and others, are described in a work, entitled, "Bildliche Darstellung der Krankheiten des Menschlichen Auges, von Dr C. G. T. RUETE; 1 and 2 Lieferung: Leipzig, 1854." Professor RUETE'S beautiful work contains a series of coloured drawings, representing the internal structures of the eye, as seen under the speculum.

Since this paper was read to the Society, a valuable communication on the medical employment of eye-specula has appeared in the British and Foreign Medico-Chir. Review for April 1855 (p. 501) It is entitled, "On the Means of Diagnosing the Internal Diseases of the Eye. By C. BADER, M.D., and BRANSBY ROBERTS, Esq., Resident Medical Officer, Royal London Ophthalmic Hospital, Moorfields," and contains the fullest and most recent account of eye-specula accessible to English readers, with a record of observations made on healthy and diseased eyes. From this paper, I have borrowed the use of the word Ophthalmoscope, used occasionally in the text.

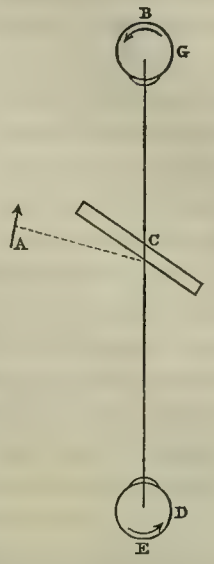
depends upon the fact, that when rays of light enter the eye, and fall upon its back wall, as many of them as are reflected from the retina, or from the choroid behind it, will exactly retrace their course, and pass out through the pupil to the luminous body or illuminated object from which they came. Thus the diverging rays of a gas-flame are converged by the refracting media of the eye, to a focus upon the retina, where they unite to produce a picture, and thereafter in great part traverse that membrane and fall upon the choroid. If from either of these membranes rays are reflected (and for the sake of simplicity, we may, for the present, limit ourslvees to the retina, which is the more powerful reflector of the two), they will follow in a reversed direction, the very course which they took in reaching that membrane, and return to the gas-flame, producing there an image of the picture on the retina, so that the reflected image of the flame is placed upon, and coincides in size and position with the actual flame.* To see, therefore, into the deeper chambers of a living eye, we must arrange matters so that we can look along the straight line of the reflected rays, without intercepting the light from which they originally came. The earlier observations of CUMMING were made without any special arrangement to prevent such interception of light; they were rendered possible by the circumstance, that certain of the rays returning from the bottom of the eye undergo irregular reflection, and diverge from the direct line which theoretically all should follow, so that if the observer keeps a very little to the side of this line, standing almost between the light and the observed eye on which it is falling, he catches a sufficient number of the irregular rays to see into the interior of the eye from which they come.

It was doubtless the accidental realization of this condition of matters, that led to the occasional observation, from very early times, of luminous emissions from human eyes;† but even when the necessary conditions are fully realized, the illumination is so imperfect that the results are unsatisfactory. By the employment, however, of a plane transparent reflector, such as a plate of polished glass, or of a plane or concave mirror, perforated or rendered transparent at the centre, the source of the light may be placed at an angle, both to the observing and the observed eye, so as to enable the former to receive directly much of the light reflected from the latter. The following diagram will show how this occurs in the case of the transparent reflector, which is the essential part of HELMHOLTZ'S instrument; the opaque perforated reflector forming the basis of that of COCCIUS; the whole reflection is supposed to take place from the surface of the retina.

* See HELMHOLTZ'S "Beschreibung eines Augen-Spiegels," &c.; and Dr SANDERS' excellent abstract of this Memoir, from which I have borrowed in the text; also RUETE'S Preliminary Chapters in his Bildliche Darstellung.

† On the Luminousness Observed in the Eyes of Human Beings. Edinburgh Phil. Journal, 1827, p. 164.

" Let A be a flame, whose rays are caught at an angle on a glass-plate C, the rays will be thrown along the line CD, into the eye D, which will see an image of the flame along the line DB; but the rays reflected from the retina passing out in the same line DC, will again meet the plate C; they will be in part turned towards A, but in part also will traverse the glass-plate C, and go to form a picture at B of the image on the retina; but an eye G, placed behind the glass-plate and on the line CB, will meet these reflected rays, and will consequently see the posterior chamber of the eye D illuminated."*



The experiment is thus made:—" In a dark room, with a single flame at the side of the experimenters, and on a level with their eyes, the person whose eye is to be observed holds a piece of glass (a microscope glass slip), so as to catch the image of the flame on it; he then, by inclining the glass, brings the image of the flame opposite the pupil of the observer's eye; the latter will then see the pupil of the observed eye luminous, of a reddish-yellow bright colour. . . . . A person may also see one of his own pupils luminous: standing before a looking-glass, and seeing the image of the flame in the reflector with his *right* eye, let him bring this image opposite the pupil of the left eye in the looking-glass; the left eye will then perceive the right pupil in the mirror luminous."†

The very simple arrangement which has been described, is all that is required to develop the phenomena to which I wish to refer; and the speculum differs from it only by employing four glass-plates instead of one, to increase the illumination of the observed eye, and adding a double concave glass, to increase the distinctness of the image on the observer's eye.

The reflector of Coccius, plane or better concave, is an ordinary glass mirror, with the silvering on the back scraped off, so as to leave in the centre a small transparent circular spot, or with a hole bored through the centre of the entire mirror. Through this transparent spot or aperture the observer looks from the unsilvered side, directing the light reflected from the opposite surface into the observed eye, while the pupil of the latter and of his own eye are in a straight line.

The mirror is round, with a diameter of about two and a half inches; and a focal distance of about six inches. The aperture in the centre has a diameter of about two lines. The arrangement of the lamp is substantially the same as when Helmholtz's speculum is used, and the mode of action, so far as illumination of the observed eye is concerned, similar.

With either of the arrangements described, the interior of any eye can be

* Edinburgh Monthly Med. Journal, July 1852, p. 41.

† Op. cit., p. 42.

readily examined, and I see no reason to doubt that photographic images of the retina and choroid may be obtained and preserved on collodionised or other actino-sensitive surfaces.

By means of such Ophthalmoscopes, in the hands of HELMHOLTZ, COCCIUS, RHETE, and others, it has been placed beyond question, that much of the light which enters the human eye is reflected from the anterior surface, or from some depth within the layers, of the retina, without entirely traversing that membrane, or reaching the choroid, so as to be subjected to the absorptive action of its dark pigment. So far, therefore, as the accepted theory of vision demands that the light which has reached the retina shall not return across the chamber of the eye, it must be abandoned; and the evils which are supposed to be inseparable from such cross lights, or luminous reverberations, are encountered at every moment by every eye, whether animal or human, at least in the case of the Vertebrata, which is engaged in the exercise of vision; yet it is vision so marred and obstructed which we are in the habit of calling perfect. We must plainly mend our theory, or our language, for they are inconsistent with each other.

Before seeking to determine which must be altered, it is important to notice, that, according to the views of certain recent writers on the eye, among whom I may specially name KÖLLIKER and HENRY MÜLLER, the German physiologists, it is not the anterior part or face of the retina, but its deeper portions which are optically sensific; and light must penetrate to them before the sensation of vision can be experienced. If this view be well founded, then we may be led to the somewhat startling conclusion, that much of the light which is reflected from the retina, at the best contributes nothing to vision whilst within the eye, even if we deny that it positively obstructs it. And if we suppose, with the older authorities, that all the light which reaches the retina penetrates it sufficiently deeply to excite luminous sensation, then it is manifest that so far as that portion which is reflected outwards is concerned, the dark surface of the choroid is quite superfluous.*

* I am indebted to my pupil, Mr JAMES WARDROP, an accomplished theologian and naturalist, for an abstract of the views of KÖLLIKER and H. MÜLLER. Their opinions are contained in KÖLLIKER's Micr. Anatomie, B. 2, Zweite Hälfte 606–720. See also H. MÜLLER's *Remarks on the Structure and Function of the Retina*, translated in the Quarterly Journal of Micr. Science, July 1853, pp. 269–273. Without entering into the minute discussion of anatomical questions which I am not competent to decide, it may be noticed, that both the skilful observers referred to, agree in denying to the fibres or expansion of the optic nerve, the function of perceiving "objective light." This function belongs, according to them, to the *deepest* of the five layers of which they regard the retina of vertebrate animals as composed. This layer, which is immediately in front of the choroid, is thus described by KÖLLIKER:—"The external or bacillar layer consists of the 'rods and bulbs' (bacilli et coni), whose ends evenly terminated, form a sort of mosaic pavement towards the black pigment, and which internally are continued by fibres (MÜLLER's fibres) through the three succeeding retinal layers, to abut abruptly, and with radiating terminations on the external aspect of the fifth layer, which, however, they do not penetrate." This fifth layer is a very delicate membrane, investing the entire internal or anterior aspect of the retina.

Accepting this description as well-founded (and it has received the approbation of the majority of recent physiologists), it appears that light must more or less traverse the anterior layers of the retina, till it reaches the "rods and bulbs," with their connecting radiating fibres, before it can excite a

On either view, we must qualify the statement that the eye is a camera obscura; but before attempting such qualification, it is necessary to consider the reflection of light from the choroid behind the retina. As the latter membrane during life is quite transparent, and is traversed throughout its entire thickness by many of the rays which fall upon it, these must, in part, be reflected from the choroid which receives them, unless we impute to this membrane a power of absolute absorption, so far as light is concerned. Something little short of such a power is habitually attributed to the choroid, and not unnaturally. The darkness of the pupil, even in the lightest normal eye, is by most persons referred to the dark background against which we are supposed to see it, as its cause. The pink pupil of the Albino is in the same way connected with the crimson choroid at the bottom of his eye; and these views are supposed to be justified by the appearances in the dead organ, where a black or brown pigment is found coating the one choroid, and absent from the other. It has thus been generally inferred, that the dark choroid of the perfect eye does not sensibly reflect light, whatever may be the case with the retina,—a conclusion certainly not warranted by dissection; for, on the one hand, the choroid after death appears darker and less reflective of light than during life, in consequence of the bloodvessels of which it so largely consists becoming emptied of blood; and, on the other, JOHN HUNTER has long ago permanently illustrated in his great museum, that the pigment of the choroid "in the human species is," to use his own words, "of all the different shades between black and almost white," * and, as he also states, is generally lightest in colour at the bottom of the eye,† where, of course, its position best enables it to act as a reflector. Excluding the white choroid, which belongs only to the abnormal albino, there is plainly room in the other pale tints for much reflection.

But it is needless to accumulate arguments in proof that the choroid *must* reflect light, seeing that by means of the ophthalmoscope, every one may satisfy himself that this membrane certainly does. HELMHOLTZ referred to his instrument chiefly as a retina-speculum, and as such it was described by Dr SANDERS, writing for practical medical men, to whom the retina is much more an object of interest, as liable to disease, than the choroid. The latter membrane, however, was plainly within the reach of the speculum; and later observers have carefully depicted and described its appearance under their ophthalmoscopes. RUETE and

luminous sensation. It must, however, in part be reflected from those anterior layers before reaching the deepest one. The internal or anterior surface of the bacillary layer is further described, as smooth and brilliant, the "bacilli and coni" appearing like the polished surfaces of crystals, so that they reflect light powerfully, and the greater number of the rays which are returned from the retina, have probably been reflected from its deeper layers, whilst a portion has been thrown back from the anterior layers, without contributing to the perception of light. The only point, however, which I am much concerned to urge is, that the retina, *as a whole*, reflects much of the light which reaches it.

* Observations on certain parts of the Animal Economy. By JOHN HUNTER, F.R.S., vol. iv., p. 278.

† Catalogue of Museum, R. C. S., Lond., vol. iii., p. 133.

Coccius specially refer to the varying colours of the "*background*," as they term it, of the eye, *i. e.*, the retina and choroid taken together, which is never black, nor even brown, but bright red when the *pigmentum nigrum* is scanty, yellowish-red when it is more abundant, and brownish-red when it is largely present. Coloured drawings are given in illustration of these statements.*

Messrs Bader and Roberts describe the choroid as exhibiting under the ophthalmoscope, "a brilliant red surface," and state more particularly, that this membrane is "covered by a very tender, pointed, grayish-brown layer of pigment, giving the appearance of an uniform red surface;" adding that "a comparison of many healthy eyes of different ages is needed for the purpose of having a correct idea of the normal red of the choroid." †

In the comparatively few observations which I have been able to make on the appearance of the choroid through Helmholtz's speculum, it has appeared yellowish-red, and it is thus represented in Ruete's drawings. Some allowance must doubtless be made for the quality of the light employed, which is always from an artificial source when ophthalmoscopes are used; but after allowing for this, it appears that even minute medical observers are content to describe the choroid as uniformly red or yellowish-red, and that in the darkest human eyes it deepens only by a degree or two into brownish-red. In short, whereas, were the choroid so powerfully absorbent of light as it is generally supposed to be, it should more or less resemble, as seen through the retina, polished ebony or walnut wood, it reflects so much red light, that one who saw it for the first time, would probably at once credit the statement, that he was gazing on the choroid, not of a normal, but of an Albino eye.

The choroid thus, as well as the retina, is a reflector of light, and deprives the eye of the character of a camera obscura; nor can it have escaped the notice of the able Germans to whom we are indebted for the first full exposition of this truth, that it necessitates an alteration in the current theory of vision. Yet so far as I am aware, no alteration of that theory has hitherto been proposed by any writer; nor are the facts which demand a change as yet familiar to opticians or natural philosophers who are not physiologists.‡

The reluctance which has thus been shown to include in an altered theory of vision the occurrence of deep reflection of light within the eye, appears to be based upon three considerations. The 1*st*, that in the eyes of human Albi-

* Bildliche Darstellung, Tab. II. † Brit. and For. Medico-Chir. Rev., April 1855, p. 509.

‡ Since reading this paper to the Society, I have learned from Mr Wardrop's Abstract of Kölliker's paper, that some of the latter's countrymen suppose that the bacillar layer of the retina reflects light on the optic fibres, and thus enhances vision. Kölliker's words are "This bacillar layer does not act as a catoptrical apparatus, as Hannover, Brücke, and Helmholtz think, for reflecting the light back to the optic fibres, but as a true nervous apparatus for itself receiving the impression."—(Micr. Anat., B. ii., Zweite Hälfte, p. 691.) Even, however, were the view objected to by Kölliker well-founded, it would not dispose of the question discussed in this paper, for the main direction of the light reflected from the bacillar layer of the retina must be forwards and ontwards, so as to illuminate more or less the entire chamber of the eye.

noes, where the pigment of the choroid is altogether absent, and choroidal reflection is at a maximum, vision is imperfect, as well as painful. The 2*d*, that those animals which have the pigment of the choroid at the bottom of the eye covered or replaced by a *tapetum lucidum*, or metalline mirror, are impatient of the sun, and, like albinoes, see comfortably only by faint twilight. The 3*d*, that the known laws of luminous reflection make it impossible that vision should be perfect, if light is free to cross the chamber of the eye in all directions.

Now, to take those objections in order:—1. It is not to be denied that albinoes of our own race experience great intolerance of bright light; see with difficulty, unless it is faint; are generally short-sighted; and exhibit, unless in darkness, a continual tremulous motion of the eyeballs, as if vainly seeking to remove from the influence of light each dazzled point of the retina on which it falls. But it appears to have escaped the observation of physiologists, that it is otherwise with many of the albino varieties which occur among the lower animals. In thinking over this matter, I called to mind, that in watching in earlier days the habits of pink-eyed white mice and white rabbits, I detected no appearance of their vision being less perfect, or exercised with less readiness and pleasure than that of their dark-eyed brethren; and all to whom I have mentioned this opinion, familiar with the ways of those animals, have confirmed my conclusion. Nor is it difficult to explain why animal and human albinism should differ. There are, in truth, two kinds of albinism, the one showing itself suddenly, or, as it were, *per saltum*, in the immediate offspring of parents who are not themselves albinoes; the other appearing in every individual of a particular race or variety of animals, and having been transmitted to it as a hereditary peculiarity, which has descended through hundreds or thousands of generations.

In the lower animals both kinds of albinism are frequent. A white crow, for example, or a white blackbird (as we are compelled to call it), is known occasionally to make his appearance among his darker brethren, although unquestionably himself of sable lineage; but a permanent albino variety of either bird is not known. In the mouse, on the other hand, the rat, the rabbit, and a few other animals, albinism has become permanent in certain varieties, probably assisted in some by the interference of man in mating the animals. The ferret, moreover, appears to be a natural variety of permanent albinism.

The albinoes of our own race are all, so far as I am aware, accidental, or perhaps it would be better to say, incidental or sporadic cases. The older writers, indeed, acknowledged a distinct race of *Leucaethiopes*, or White Ethiopians, and a modern tradition, still sacred among travelling exhibitors, ranks all European Albinoes among Circassians. There is probably some foundation for both opinions; but though albinism is certainly liable to become hereditary among ourselves, I am not aware that a case is on record where the parents of a human albino both displayed this structural peculiarity; or when it has descended to a

third generation, that the majority of the parents were albinoes; or that in any case *all* the children of a single pair in an albino line exhibited albinism.

The human albinoes, accordingly, to whom our physiologists have referred, have been generally exceptional or transitional examples of albinism. They have inherited the highly sensitive retina, and rapidly contractile, irritable iris of their parents, without a similar legacy of the defensive dark pigment, which in them protected both organs from the painful impression of excessive light. With the instincts, accordingly, of their progenitors, and trained by them to pursuits like their own, albinoes of our own race have sought to follow all legitimate callings, and in so doing have soon betrayed their visual infirmity. The imperfection of their sight has thus been rendered so manifest, that the hypothetically perfect vision of others of our race has been assumed to be necessarily the result of an exactly opposite condition of the eye. On the other hand, in an albino rabbit of the present day, which has probably had some thousand pink-eyed ancestors, the sensibility of the eye has, generations before its time, been adjusted to the conditions of its existence; for it is certain that the permanence of any variety among animal species is possible, only provided the variation does not oustep the limits within which the conservation of life and health is possible. Hereditary albinism had in prospect the alternative of total blindness, such as characterises the eyeless fishes, and crustacea of the Mammoth Caves of Kentucky; or modified perfect vision, such as it has actually attained to. The modification which it has suffered is in reality very small, for what has been lost in one direction by the hereditary Albino eye, has been gained in another; if it sees worse than a dark eye by meridian light, it sees better by twilight.

The Edinburgh Zoological Gardens afford at present an excellent opportunity of testing the truth of the opinions I have brought forward. They contain an albino monkey, an albino rabbit, and several litters of albino rats and mice. The genealogy of the monkey is not known, but permanent albinism is believed to be as rare among monkeys as among ourselves, and the individual in the Gardens exhibits all the peculiarities which characterize human albinoes. I paid a visit recently to the Gardens, and found my preconceptions more than confirmed. The monkey, a young adult female, is covered with white hair, which gives her an aspect of great age; and the overhanging eyebrows, knitted to exclude the light, add an air of gravity to her venerable look. But she is a gentle creature, and the offer of a nut or a piece of biscuit, brings her at once from a favourite shady corner of the cage to the front, where her vision can be readily tested. I saw her in the afternoon of a bright sunny day, but not in direct sunlight. Diffuse light evidently distressed her; the eyebrows were pulled down to the utmost, the pupils strongly contracted, the eyeballs in constant oscillation, and the hands raised in a most human-like way to shade the sight. Frequently one eye was entirely covered, and the other alone employed in vision; and when anything

specially attracted notice, both eyes were carefully shaded. By a liberal gift of nuts, however, which occupied for a time both anterior hands, the eyes were left unshaded, and could be examined. They did not exhibit the signs of extreme albinism. The pupil was not pink, and the iris was a pale blue.

In strong contrast with the painful vision of the monkey is the quick sight of a white rabbit, and several white rats and mice, which occupy neighbouring cages. Their fur is milk-white; the skin colourless; the pupil pink or rather crimson, and large, as compared with that of the monkey.* The animals were exposed freely to diffuse daylight, but did not shrink from it. No conspicuous change could be observed in the pupil; there was no oscillation of the eyeballs, and no closure of the eyelids. They seemed to see as well as their non-albino neighbours, and to be as little incommoded by daylight, and they were equally quick in discerning and seizing food. Their keepers, who were aware of the monkey's peculiarities of vision, considered the other albino animals quite as sharp-sighted as their dark-eyed brethren. I would not say so much myself, or rather I would say it differently. I have no doubt that the albinoes would see worse in full sunlight, but I have as little doubt that they would see better in faint twilight.

It cannot then, I think, be questioned, that in those animals which exhibit the full development of long hereditary albinism, the sensitiveness of the retina to light has undergone a permanent abatement, whilst the iris has probably altered also in thickness and contractility. I venture to predict, that if ever an albino race of men shall be observed or developed, they will prove, after the lapse of a generation or two from their founders, to have eyes as serviceable as those of the majority of mankind.†

* A comparison, also, of pink-eyed with dark-eyed rabbits appeared to show that in diffuse daylight the average size of the pupil is the same in both; but in direct sunlight the albino pupil is smaller.

† In truth, in the case even of casual human albinoes, vision, however painful in full daylight, is not to any marked extent optically imperfect; and in the diffuse or moderate light by which they see correctly, reflection of its rays within the eye is occurring as certainly as when the light is direct and intense. I hesitated to urge thus much when the text of this paper was read to the Society, although the conclusion in question seemed justified by the accounts on record of albinism, especially by Dr Sachs' very interesting description of his own case and his sister's. (*Hist. Nat. duorum* Leucaethiopum *auctoris ipsius et sororis ejus, à* G. Sachs, M.D. See also my *Researches on Colour-Blindness*, p. 102). But since this paper was read, I had an opportunity, through the kindness of Dr James Sidey, of testing, along with Mr James C. Maxwell, the vision of an albino girl, aged 18 or 19. She was born in India, of Scotch parents, in humble life, and is, in all respects, a well-marked case of albinism. She sees with much less suffering in this country, than she did in that of her birth, but bright sunlight still distresses her. There is occasional strabismus of one eye, and both eyes exhibit, under exposure to light, the tremulous oscillation characteristic of albinism; not, however, to a very great degree. She thinks that her vision has improved within the last few years, but how far this is the result merely of removal from the influence of a tropical sun, it would be difficult to determine.

We found her quick and intelligent. By diffuse daylight, she distinguished the *forms* of objects rapidly and accurately. Many trials also were made as to her perception of *colours*, which Mr Max-

However that may be, it is sufficient for my present purpose to point to the albino animals, whose eyes are totally destitute of pigment, and reflect light from every point of the surface, both of the retina and the choroid, but, nevertheless, exercise the faculty of sight in perfection. Their eyes, even when the iris is fully contracted, remain, in virtue of the transparency of that membrane, *camerœ lucidæ;* their possessors cannot render them *camerœ obscurœ* ; and yet they are excellent organs of vision.

2. If the reasoning pursued in reference to the albino eye be valid, it will serve also to dispose of the difficulty experienced by some in explaining how vision is compatible with the presence of a *tapetum lucidum* in the eyes of many animals, This tapetum is equivalent to a concave mirror of polished metal, replacing the pigment of the choroid over a greater or smaller part of its surface, especially at the deepest or most posterior portion of the chamber of the eye, so that lying behind the retina, it is more or less directly opposite the pupil, and receives the light which enters by it. A brilliant reflecting surface of this kind is found in many of the mammalia, both graminivorous and carnivorous, as the horse, the ox, the sheep, the cat, the dog. It is present in the eyes of the whale, seal, and other marine mammalia ; and in fishes, such as the shark, in which it is peculiarly brilliant. It occurs also in certain of the mollusca, as the cuttle-fish ; in certain insects, as the moths ; but never, I believe, in birds. It is most largely developed in animals which are nocturnal in their habits, or live like fishes in a medium which is dimly illuminated. All must be familiar with the glare of light which it throws from the eye of the cat or dog, when these animals exhibit dilated pupils in twilight.

This *tapetum lucidum*, has been a great stumbling block to physiologists. The albino eye was set aside as abnormal ; and the reflection of light in normal eyes from the retina and choroid was overlooked, or regarded as accidental, but that from the tapetum could not be. Most writers, however, dismiss it with an unsatisfactory and very brief comment, unable evidently to reconcile its presence with the maintenance of that internal darkness of the eye, which is supposed to be essential to vision.

Thus, J. Müller, in the passage already quoted, alludes to dazzling from excess of light, and indistinct vision, as inevitable peculiarities of the tapetal eye.

well's Colour-Top allowed us to test carefully, and with the result, that her sense of colour was acute, precise, and normal. In short, her vision by diffuse light was optically as perfect as that of the majority of mankind, and to appearance, a diminution in the sensitiveness of the retina was all that was required to make vision equally perfect in direct light.

The iris in this case was pale blue, and the pupil was not pink by diffuse daylight. It became so, however, in the neighbourhood of a gas flame, and her friends were familiar with the fact that by gas-light, her eyes often "flashed fire."

We found it easy to observe this ocular luminousness at will, and the fact is important, as proving that the comparatively perfect vision which this Albino possessed, was exercised by eyes, within which a large amount of cross-reflection of light was continually occurring.

B. Prevost satirizes the belief, that a mirror within an animal's eye can assist it in seeing.* Dr W. Mackenzie agrees with him, and observes, that "Reasoning à priori, we should say that the tapetum would render the eyes weak, and impatient of light."†

It is needless to discuss those opinions in detail. If in the eyes of those animals which have not tapeta, there is, as we may phrase it, simply *permission* for internal reflection of light to occur; in those which have tapeta, there is plainly direct *provision* for its occurrence; and it is astonishing to find writers of such ability as those I have quoted, finding no function for the tapetum but that of disturbing the vision of its possessor. We are often compelled to acknowledge our ignorance of the function of an animal organ, but surely never to confess that it was given to its owner solely to incommode or injure it! Only those whose thoughts were preoccupied by the theory that the eye must be preserved a camera obscura, could have brought themselves to credit and affirm that the horse, the ox, the lion, the dog, the seal, or the shark, are animals that see imperfectly. With the exception of birds, they are probably excelled by no animals in the quality of their vision.

3. The last objection I have to notice raises the difficulty, that the known laws of luminous reflection render it impossible that perfect vision can be performed by an eye, across the chamber of which light is continually passing and repassing. But if it be the case, as I have sought to show, that the most perfect vision which comes under our notice is performed in spite of such reflection continually occurring, it is manifest that there must be some misconception regarding the evil influence of cross lights within the chamber of the eye.

A little consideration will show, that as all the light which enters a normal eye enters it by the pupil, and is refracted to a focus on the retina at a point more or less directly opposite its place of entrance, if it is reflected from the retina or choroid, it will in larger part simply retrace its course, and pass out through the pupil as it entered by it. This escape of the reflected light through the pupil, carries it clear of the internal walls of the eye-chamber, and, as we have already seen, renders possible the construction of ophthalmoscopes.

Two circumstances may interfere with the exit through the pupil of the reflected rays. 1*st*, The pupil suddenly exposed to bright light, may contract and diminish its area before the rays which entered by it have had time to undergo reflection. In that case the rays furthest from the centre will fall upon the back of the iris, and undergo in part a second reflection from it. But the reflection thus occurring will not be great, for special provision is made in all but Albino eyes for the stoppage of such rays, by the great thickness of the uvea or pigment on the posterior surface of the iris, which has no highly reflective retinal covering like the choroid.

* Edin. Phil. Journal, 1827, p. 302. † Physiology of Vision, p. 220.

2*dly*. A good deal of light must undergo irregular reflection and dispersion from the retina and choroid. But so much of this light as passes towards the front of the eye will be arrested in greater part by the abundant pigment of the ciliary processes, and anterior lateral portions of the choroid; and what scatters laterally will only produce a general excitation of the retina without developing a second complete image of any visible object on the nervous membrane, which is the chief optical evil to be apprehended from intra-ocular reflection. So long as the direct seat of vision is not exposed to strong reflex illumination, and the same entire pencil of rays does not twice depict the image of the same object on different parts of the retina, and thus produce double vision of single things, the general reflection and dispersion of light within the eye cannot do more than diminish the darkness of the eye-chamber as a whole. In the eye of the casual albino, light is liable to be returned from the back of the greatly contracted iris to the place of most perfect vision, and to disturb it, so that, as far as internal reflection is concerned, he would be better without an iris at all. Moreover, his iris is transparent, and is continually transmitting light from without as well as reflecting it from within.

But even in the human albino, although unquestionably there must be painful dazzling of the eye from the continual action of light on every point of the retina, still, unless during sudden transitions from very faint to very bright illumination, involving great and rapid change in the area of the pupil, the *yellow spot* or place of perfect vision being opposite that aperture, will be less exposed than any portion of the retina to the impact of light which has undergone intra-ocular reflection; and as the utmost contraction of the iris does not close the pupil, a pencil of rays reflected from the yellow spot, can never return *in toto* to repeat upon another portion of the retina the image which it has already produced upon that spot.

It thus appears that the laws of luminous reflection do not necessitate imperfect vision, as applied to the fact, that the retina and choroid return much of the light which reaches them, for:—

1*st*, In the normal and also in the albino vision of all animals, man included, the amount of direct retinal and choroidal reflection is necessarily coincident with the width or degree of dilatation of the pupil; the larger the pencil of light entering the pupil, the larger the pencil leaving it, so that in every case the reflected rays are thrown out of the eye and do not disturb vision: further:—

2*nd*, In those animals provided with *tapeta lucida*, such as the cat, the dog, or the ox, which are only partially nocturnal in their habits, the tapetum is so placed that in bright light it is not opposite to the contracted pupil, or is so only to a small degree.* When, however, the choroidal mirror is called into action in

* John Hunter, in Catal. of Museum of Royal College of Surgeons, London. Vol. iii., p. 165.

twilight, the pupil is correspondingly dilated, and all the light which the tapetum reflects finds a free passage for its escape.

3*d*, In the eye of man, as well as in that of a large number of other animals, the background of retina and choroid on which the image is depicted, is not the darkest portion of the ocular screen, nor even so dark as those parts of the inner walls of the eye on which objects are never figured. On the other hand, as JOHN HUNTER has shown, and illustrated by existing specimens, the front and the anterior sides of the eye-chamber are the darkest, so that the reflecting power is greatest at the bottom of the eye.*

4*th*, In the human eye, where, more even than in those of the lower animals, it has been contended that the conditions of a camera obscura must be realised, the place of perfect vision, instead of being additionally darkened, is occupied by the well-known *yellow spot*, which has a marked reflective power, and is easily discerned by ophthalmoscopes.

The results which are announced in the preceding argument may be summed up as follows:—

1. The total absence of pigment from the choroid, the ciliary processes and the iris is compatible (especially where this condition is hereditary) with perfect vision.

2. The replacement of the pigment of the choroid lining the bottom of the eye by a concave mirror (*tapetum lucidum*) powerfully reflecting light, characterizes animals whose vision is very acute.

3. The non-tapetal or mirrorless eye of man, and of many animals, differs only in degree from the tapetal or mirrored eye of others; for the retina and choroid act as a tapetum, and reflect light in the same way.

4. The eyes of vertebrate animals are only to a limited extent *cameræ obscuræ*, and internally are least dark in the portions most directly exposed to the action of light, and where the seat of perfect vision is placed.†

5. The back of the iris, over which the retina does not pass, is the darkest internal portion of the eye in vertebrates; and next to it, in the majority of these, are the ciliary processes of the choroid, and its anterior lateral portions.

* HUNTER states that in animals where the pigment of the choroid is light in colour, "the lightest part is always at the bottom of the eye, becoming darker gradually forwards, and in such it is often quite black; viz., from the termination of the retina to the pupil; or if not black, it is there much darker than anywhere else. This is generally the case in the eyes of the human subject." Catal. Mus. R. C. S., London. Vol. iii., p. 133.

† Comparative anatomists must decide to what extent these observations demand qualification in reference to particular tribes of animals. The nocturnal lemurs, which have a uniformly coloured dark choroid, no tapetum, and a very sensitive retina, probably exhibit intra-ocular reflection to a small extent compared with other quadrupeds. A similar remark applies to birds, qualified by the fact that the bottom of the eye-chamber is occupied in them by the *marsupium* or *pecten*, an organ the use of which has not been ascertained, so that we cannot be certain how it modifies vision. But as the researches of KÖLLIKER and H. MÜLLER demonstrate that the general structure of the retina is the same in all vertebrates, it appears certain that, however dark and absorptive of light the choroid may be in some of them, the retina in all will act as a mirror towards light incident upon it; and

From those premises the conclusion is deducible that in vertebrates much light is reflected from the bottom of the eye-chamber during the exercise of vision without disturbing it; but that little is reflected again, so as to return to the bottom of the eye, in consequence partly of its absorption by the pigment of the anterior portions of the choroid, partly of its escape through the pupil.

It may seem to some that this reasoning proves too much, for why is there in man and many other animals a pigment at the bottom of the eye, if reflection from the membrane there is so free to take place? To this I reply that the pigment, which is never altogether inoperative, comes into special action when the eye is exposed to very bright light, and saves the retina from the paralyzing influence of intensely luminous rays. Vision, however, cannot be continuously exercised under such exposure, even where the light is not excessively brilliant, in consequence of the instinctive closure of the eyelids, and the abundant secretion of tears which then take place. The pigment at the bottom of the eye is thus, I apprehend, a safeguard against sudden exposure to intense light; but during continuous vision under an illumination which does not dazzle the eye, its action is secondary as an absorber of light, and it always acts as a reflector.

Hitherto I have been arguing almost solely for the negative conclusion, that the vertebrate, and especially the human eye, is not the kind of darkened chamber which it has been supposed to be. It is impossible, however, to regard the deep intra-ocular reflection which so certainly occurs in most animals, as an incidental or useless phenomenon. That it has a direct and beneficial influence over vision I cannot doubt, and I proceed briefly to indicate where the proof of this is to be found.

Intra-ocular reflection, as a normal phenomenon, is at a maximum in the tapetal or mirrored eye of the lower animals. It is desirable, accordingly, to study it first as occurring in them; nor can a better example of a mirrored eye be found than that presented by the shark. In it the *tapetum lucidum* occupies the whole of the bottom, and one-half or more of the lateral surface of the choroid, which is covered by pigment only in front. The iris, as in other fishes, is incontractile, so that the diameter of the pupil never varies; and the tapetum, which is colourless and very brilliant, is thus always in action as a reflector. The shark, moreover, swims near the surface of the sea, where the amount of light is considerable, and the acuteness of its vision is proverbial. I have selected it, rather than a mammal, with eyelids and a contractile iris, because in the shark luminous reflection never ceases unless in absolute darkness; and when light is shining occurs the more, the brighter the light is. Its eye thus is always in the

the very curious observations of the latter author on the eyes of the *cephalopoda* (Quart. Journal of Micr. Science, July 1853, p. 269), show that the retina of these invertebrates will act in the same way, a remark which, *mutatis mutandis*, may be applied to every creature, whatever its rank in the animal scale, which has shining or so-called phosphorescent eyes. A most interesting field is open to naturalists in the examination, by means of the ophthalmoscope, of the eyes of living animals of all grades.

condition in which that of a cat, or dog, or ox is, when subdued light causes the iris to expand, and allows the reflecting tapetum to come into play, so that the considerations which I have to urge apply to the mammal as much as to the fish, provided they are taken with pupils equally dilated; but as the tapetum in the shark is very large, very brilliant, and always in action, I shall restrict myself for the present to it.

The light, which penetrates to the bottom of a shark's eye, will, in part, be reflected from the retina (a phenomenon which for the present I disregard), in part traverse it, and reach the tapetum, where a portion will be lost by absorption and irregular reflection or dispersion, and (what alone concerns us here) in part undergo direct reflection, return through the retina, and escape by the pupil. This returned light will impress the retina in traversing it, and illuminate external objects on leaving the eye.

The first question, then, is, "How will this light impress the retina?" According to J. Müller and W. Mackenzie, as we have already seen, only injuriously, so far as freedom from the sensation of dazzling, or distinctness of visual perception, are concerned; according to Todd and Bowman "probably" by "increasing the visual power, particularly when the quantity of light admitted into the eye is small."* I have urged elsewhere that "what is equivalent to two rays of light falling upon the retina will produce two impressions. We send a capillary sunbeam through the retina in one direction, and instantly return it through that membrane, a little diminished in intensity, in the opposite direction; if it determined a sensation in its first passage, what is there to prevent its doing so in its second? If, for simplicity's sake, we suppose exactly the same points of the retina to be traversed by the incident and the reflected ray, then (unless the luminous intensity of the incident ray was so great as by its passage to exhaust the sensibility of the retina), the reflected ray will repeat somewhat less powerfully the impression made by the incident one. The difference will be as great as there is between a sound and its echo, but not greater.

"On this view of matters, the tapetum, especially in twilight, will serve the important purpose of making every perceived ray of light *tell twice* upon the retina, so that the sensation it produces will either be increased in distinctness or in duration, and probably in both."†

I will not deny that we are not entitled at once to infer that because a molecular change (modulation, vibration, polarization?) transmitted through a special structure in *one* direction produces a peculiar sensation, it will certainly produce the same sensation on being transmitted through that structure in the *opposite* direction; but there are strong analogies in favour of such a view, and it is entitled to be regarded as a likely hypothesis.

* Physiology of Man, chap. xvii., p. 23.

† Researches on Colour-Blindness, p. 99.

The first probable use of the tapetum, then, is to double the impression which light produces upon the retina, whilst that light is within the eye.

The greater part of this light, however, after traversing the retina with little diminution by absorption, passes outwards through the pupil, and, along with the light reflected from the retina, is thrown upon external objects, and illuminates them. A singular reluctance has been shown by physiologists, especially in recent times, to acknowledge this. The supposed necessity of maintaining the chamber of the eye dark, the apparent impossibility of the eye reflecting and receiving light simultaneously, and the faintness of the light emitted from tapetal eyes, have led most writers to contemn the doctrine that the tapetum is a serviceable reflector of light. But the objections to this doctrine are in reality of no value, and were not entertained by the older writers, such as HUNTER and MONRO, who not only regarded the tapetum as casting light on external objects, but, in the case of graminivorous animals, as affording them, by the green colour of the light which is reflected, an assistance in discovering their food; an opinion which CUVIER in part countenances.

As I have discussed this question at length elsewhere,* I shall merely observe here that as the light emitted from a cat's or a shark's eye, *ex. gr.*, is veritable light, there is no room for affirming that its illuminating powers are not, *cæteris paribus*, equal to light of the same quality from any other source. If we can see a cat in the apparent darkness, which otherwise would render it invisible, by the light which issues from its eyes, it cannot be questioned that it will see us by so much of that light as our persons reflect back into those eyes. The *tapetum lucidum* is, for every creature which possesses it, a lantern, by which it can guide itself in the dimmest twilight, and make each ray of light do double or triple service, in assisting it to steer its course, and to find its food or prey.†

But if the tapetum assists carnivorous animals in finding their living prey, it must also give the latter warning of the approach of the destroyer. I am not aware that this use of the tapetum has hitherto attracted attention.‡ But a lion or a shark does not more certainly bring into view, by means of tapetal light,

* Researches on Colour-Blindness, pp. 88–100.

† JOHN HUNTER fully recognises this function of the tapetum, but (unless I misunderstand his meaning) regards it as useful to its possessor solely by reflecting rays of light "on the very object from which they came"—(*On the Colour of the Pigmentum of the Eye*, HUNTER'S *Works*, vol. iv., p. 285),—so that, on their return from this object, they "strike exactly, or nearly, on the same points in the retina through which they first passed" (Op. et loc. Cit.), and increase the visibility of the object in question.

This undoubtedly will be the result where the eye of the animal remains for some interval of time perfectly at rest; but the movements of a shark, *ex. gr.*, are sufficiently rapid to enable its eye to receive light from one object and reflect it upon another, from which it receives it again, so that the rays sent from the first body enable it to see the second; and this, I apprehend, is as much the function of the tapetum as deepening the visual impression of the same object.

‡ I suggested it last autumn in *Researches on Colour-Blindness*, Edin. Monthly Med. Journal, September 1854, p. 234.

the creature it would devour, than it betrays its own presence to that creature, and the balance is thus mercifully maintained between the preyer and the prey. That singular "hypnotising" or "mesmerising" power which, in the case of the serpent, is called "fascination," is probably largely possessed by the glaring tapetal eye, which acts with all the advantage of surrounding darkness to increase its impressiveness, and prevent other objects from distracting the attention of the subject of fascination. On the other hand, however, the tapetal light is peculiarly startling to an observer, for it is always coloured and unlike that of day, resembling in character (in the case at least of the cat and the dog) those fluorescent rays of the spectrum, which Mr STOKES describes as "ghostly," and of which it probably largely consists. At all events, its unfamiliar appearance specially qualifies it to alarm creatures who suddenly perceive it, and are led by instinct to flee from all strange lights.†

In the lower animals, then, the tapetum is probably serviceable—

1°. By doubling within the eye the impression of each ray upon the retina.

2°. By reflecting light from the eye upon external objects, so as to render food or prey more visible.

3°. By warning, through the agency of that light, creatures on which carnivorous animals prey, of the neighbourhood of their enemies.

In the discharge of those functions the retina more or less conspires, differing from the tapetum chiefly in reflecting a less coloured light than the latter does. Further, in such of the lower animals as have not tapeta, there must occur in most, alike from the choroid and the retina, and in all at least from the retina, reflection of light. In those whose eyes exhibit choroidal reflection, the same good ends will be served by it, though in a much less degree, as are secured by tapetal reflection, and of these probably the most important is the first, which cannot be attained with light reflected from the retina.

How far human vision is *sensibly* influenced by the choroido-retinal reflection which is continually occurring within the living eye, it is difficult to decide; but it must be influenced to some extent by it. It seems probable that the acute vision in faint light which characterizes those who are imprisoned in dark chambers, and which the astronomer sometimes purposely induces by long shading of his eyes before making observations, is in part due to the return of light from the choroid through the retina; in part to the passage through the highly-dilated pupil of light reflected both from the choroid and retina, which is thrown upon

† Colonel MADDEN, H.E.I.C.S., who was present when this paper was read, informed me afterwards, in reference to the subject discussed in the text, that in India, where he had served for many years, he had had occasion to verify the truth of the statement made above, so far as one animal is concerned. In a district at the foot of the Himalayas much infested by tigers, the natives, according to their own statement, were frequently afforded timely warning of the approach of these animals in the dusk, by the glare of their eyeballs, which the men compared to "yellow pumpkins."

external objects. It may startle us at first to be told that we see in part by light issuing from our eyes, but it must be so; and those traditions of learned men who could read by the light of their own eyes in what was darkness to others, are only exaggerations of a power more or less exercised by every human organ of vision.*

To one result of this choroido-retinal reflection in the human eye I would, in conclusion, refer. The light which is thus reflected, is always *coloured*, being, as we have already seen, red, yellowish-red, or brownish-red, and differing necessarily in its tint, according to the abundance of pigment in different eyes. Each of us thus adds to every object on which he looks so much *colour*, but no two pairs of eyes the same amount, and hence one great reason why no two persons, almost, will be found to agree as to the matching of one colour with another where the coloured substances compared consist of different materials; and why very marked differences present themselves in the judgments of persons equally practised in observing and copying colours.

Two artists, for example, paint from nature the same flower. The pigments which they employ for this purpose, will, of course, be as much affected by the colour communicated from the eye, as the flower is, so that, could the latter be imitated in its own materials, the copies might be identical. But as these must be made with substances whose lustre, transparency, and particular tint, differ from those of the body copied, the added colour from the eye tells unequally on the original and the copy, as compared together, and as seen by different eyes. Each, accordingly, objects to the other's colouring, but neither can induce his neighbour to adopt his tints, and both appeal confidently to third parties (who perhaps differ from both), assured that the adjudication will be in favour of the appellant. Here each may have been equally skilful and equally faithful: and neither has any means of testing to what extent he sees everything as if through coloured spectacles, which give all objects a tint for him inseparable from their natural colour. A "chromatic equation," thus originated, belongs, I believe, to every eye.

* Esser on the Luminousness observed in the Eyes of Human Beings, Edin. Phil. Journal, 1827, p. 164.

Postscript.

From my friend, Professor GOODSIR, who recently (June 27th) delivered a lecture of great interest and originality, on the Retina, to the Anatomical Students of the University of Edinburgh, I learn that, as he stated to his hearers, his microscopical observations on the structure and development of the eye had led him to the conclusion, that *only* the rays of light which are returned from behind through the retina produce a luminous sensation, and that the objective perception of light commences physically towards the choroidal, not the hyaloid extremity of the optically sensific constituents of the retina.

According to BRÜCKE (as mentioned in the text, Note, p. 334), the bacillar layer acts as a mirror, reflecting light forwards, and luminous sensation begins in a layer of grey nervous substance, situated nearer the front of the retina,—an opinion combated by KÖLLIKER.

According to Mr GOODSIR, the objective perception of light begins somewhere near the junction of the rod or cone with the Müllerian fibre (see Note, p. 332). On this view, the entire arrangement of rod or cone, with its Müllerian filament, is not a nerve-structure, as KÖLLIKER holds, but a peculiar organ referable to the same category as the tactile corpuscles and Pacinian bodies, and so constructed as to oppose the extremity of the nerve, which is contained in it, to the ray of light passing backwards from the choroid along the axis of the rod or cone, so that the ray shall impinge upon its extremity in the line of its axis, this being, according to Mr GOODSIR's hypothesis, the only direction in which a luminous ray can optically affect a nervous filament.

I have argued, in the preceding paper, for such returned light being *accessory* to vision, but according to this view it is the only light by which it is exercised. If this doctrine (however modified in details) be established, the reflection of light from the choroid will prove to be *essential* to the functions of seeing, and the necessity for the living eye being a Camera Lucida will be based upon deeper grounds of proof than I have attempted to offer.

August 10, 1855.

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FIG. 7

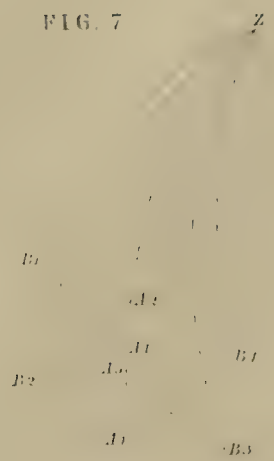


FIG. 1

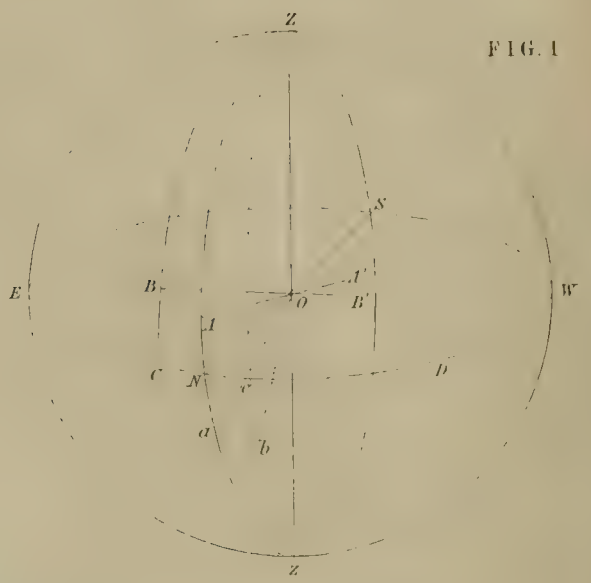


FIG. 4

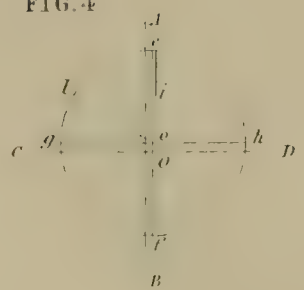


FIG. 5

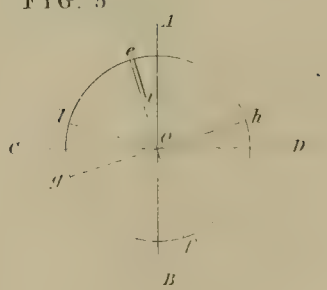


FIG. 6

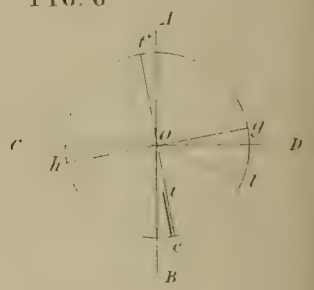


FIG. 3

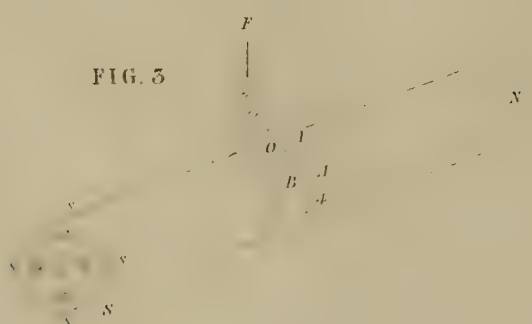
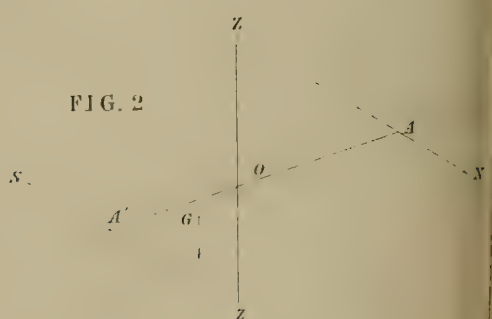


FIG. 2



XXII.—*On Errors caused by Imperfect Inversion of the Magnet, in Observations of Magnetic Declination.* By William Swan.

(Read 30th April 1855.)

1. A compass needle, although freely suspended, will not always point with accuracy in the direction of the magnetic meridian, for its magnetic axis may not be strictly parallel to its axis of figure; and hence, when a rigidly exact value of the magnetic declination is required, it is necessary to take the mean of at least two observations of the needle, first, in its usual position, and next, inverted. Some time ago it occurred to me that the declination obtained in the manner now described, will only be approximately correct, unless the inversion is accomplished with perfect accuracy; and I wished to ascertain the greatest residual error which, in given circumstances, is likely to affect the mean of two observations of a magnet, first in an erect, and then in an inverted position. I failed, however, to find any allusion to such errors in those works on magnetism to which I had access; and I was therefore obliged to investigate the subject for myself.

2. There are three forms in which a suspended magnet has been used to ascertain the magnetic declination. *First*, The ordinary compass needle, traversing a divided arc, or having a divided card attached to it; *secondly*, The magnet, having a small mirror attached to it, in which the divisions of a fixed scale are seen by reflexion, and observed by means of a fixed telescope; and, *thirdly*, The magnet converted into a collimator, by attaching to it a lens with a divided glass-scale, or cross fibres placed in its principal focus. I will consider the declinometer magnet only in the last of these cases, where the collimator is observed through the telescope of a theodolite; but the formulæ for computing the errors due to imperfect inversion will apply to any kind of magnet. I will also at first assume, that the magnet turns on its point of suspension without friction, or that it is suspended by a fibre without torsion, and that no change occurs in declination during the observations. These assumptions, it is evident, will in no way affect the accuracy of the reasoning; for, as torsion of the suspension-fibre, changes of declination, and imperfect inversion of the magnet, are all totally independent sources of error, which must be either by some means avoided, or have their effects separately computed and applied to the observations, so also the theories of these errors may be separately discussed.

Definition of Accurate Inversion of a Magnet.

3. Let ZO, Fig. 1, (Plate VII.) be a vertical line passing through the point of suspension of the magnet; O being either the point on which the agate cap sup-

porting it rests, or ZO, the torsion-fibre by which it is suspended. We may then suppose the magnetic axis to be transferred parallel to itself, until it passes through O, without altering the direction which the freely suspended magnet will assume; and, in like manner, the *observed* direction of the magnet will remain unchanged, although we suppose the optical axis of the collimator attached to it to be transferred parallel to itself, until it passes through O. Let AA′ represent the direction of the magnetic, and BB′ that of the optical axis, and let a spherical surface described about O meet the lines OA, OB, OZ, in the points A, B, Z; then the circle Z N z will be the plane of the magnetic meridian, and the spherical angle AZB, or the arc NC of the horizontal circle NESW, will measure the horizontal angle between the optical axis of the collimator and that plane. It is obvious, also, that we may include the cases of the ordinary compass-needle or of the magnet with an attached mirror, if in the one case, we conceive OB to be the axis of figure of the needle; or if in the other, we suppose that a pencil of rays proceeding from the fixed scale along a given line, such as NO, and falling on the mirror at O, is reflected to B.

If now, when the verniers of the theodolite employed to observe the magnet indicate zero, the optical axis of the theodolite-telescope is in a plane parallel to the plane ZOD, the arc DN will be the true, and DC the apparent reading for the magnetic meridian.

Hence, if the arcs DN, DC, and NC, be represented by δ δ_1 and ϕ, we shall have the apparent reading for the magnetic meridian,

$$\delta_1 = \delta + \phi.$$

Next, let z, instead of Z, be taken as the point of suspension, the angles AOB, AOZ, BOZ, all remaining unchanged, then the magnet will be in precisely the position which it would occupy if the whole figure revolved about the line NS, until z coincided with Z.

If then a O, b O represent the new positions of AO, BO, we shall have

$$a\,\mathrm{Z}\,b = a\,z\,b = \mathrm{AZB} = \phi;$$

and if

$$c\,\mathrm{D} = \delta_3,$$

$$\delta_3 = \delta - \phi,$$

where δ_3 is the apparent reading for the magnetic meridian in the new position of the magnet.

Finally, adding, ϕ is eliminated, and we have

$$\delta = \tfrac{1}{2}(\delta_1 + \delta_3);$$

which shows, that if the magnet be inverted, in the manner described, the mean of the readings in the erect and inverted positions will give a rigidly accurate result.

It will be observed, that unless the apparatus has been disturbed in the process of inversion, the angle AOB will remain unaltered. It is otherwise with AOZ, and BOZ, which do not necessarily remain unchanged, after the point of suspension has been transferred to z, or the magnet has been inverted. We may therefore define *accurate inversion* of a magnet to mean, that, a vertical straight line being supposed to be rigidly connected with it, the magnet is turned round its axis, until the line, moving along with it, becomes again vertical; the inclinations of the magnetic and optical axes to the line, thus remaining unchanged after inversion.

4. When the magnet has been inverted in the manner now defined, the mean of the readings, in its erect and inverted positions, gives a correct value of the magnetic declination. It does not, however, follow, that no other position after inversion would secure the same result, for it is obvious that a curve might be described on the surface of the sphere, such that, for all points in it, the angle $a\,Z\,b$ should be equal to AZB; and if the magnet were suspended from any of these points, we should still have the desired condition fulfilled. We might also have the magnet inverted, so that the arc AB revolved until it made an angle with ZA on the opposite side of the line, and equal to ZAB. No importance, however, attaches to the existence of these different modes of inversion; for one method, answering the desired conditions, is sufficient, and the one which has been defined as accurate inversion seems to be that most easily effected.

5. It remains, however, to be shown that the magnet will remain in equilibrium in the new position of accurate inversion. For this purpose let AA′, Fig. 2, be the magnetic axis, ZO a vertical line passing through the point of suspension, and G the centre of gravity of the magnet, which, in the position of equilibrium, will be in the same plane with AA′ and ZO. The magnet is then kept in equilibrium by the forces at A, A′, the weight of the magnet acting at G, and the tension of the suspension fibre acting in the line OZ.

Let m = the force of free magnetism in either pole,
w = the weight of the magnet;

and put

$$\mathrm{AO} = a, \qquad \mathrm{A'O} = b, \qquad \mathrm{GO} = c,$$
$$\mathrm{AZO} = \mathrm{A}z\,\mathrm{O} = \iota, \qquad \mathrm{AOZ} = \lambda, \qquad \mathrm{GOZ} = \eta.$$

Then we have

$$m\,(a + b)\sin(\lambda + \iota) - w\,c\sin\eta = 0.$$

After inversion ι remains unchanged, but λ and η may be supposed to change to λ' and η'; while the new point of suspension may either be the former point O, or any other point in the line $\mathrm{ZO}\,z$; and we shall in like manner obtain,

$$m\,(a + b)\sin(\lambda' + \iota) - w\,c\sin\eta' = 0;$$

Hence $$\frac{\sin(\lambda + \iota)}{\sin(\lambda' + \iota)} = \frac{\sin\eta}{\sin\eta'}.$$

Now if the inversion has been accurate, according to definition, we should have

$$\lambda' = 180° - \lambda, \quad \text{and} \quad \eta' = 180° - \eta.$$

Therefore $$\frac{\sin(\lambda + \iota)}{\sin(\lambda' - \iota)} = 1.$$

a condition which, as ι may have all values from 0° to 90°, can only be satisfied by $\lambda = 90°$ or $\lambda = 270°$; that is, when the magnet is suspended with its magnetic axis horizontal. Such is the position which the magnetic axis is made to assume in practice, with more or less accuracy; and it therefore follows, that the magnet may remain in equilibrium after accurate inversion.

Inaccurate Inversion of a Magnet.

6. We have next to inquire what will be the errors occasioned by inaccurate inversion.

In fig. 1, let $\quad AB = a, \quad BAZ = \beta, \quad BZ = \psi_1, \quad AZB = \phi_1;$

and after inversion, let

$$b\,a\,Z = \beta_3, \qquad b\,Z = \psi_3, \qquad a\,Z\,b = \phi_3,$$

while $$a\,b = AB = a,$$

Also, as before, let

$$DN = \delta, \qquad DC = \delta_1, \qquad D\,c = \delta_3$$

Then the observed angles for the magnetic meridian are

$$\delta_1 = \delta + \phi_1, \qquad \delta_3 = \delta - \phi_3$$

From which

$$\delta = \tfrac{1}{2}(\delta_1 + \delta_3) - \tfrac{1}{2}(\phi_1 - \phi_3)$$
$$= \tfrac{1}{2}(\delta_1 + \delta_3) - \epsilon;$$

where the error committed by taking the mean of δ_1 and δ_3 for δ, is

$$\epsilon = \tfrac{1}{2}(\phi_1 - \phi_3).$$

Also in the triangles ABZ, $a\,b\,Z$, we have

$$\sin\phi_1 = \frac{\sin a \sin\beta_1}{\sin\psi_1}, \qquad \sin\phi_3 = \frac{\sin a \sin\beta_3}{\sin\psi_3};$$

from which equations, supposing the other angles to be known, ϕ_1, and ϕ_3, may be calculated, and ϵ, the correction to be applied to δ, may be obtained.

Method of ascertaining the relative positions of the Magnetic and Optical Axes in a Collimating Magnet.

7. It thus appears that we can calculate the error in the determination of magnetic declination due to imperfect inversion, provided we know the angles a, β, and ψ, both in the erect and inverted positions of the magnet.

The angle ψ can be ascertained with the utmost accuracy by direct observation, for it is the supplement of the zenith distance of the collimator-cross, as seen through the theodolite-telescope: but the ordinary methods of observation do not afford sufficient data for ascertaining the angles α and β. I have found, however, that these angles may be computed, provided we observe the magnet not only *erect* and *inverted*, that is when turned round its axis 0° and 180°, but also when turned round 90° and 270°.

8. In order to put this mode of observation in practice, I had a small collimating magnet constructed by Mr Adie, shown in fig. 3, consisting of a hollow steel cylinder 2·1 inches long, 0·5 inches in diameter, and about 0·04 inches thick, furnished at the end N with an achromatic lens of about 2 inches focal distance, and 0·3 inches aperture, and at C with a diaphragm carrying a cross of fine spider-lines. The diaphragm is supported by four screws, *s*, having their heads so deeply sunk below the surface of the cylinder as to be out of risk of disturbance; and the cell of the lens is made to screw into the cylinder, in order to adjust the cross-wires to its principal focus. The cylinder having been placed in temporary Ys, the lens was screwed in, until the wires were distinctly visible through the theodolite-telescope, which had previously been brought to sidereal focus; and then, by means of the screws *s*, the diaphragm was adjusted until its cross-wires continued nearly to intersect those of the theodolite-telescope, while the cylinder was turned round in the Ys. In this manner, the line of collimation was rendered nearly parallel to the axis of the cylinder, and thus also approximately parallel to the magnetic axis.

9. On the cylinder there is a tightly fitting brass ring A, having on its surface four lines, two of which, marked 1, 4, are shown in the figure; and the cylinder itself turns without much friction in the ring B, to which the torsion-fibre of silk, F, by which it is suspended, is attached. The lines on the ring A are situated 90° from each other, and are marked for reference 1, 2, 3, 4; and the ring B having also a line marked 0 upon it, with which the lines on A may be made successively to coincide, the cylinder may be suspended, either in its usual position, or turned through 90°, 180°, or 270°. The ring A was turned on the cylinder, until, when the lines 1 and 0 coincided, the wires of the collimator were sensibly horizontal and vertical,—a condition which was known to be fulfilled when, on looking through the theodolite-telescope, it was possible, by means of the tangent-screws, to make the vertical and horizontal wires of the theodolite cover those of the collimator.

In figures 4, 5, and 6, AB, CD are the wires of the theodolite-telescope, and *ef*, *gh* those of the magnet-collimator, represented, for the sake of simplicity, as they would appear when viewed through an erecting eye-piece. In figure 4, the magnet is in its usual or erect position, which is known to be the case when a small index *i*, projecting from the edge of the diaphragm, is seen *uppermost*. In fig. 5,

the magnet is also represented as in its erect position, but inaccurately so; and in fig. 6, it is represented as inaccurately inverted; while in both figures the intersections of the theodolite and magnet wires have been brought into optical coincidence, in order to read off the magnetic declination. Since the optical axis of the collimator coincides approximately with that of the cylinder we may assume that the magnetic axis is turned round the line OB, fig. 1; and if, in figs. 4, 5, 6, loe be an angle equal to the inclination of the planes AOB, BOZ, fig. 1, we may, since the angle AZB is small, assume that loe is sensibly equal to the inclination of the planes AOB, AOZ, or to β.* The angles Aoe, Boe will then, with sufficient accuracy, represent the errors in the values of β in the different positions of the magnet, and those angles may evidently be either estimated, or measured by means of a position-micrometer. The micrometer I have employed for this purpose consists of a piece of plate-glass, having on it a small circle about 0·25 inches in diameter, divided to every five degrees, by means of a fine diamond point. The glass is cemented with Canada-balsam to the flat surface of the field-lens of a RAMSDEN's eye-piece, and its thickness is such, that when the diaphragm-wires are brought to focus, the divided circle is so close to them as to be also very nearly in focus, so that its divisions are sufficiently well defined. The angle AEO can then be easily estimated to the nearest degree.

In the usual position of the magnet, fig. 5, let

$$\mathrm{AO}\,l = \beta_1, \qquad \mathrm{AO}\,e = \gamma_1, \qquad e\,o\,l = \beta,$$

Then

$$\beta_1 = \beta + \gamma_1.$$

Similarly, when the magnet is inverted, fig. 6, reckoning the angle AOl in the direction ACBD, let

$$\mathrm{AO}\,l = \beta_3, \qquad \mathrm{BO}\,e = \gamma_3,$$

Then

$$\beta_3 = 180^\circ + \beta + \gamma_3:$$

where, as the angles β_1 and β_3 are measured always in the direction ACBD, the angles γ_1 and γ_3 are to be reckoned positive or negative, according as they are measured in the same or in the opposite direction.

If, now, the magnet be inverted again and again, causing the lines 1 and 3 alternately to coincide with the line 0, and if, after each inversion, the cross-wires of the theodolite be made to intersect those of the collimator, we shall obtain the following series of readings:—

1*st*, From the verniers of the horizontal limb of the theodolite,

$$\delta_1 = \delta + \phi_1; \quad \text{and } \delta_3 = \delta - \phi_3:$$

2*dly*, From those of the vertical limb, the zenith distances of the collimator-cross,

$$180^\circ - \psi_1; \qquad 180^\circ - \psi_3.$$

* Supposing BAZ or $\beta = 45^\circ$, $\mathrm{AZ} = 90^\circ$, and $\mathrm{AZB} = 1^\circ$, the error in the value of β introduced by this supposition is only about 31″, which would not appreciably affect the subsequent calculations; and as AZB need not exceed 5′ or 10′, the error in β will generally be much smaller.

3dly, At each observation we may also register the values of the angles γ_1 and γ_3, the inclinations of the collimator-wires to those of the theodolite, as found by estimation or otherwise.

10. The readings δ_1 and δ_3 will be affected by changes in declination occurring during the observations; which, however, may be eliminated with sufficient accuracy, by combining each reading with the mean of the preceding and subsequent ones. At each observation, also, the values of β_1 or β_3 will be incorrect, owing to imperfect inversion, the errors being γ_1 and γ_3; but, as the latter angles may be expected to have sometimes positive and sometimes negative signs, the errors in the individual observations may be eliminated more or less completely, by taking the mean of a sufficient number of readings.

We shall thus obtain the angles

$$\phi_1 + \phi_3 = \delta_1 - \delta_3, \quad \psi_1 \text{ and } \psi_3; \text{ and with more or less accuracy,}$$
$$\beta_1 = \beta, \quad \text{and } \beta_3 = 180° + \beta.$$

Similarly, if the magnet be repeatedly inverted, with the lines 2 and 4 alternately coinciding with the line 0, we shall obtain, in like manner,

$$\phi_2 + \phi_4 = \delta_2 - \delta_4, \quad \psi_2 \text{ and } \psi_4,$$
$$\beta_2 = 90° + \beta \text{ nearly, and } \beta_4 = 270° + \beta \text{ nearly.}$$

If now, as before, we suppose the magnetic and optical axes, and a vertical line through the point of suspension, to meet a spherical surface in the points A, B, Z, we shall have, in fig. 7, $A_1 B_1 Z$, $A_2 B_2 Z$, $A_3 B_3 Z$, $A_4 B_4 Z$, for the positions of those points in the four positions of the magnet, and we shall obtain

$$\frac{\sin\phi_1}{\sin\beta_1} = \frac{\sin\alpha}{\sin\psi_1}; \qquad \frac{\sin\phi_3}{\sin\beta_3} = \frac{\sin\alpha}{\sin\psi_3} \text{ &c.}$$

from which, substituting the values of β_1, β_2, &c, in terms of β, we have

$$\frac{\sin\phi_1}{\sin\beta} = \frac{\sin\alpha}{\sin\psi_1}; \qquad \frac{\sin\phi_3}{\sin\beta} = \frac{\sin\alpha}{\sin\psi_3},$$
$$\frac{\sin\phi_2}{\cos\beta} = \frac{\sin\alpha}{\sin\psi_2} \qquad \frac{\sin\phi_4}{\cos\beta} = \frac{\sin\alpha}{\sin\psi_4}.$$

Whence

$$\sin\beta = \frac{\sin\phi_1 + \sin\phi}{\sin\psi_1 + \sin\psi_3} \cdot \frac{\sin\psi_1 \sin\psi_3}{\sin\alpha},$$

$$\cos\beta = \frac{\sin\phi_2 + \sin\phi_4}{\sin\psi_2 + \sin\psi_4} \cdot \frac{\sin\psi_2 \sin\psi_4}{\sin\alpha}.$$

Therefore

$$\tan\beta = \frac{\sin\frac{1}{2}(\phi_1+\phi_3)\cos\frac{1}{2}(\phi_1-\phi_3)\sin\frac{1}{2}(\psi_2+\psi_4)\cos\frac{1}{2}(\psi_2-\psi_4)\sin\psi_1\sin\psi_3}{\sin\frac{1}{2}(\phi_2+\phi_4)\cos\frac{1}{2}(\phi_2-\phi_4)\sin\frac{1}{2}(\psi_1+\psi_3)\cos\frac{1}{2}(\psi_1-\psi_3)\sin\psi_2\sin\psi_4}$$

and

$$\sin\alpha = \frac{\sin\frac{1}{2}(\phi_1+\phi_3)\cos\frac{1}{2}(\phi_1-\phi_3)\sin\psi_1\sin\psi_3}{\sin\frac{1}{2}(\psi_1+\psi_3)\cos\frac{1}{2}(\psi_1-\psi_3)\sin\beta}$$

11. In these formulæ the angles, in terms of which α and β are found, are all given by observation except $\phi_1 - \phi_3$ and $\phi_2 - \phi_4$. As, however, these angles are small, their cosines will differ little from unity: and since also ψ_1, ψ_2, &c., are all nearly equal to 90°, we may evidently, for an approximation, assume all the factors on the right hand, except $\sin \frac{1}{2}(\phi_1 + \phi_3)$ and $\sin \frac{1}{2}(\phi_2 + \phi_4)$ as equal to unity.

Then since

$$\phi_1 + \phi_3 = \delta_1 - \delta_3 \text{ and } \phi_2 + \phi_4 = \delta_2 - \delta_4,$$

we shall obtain

$$\tan \beta = \frac{\sin \frac{1}{2}(\delta_1 - \delta_3)}{\sin \frac{1}{2}(\delta_2 - \delta_4)},$$

$$\sin \alpha = \frac{\sin \frac{1}{2}(\delta_1 - \delta_3)}{\sin \beta}, \text{ or } \sin \alpha = \frac{\sin \frac{1}{2}(\delta_2 - \delta_4)}{\cos \beta}.$$

Formulæ for calculating the Errors occasioned by Imperfect Inversion of a Declinometer Magnet.

12. Having thus found approximate values of α and β, it will be seen, on referring to the equations of Art. 6, that we are in possession of all the data necessary for calculating, from those equations, ϕ_1 and ϕ_3 and hence ϵ, the correction to be applied to the observations of the imperfectly inverted magnet. If, however, formulæ for calculating ϵ directly be preferred, the following may be used:—

1. In the erect and inverted positions of the magnet, we have the equations

$$\frac{\sin \phi_1}{\sin \alpha} = \frac{\sin \beta_1}{\sin \psi_1}, \qquad \frac{\sin \phi_3}{\sin \alpha} = \frac{\sin \beta_3}{\sin \psi_3},$$

where ψ_1 ψ_3 are found by observation, also $\beta_1 = \beta + \gamma_1$, and $\beta_3 = \beta + \gamma_3$; γ_1 and γ_3 being found by observation, and α and β calculated by the formulæ of Art. 11.

Then remembering that

$$\phi_1 + \phi_3 = \delta_1 - \delta_3,$$

and putting

$$\cos \theta = \frac{\sin \alpha}{8 \sin \psi_1 \sin \psi_3 \cos \frac{1}{2}(\delta_1 - \delta_3)}$$

it may be shown that

$$\sin \epsilon = \cos(\theta + \psi_1 + \beta_3) - \cos(\theta - \psi_1 - \beta_3) + \cos(\theta - \psi_1 + \beta_3) - \cos(\theta + \psi_1 - \beta_3)$$
$$- \cos(\theta + \psi_3 + \beta_1) + \cos(\theta - \psi_3 - \beta_1) + \cos(\theta + \psi_3 - \beta_1) - \cos(\theta - \psi_3 + \beta_1)$$

13. More convenient formulæ, however, may be obtained by calculating the errors in the resulting value of the magnetic declination on the supposition that variations take place in the values of β and ψ separately.

1*st*, If errors occur in the values of β alone, we may suppose ψ to have its *correct* value, which, as shown in Art. 5, is 90°.

The equations of last article then become

$$\sin \phi_1 = \sin \alpha \sin \beta_1, \quad \sin \phi_3 = \sin \alpha \sin \beta_3$$

from which, if ϵ_1 be the value of the error in the observed declination expressed in seconds of arc, we have

$$\epsilon_1 = \frac{\sin\alpha \cos\frac{1}{2}(\beta_1 + \beta_3) \sin\frac{1}{2}(\beta_1 - \beta_3)}{\sin 1'' \cos\frac{1}{2}(\delta_1 - \delta_3)}.$$

2*dly*, If errors occur in the values of ψ alone.

Let $$\psi_1 = 90^\circ - \zeta_1, \qquad \psi_3 = 90^\circ - \zeta_3$$

Then supposing β to have its *correct* value

$$\sin\phi_1 = \frac{\sin\alpha \sin\beta}{\cos\zeta_1}, \qquad \sin\phi_3 = \frac{\sin\alpha \sin\beta}{\cos\zeta_3}$$

and putting ϵ_2 for the error in seconds in the value of the observed declination,

$$\epsilon_2 = \frac{\sin\alpha \sin\beta \sin\frac{1}{2}(\zeta_1 + \zeta_3) \sin\frac{1}{2}(\zeta_1 - \zeta_3)}{\sin 1'' \cos\zeta_1 \cos\zeta_3 \cos\frac{1}{2}(\delta_1 - \delta_3)}.$$

Finally, if ϵ be the error in declination arising from simultaneous errors in the values of β and ψ, since the whole error produced by imperfect inversion will generally be small, we shall have, with sufficient accuracy,

$$\epsilon = \epsilon_1 + \epsilon_2$$

14. To give some idea of the extent of the errors which may be expected to arise from inaccurate inversion, I will assume the case of a magnet which, when accurately inverted, gives a difference of 20′ in the theodolite readings; and also, that in the erect position, $\psi = 90^\circ$ and $\beta = 45^\circ$. The following table shows the errors in declination due to imperfect inversion of such a magnet, corresponding to the tabulated errors γ and ζ in the values of β and ψ, respectively, *after* inversion, these errors being supposed to occur separately.

γ	2°	4°	6°	8°	10°
ϵ_1	−10″·3	−20″·2	−29″·7	−38″·8	−47″·5

ζ	2°	4°	6°	8°	10°
ϵ_2	−0″·2	−0″·7	−1″·7	−2″·9	−4″·6

It thus appears that errors in β affect the accuracy of observations of declination much more than those in ψ, a result which might have been anticipated; and also, that an error in β of only 2° would, in the assumed case, cause an error in the observed declination exceeding 10″,—a quantity quite appreciable in such observations.

15. It was originally my intention to give an example of the application of the formulæ which have now been investigated, to the correction of an observation of *absolute* declination made by means of the magnet described in Arts. 8, and 9; but hitherto I have been unable, from want of leisure, to undertake such a series of observations as would be necessary to secure a trustworthy result. I will therefore merely state, in conclusion, what I conceive are the principal uses to which the formulæ may be applied.

1*st*, The greatest error likely to occur in inverting a declinometer magnet having been ascertained by a sufficient number of observations, the greatest corresponding error in the observed declination may be computed. This may be found to be so minute as to render it unnecessary to make allowance for such errors in future.

2*dly*, If the errors in the observed declination are found to be appreciable, the most convenient course seems to be, first, to find α and β by the formulæ of Art. 11; and then, by the formulæ of Art. 13, to calculate a table of errors. The corrections of the individual observations could then be at once obtained by inspecting the table.

3*dly*, The formulæ may be useful in indicating to the observer the proper instrumental adjustments for diminishing the errors caused by inaccurate inversion. It is evident, from the formulæ of Art. 13, that the errors ϵ_1 and ϵ_2 are least when α is least, and $\beta=90^\circ$. In the form of the magnet described in Arts. 8 and 9, it is obvious, that provided the position of the line of collimation referred to the magnetic axis be known, α may be diminished by means of the adjusting screws of the diaphragm, and β may be brought approximately to 90° by turning the ring A, fig. 3, through the proper angle.

XXIII.—*On a Problem in Combinations.* By The Rev. Philip Kelland, M.A., Professor of Mathematics in the University of Edinburgh.

(Read 3d December 1855.)

Several years ago, when discussing the question of the distribution of the stars, a problem occurred to Professor Forbes, which, simple as it is, appears to have escaped notice prior to that time. Having been consulted as to its solution, I communicated my results to Professor Forbes, who has inserted one of them in his paper printed in the Philosophical Magazine for 1850, vol. xxxvii., p. 425. But for the very ingenious application which Professor Forbes has there made of it, the problem might probably not be worth recurring to. As it is, I have thought it would not be altogether uninteresting to give the complete solution.

The Problem is as follows:—There are n dice, each of which has p faces, p being not less than n; it is required to find the number of arrangements which can be formed with them, 1°, So that no two show the same face; 2°, That no three show the same face; 3°, That no four do so, and so on.

1. The number of arrangements in which no two show the same face is easily seen to be the same as the number of permutations of the p faces, taken n together; and is therefore $p\,(p-1)\,(p-2)\,\ldots\,(p-n+1)$.

2. Remove the dice A and B, and cover the face 1 of the remainder. The number of arrangements which these can now form, omitting the covered face, and no two showing the same face, will be—

$$\begin{aligned}&(p-1)\,(p-2)\,\ldots\,(p-1-\overline{n-2}+1)\\ =&(p-1)\,(p-2)\,\ldots\,(p-n+2).\end{aligned}$$

Place with each of these the dice A and B, showing face 1, and you have the arrangements in which the dice A and B, and these alone, show face 1. The same applies to each of the other faces. Consequently there are $p\,(p-1)\,\ldots\,(p-n+2)$ arrangements in which the dice A and B, and these alone, show the same face. The same is true of every other pair of dice. Hence the number of arrangements in which two, and two only, show the same face, is—

$$n\,\frac{(n-1)}{1\,.\,2}\,p\,(p-1)\,\ldots\,(p-n+2).$$

3. Remove the dice A, B, C, D, and cover the faces 1 and 2 of the others. The number of arrangements which can now be formed in which no two show the same face is—

$$\begin{aligned}&(p-2)\,(p-3)\,\ldots\,(p-2-\overline{n-4}+1)\\ =&(p-2)\,(p-3)\,\ldots\,(p-n+3).\end{aligned}$$

By placing A and B showing face 1, and C and D showing face 2, with each of these, you have the arrangements in which two particular duplications of the dice A, B, C, D, and those only, occur. Now the number of arrangements of faces 1 and 2, on dice A, B, C, D, is the number of permutations, all together, of four things, two of one kind and two of another, or, $\frac{4.3.2.1}{1^2.2^2}$.

Hence $$\frac{4.3.2.1}{2^2}(p-2)(p-3)\ldots(p-n+3)$$

is the number of arrangements in which two duplications occur of faces 1 and 2, and on dice A, B, C, D. The same is true of any other pair of faces; consequently the number of arrangements in which two duplications are found on the dice A, B, C, D, but on no others, nor any repetition of the faces shown on these four dice, is—

$$\frac{p(p-1)}{1.2}\cdot\frac{4.3.2.1}{2^2}(p-2)(p-3)\ldots(p-n+3).$$

In like manner, any other four dice form the same number of arrangements; and hence the total number of arrangements in which two duplications and no more occur, is—

$$\frac{4.3.2.1}{2^2}\cdot\frac{n(n-1)(n-2)(n-3)}{1.2.3.4}\cdot\frac{1}{1.2}p(p-1)\ldots(p-n+3).$$

4. Similarly, the number of arrangements in which three duplications, and three only, occur without any other repetition, is,—

$$\frac{6.5.4.3.2.1}{2^3}\cdot\frac{n(n-1)\ldots(n-5)}{1.2\ldots6}\cdot\frac{1}{1.2.3}p(p-1)\ldots(p-n+4),$$

and the law of formation is evident.

5. We may now write the number of arrangements in which no triplication occurs, in the following form:—

$$p(p-1)\ldots(p-n+1)+\frac{n(n-1)}{1.2}p(p-1)\ldots(p-n+2)$$
$$+\frac{4.3.2.1}{2^2}\cdot\frac{n(n-1)\ldots(n-3)}{1.2.3.4}\cdot\frac{1}{1.2}p(p-1)\ldots(p-n+3)$$
$$+\frac{6.5\ldots1}{2^3}\cdot\frac{n(n-1)\ldots(n-5)}{1.2\ldots6}\cdot\frac{1}{1.2.3}p(p-1)\ldots(p-n+4)+\&c.$$
$$=p(p-1)\ldots(p-n+1)\left\{1+\frac{n(n-1)}{p-n+1}\cdot\frac{1}{2}+\right.$$
$$\left.\frac{n(n-1)\ldots(n-3)}{(p-n+1)(p-n+2)}\cdot\frac{1}{1.2}\cdot\frac{1}{2^2}+\frac{n(n-1)\ldots(n-5)}{(p-n+1)(p-n+2)(p-n+3)}\cdot\frac{1}{1.2.3}\cdot\frac{1}{2^3}+\&c.\right\}$$

6. This series may be exhibited as the solution of a differential equation, but it is doubtful whether, with our present knowledge, we can simplify its form. We obtain the differential equation thus:—

Let $$u=1+\frac{n(n-1)}{p-n+1}x+\frac{n(n-1)\ldots(n-3)}{(p-n+1)(p-n+2)}\cdot\frac{x^2}{1.2}+\&c.$$

Then $$\frac{du}{dx} = \frac{n(n-1)}{p-n+1} + \frac{n(n-1)\ldots(n-3)}{(p-n+1)(p-n+2)}x + \frac{n(n-1)\ldots(n-5)}{(p-n+1)\ldots(p-n+3)}\frac{1}{1.2}x^2 + \&c.$$

$$\therefore \quad \frac{d}{dx}\left(x^{p-n+1}\frac{du}{dx}\right) = n(n-1)x^{p-n} + \frac{n\ldots(n-3)}{(p-n+1)}x^{p-n+1} + \frac{n(n-1)\ldots(n-5)}{(p-n+1)(p-n+2)}\frac{x^{p-n+2}}{1.2} + \&c.$$

$$= n(n-1)z^{2n-2p} + \frac{n\ldots(n-3)}{p-n+1}z^{2n-2p-2} + \&c.$$

when $$x = \frac{1}{z^2}.$$

Hence $$z^{2p-n-2}\frac{d}{dx}\left(x^{p-n+1}\frac{du}{dx}\right) = \frac{d^2}{dz^2}\left(z^n u\right);$$

or $$z^{2p-n-2}\left\{z^{-2p+2n-2}\frac{d^2u}{dx^2} + (p-n+1)z^{-2p+2n}\frac{du}{dx}\right\}$$

$$= 4z^{n-6}\frac{d^2u}{dx^2} - (4n-6)z^{n-4}\frac{du}{dx} + n(n-1)z^{n-2}u;$$

whence $$(x-4x^2)\frac{d^2u}{dx^2} + \left\{p-n+1 + (4n-6)x\right\}\frac{du}{dx} - n(n-1)u = 0.$$

7. To find the number of arrangements in which no quadruplications occur, let us write the result of Art. 5 under the form C_p^n, being the total number of arrangements of n dice, each having p faces, in which no triplications occur. Remove the dice A, B, C, and cover the face 1 of the others. Then C_{p-1}^{n-3} is the number of arrangements of those dice in which no triplications occur; and, consequently, the number in which A, B, C, and those only, show face 1. Hence $p\,C_{p-1}^{n-3}$ is the number of arrangements in which a triplication is found on the first three dice, and on those alone. Consequently the number of arrangements in which one triplet only occurs is—

$$\frac{n(n-1)(n-2)}{1.2.3}\,p\,C_{p-1}^{n-3}.$$

8. Remove the dice A, B, C, D, E, F, and cover faces 1 and 2 of the others; C_{p-2}^{n-6} is the number of arrangements in which A, B, C show face 1, and D, E, F face 2.

Hence $$\frac{6.5.4.3.2.1}{1.2.3.1.2.3}\,C_{p-2}^{n-6}$$

is the number of arrangements in which A, B, C, D, E, F have faces 1 and 2 tripled; consequently

$$\frac{6.5.4.3.2.1}{(1\ 2.3)^2}\cdot\frac{p(p-1)}{1.2}\,C_{p-2}^{n-6}$$

is the number in which A, B, C, D, E, F form two triplets. Hence the number of arrangements in which there are two triplets, and no more, is—

$$\frac{n\,(n-1)\,.\,.\,.\,.\,(n-5)}{1\,.\,2\,.\,.\,.\,6}\,.\,\frac{6\,.\,5\,.\,.\,.\,.\,1}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C_{p-2}^{n-6}$$

$$=\frac{n\,(n-1)\,.\,.\,.\,.\,(n-5)}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C_{p-2}^{n-6}.$$

9. The whole number of arrangements in which no quadruplication occurs is—

$$C_p^n+\frac{n\,(n-1)\,(n-2)}{1\,.\,2\,.\,3}\,p\,C_{p-1}^{n-3}+\frac{n\,(n-1)\,.\,.\,.\,.\,(n-5)}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C_{p-2}^{n-6}$$

$$+\frac{n\,(n-1)\,.\,.\,.\,.\,(n-8)}{(1\,.\,2\,.\,3)^3}\,\frac{p\,(p-1)\,(p-2)}{1\,.\,2\,.\,3}\,C_{p-3}^{n-9}+\&c.$$

10. In the same manner it may be shown, that, if the above series be represented by D_p^n, the whole number of arrangements in which no quintuplications occur is—

$$D_p^n+\frac{n\,(n-1)\,(n-2)\,(n-3)}{1\,.\,2\,.\,3\,.\,4}\,p\,D_{p-1}^{n-4}+\frac{n\,(n-1)\,.\,.\,.\,.\,(n-7)}{(1\,.\,2\,.\,3\,.\,4)^2}\,.\,\frac{p\,(p-1)}{1\,.\,2}\,D_{p-2}^{n-8}+\&c.$$

11. It is evident that the total number of arrangements of the faces is p^n. Hence the probability that no two show the same face is—

$$\frac{p\,(p-1)\,.\,.\,.\,(p-n+1)}{p^n}.$$

XXIV. *On Solar Light, and on a Simple Photometer.* By Mungo Ponton, Esq., F.R.S.E.

(Read 4th March 1856.)

In approaching the subject of solar light, the first point is to endeavour to form an idea, not altogether indefinite, with respect to its quantity and intensity, as compared with some familiar standard of artificial flame. With this view were made, in the course of last summer, the observations now to be described.

After several trials, it was found, that the most convenient mode of procedure was first to compare a definite small surface, illuminated by solar light, with a like surface illuminated by the mere light of the sky, and then to compare the latter with a similar surface illuminated by the flame of a moderator lamp. The light of the sky thus affords a middle term between the extreme lights of the sun and the lamp, which are too diverse to be directly compared.

The first difficulty to be overcome, was that arising from the difference of colour between the flame of the lamp and the light of the sky. For this purpose, it was found necessary to employ light of only one colour; and blue light was selected, as that which could be most easily obtained pure.

Various methods having been tried, it was found that the blue rays could be obtained in sufficient purity, by taking the common blue paper used by haberdashers for packing their light goods, and steeping it in a concentrated solution of sulphate of copper, and then viewing this paper through common blue glass. This glass, it is well known, transmits only the blue rays and the extreme red; but the blue paper absorbs the extreme red, and disperses only the blue and a few yellow rays, which last are absorbed by the blue glass; so that, by this combination, only the blue rays reach the eye. In the light thus obtained, no rays save the blue could be detected by prismatic analysis.

To regulate with exactness the quantity of light admitted to the eye, a number of small slips of tea-lead were perforated with holes of various diameters, from $\frac{1}{10}$th down to $\frac{1}{100}$th of an inch. These apertures were carefully made, the rugged edges being removed, so as to present a clean circular outline. The diameter of each hole was exactly measured under the microscope, with a power of a hundred diameters.

The next point was to secure the exclusion from the eye of all extraneous light. For this purpose two pasteboard tubes were made, about eight inches long, and one inch in diameter, and lined inside with dull black paper. These were placed parallel to each other, and fastened together in such a manner that the centres

of their diameters were two-and-a-half inches apart, but with the means of slightly varying this distance, so as to bring the apertures exactly opposite the pupils of the two eyes. At the end of the tubes next the eye was an aperture of about a quarter of an inch in diameter, in which was inserted a piece of blue glass. Outside of these were placed slides, into which the perforated slips of lead might be introduced, in such a manner as to prevent their apertures from touching the surface of the paper, for fear of the entrance of minute fibres. At the ends of the tubes farthest from the eyes were simple apertures of about a quarter of an inch in diameter. To each of these farthest ends was attached, at the inner edge, a piece of card, which, after projecting outwards, about an inch and a half, was bent round at a right angle, so as to face the end of the tube. These pieces of card were covered on the outside with black paper, and on the inside with the prepared blue paper before described. The lower part of the card was attached to the lower end of the tube by a similar piece of card, also lined with the blue paper; and there was thus formed, at the further end of each tube, a small rectangular box, open at the top and the outer side, and lined throughout with the blue paper. To each of these boxes were closely fitted covers, which could be put on and removed at pleasure. The cross piece connecting the two tubes was fastened to a telescope stand, so as to place the tubes horizontally at the level of the eyes. When so adjusted, the surfaces destined to receive the light were vertical.

In Plate VIII., fig. 1, is shown a bird's-eye view of the two tubes PQ and RS; while XY is the cross piece by which they are attached, and on which they slide horizontally, so as to adjust their distance; $e e'$ are the ends next the eyes, and $c c$, $c' c'$ are the cards fastened to the farther ends, on which the light is to fall. Fig. 2 is a central cross-section, showing how the tubes are connected by the cross piece, and the latter with the telescope stand T. Fig. 3 shows the two ends next the eyes, with the perforated slips of lead inserted in the slides. Fig. 4 shows the farther end of the tube, prepared to receive the sun-light, with the direction of the rays $r r'$. Fig. 5 shows the farther end of the other tube, furnished with a small screen s to exclude the direct rays of the sun, and leave the receiving surface illuminated by the mere light of the sky. Fig. 6 shows the end of the sun-tube, disposed for the lamp; the ring l being the argand burner, surrounded by a screen $s s$ to confine the light. This screen was covered with black paper outside, and lined with white paper inside, so as to give the lamp-light the benefit of its reflection. There was another screen, not shown in the figure, placed over the argand, with a hole only sufficient to admit the glass chimney. By this arrangement, all light was excluded from the little box, except that of the lamp, whose flame was placed in such a position that the rays fell on the receiving surface at a horizontal angle of about 45 degrees, the surface of the flame being about two inches from the receiving surface.

When this instrument had both its receiving surfaces lighted by mere daylight, there were seen, on looking through it with both eyes, two equal round spots, one-fourth of an inch in diameter, of a very pure blue colour. If the apertures next the eyes were equal, these spots appeared of exactly the same tint and intensity; but if one of the apertures was a very little smaller than the other, the spot, viewed through the smaller aperture, appeared of a sensibly darker shade, in so much as to impress the eye with an idea of difference of colour. This peculiarity greatly aids the eye in judging when the two spots are of exactly equal brightness. It is, of course, necessary to take care that both apertures are smaller than the pupil of the eye.

In order to determine how far the results to be obtained by this instrument might be found to agree with those which may be obtained from the method of equal shadows, the instrument was first employed to compare the light of the moderator lamp with that of a wax candle (a short 6); and, after repeated trials, it was found that the results of the comparison, when made with this instrument, exactly agreed with those obtained by the method of equal shadows, the light of the lamp proving to be $3\frac{1}{2}$ times that of the candle. Indeed it is, if anything, easier to judge of the perfect equality in brightness of the two blue spots, than of the equal darkness of two shadows.

Several preliminary trials were next made, with a view to obtain a series of approximations to the relative sizes of the apertures to be employed. In making these observations, the observer should sit in an easy, half-reclining posture, with his back to the sun, so as to have the receiving surface exactly opposite to that luminary; his elbows should be rested steadily on the table; and his hands placed at the sides of his eyes, to screen them from extraneous light. Before looking into the instrument, the eyes should be closed for a little time, to render them more sensitive to the feeble blue rays; and care must be taken to have the two images simultaneously visible without effort, so as to admit of their exact comparison. For a good observation, a bright, cloudless, and perfectly calm day, should be chosen; and the sun should be at an altitude of about 45°, so as to give the mean brilliancy of sunshine.

Thursday the 10th of August 1855 having proved entirely suited for the purpose, advantage was taken of it, to make the decisive observations, which were commenced about 11 A.M., and occupied between 15 and 20 minutes.

The diameter of the aperture fitted to the sky-tube was 0·083 inch. The sun-tube was tried first with an aperture of 0·015, through which the sun-lit surface appeared a little brighter than that lighted by the sky. It was next tried with an aperture of 0·01375, and then the sky-lit surface appeared the brighter of the two. Lastly, there was applied to the sun-tube an aperture of 0·014375, and then no appreciable difference could be detected by the most steady gaze. These three apertures were changed several times, and always with the same results.

The little box at the end of the sun-tube had now its cover put on, so as to leave open only its right-hand side, to which the lamp was approached, and disposed in the manner before described. The aperture applied to this tube was now 0·1, and, after trying several approximate apertures for the sky-tube, one of 0·0275 was ultimately fixed upon, as that which rendered the two images of exactly equal brightness.

These observations were subsequently repeated, with the same results; and, on each occasion, the apertures were, after the observations, examined under the microscope and ascertained to be clear. These results may therefore be regarded as a fair approximation to the truth.

The brightness of the images being inversely as the areas of the apertures, it follows from the first observation, that a small surface illuminated by the direct rays of the sun at an altitude of 45° is 33·6 times brighter than a similar surface illuminated by diffuse day-light; while, from the second observation, it follows that a like surface illuminated by the flame of a moderator lamp, at the distance of 2 inches, and placed obliquely so that the rays might fall as nearly as possible at a horizontal angle of 45°, is 13·2 times less bright than a similar surface illuminated by diffuse day-light. Hence, the same surface when lighted by the sun is 444 times brighter than when lighted by the lamp, under the above circumstances, the blue rays only being used in each case. This exclusion of all but the blue rays, is somewhat adverse to the artificial light, which has an excess of red and yellow rays, beyond what is required for the composition of white light; but the blue rays may be held to indicate the proportion of white light, contained in the artificial flame.

As, from the preliminary observation, it was found, that the moderator lamp employed was 3·5 times brighter than a wax candle (short 6 in the lb.), it follows, that a small surface, illuminated by mean sunshine, is 1554, or say 1560 times brighter than is the same surface when lit by such a wax candle placed at 2 inches from it, in an oblique direction.

Now, it is found not difficult to raise the electric light to such a pitch of intensity as to afford a light equal to that of 520 wax candles; so that, if the moderator lamp were replaced by three such electric lights, the surface would be equally bright as when illuminated by mean sunshine.

To form a conception, therefore, of the quantity and intensity of the light emanating from the sun, when it reaches a distance of 95 millions of miles from his centre, we may imagine the surface of a sphere, having that distance for its radius, to be covered all over with a very thin film, say $\frac{1}{1000}$th of an inch in thickness, having a brightness equal to that of an electric light of the above-mentioned intensity, and that behind this there are two similar films of equal brilliancy, the three forming a thin stratum, say $\frac{1}{40}$th of an inch in thickness; then such a stratum would represent the brilliancy of the sun's light at the earth's orbit.

Now, let us imagine this stratum to be transferred to the surface of the sun. It would there be spread over 46,275 times less area; consequently, its thickness would be increased that number of times, and would therefore amount to about 1132 inches, or about 94 feet, embracing 138,825 layers of flame, equal in brightness to an electric light of the above-mentioned intensity, and it would at its outer surface possess a brilliancy equal to that of the surface of the sun.

It is evident, however, that there might be a very considerable addition made to the thickness of such a stratum, without affecting, in any appreciable degree, its proportion to the planetary distances. If then, the thickness of the stratum were increased 520 times, making it 49,000 feet, then it might embrace 72,000,000 layers, each of them having an individual brilliancy not greater than that of a wax candle. The real thickness of the stratum in which the luminous property of the sun resides, may be very considerably greater than the above estimate, which is somewhat over 9 miles; and the luminosity of each individual film composing the stratum may be very considerably less, without affecting the general result.

Mr. Swan's observations of the Carbohydrogen Spectrum.

FIG. 1

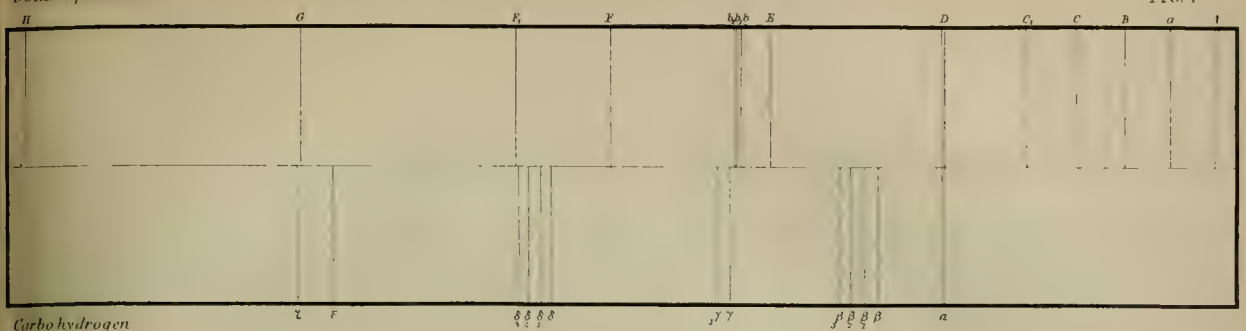


FIG. 2

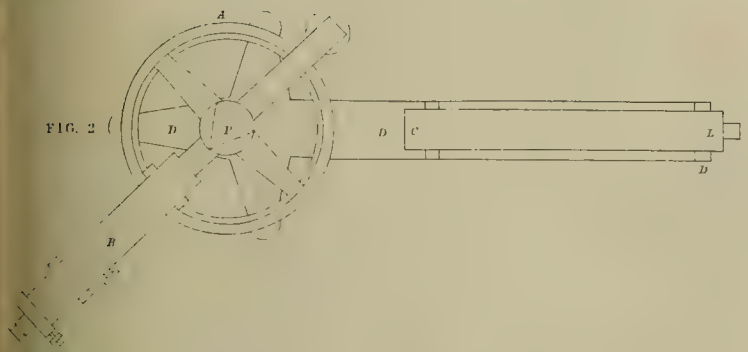


FIG 3

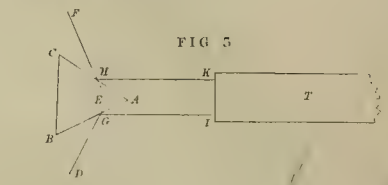
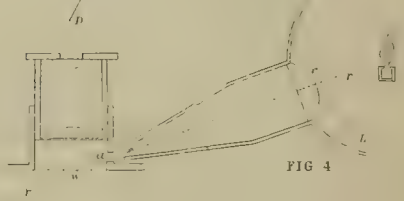


FIG 4



Mr. PONTON'S PHOTOMETER.

FIG. 1 ½ size

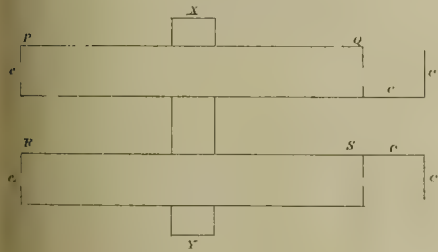


FIG. 2 ½ size

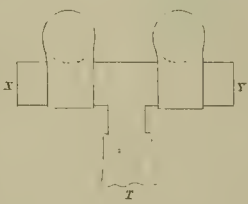


FIG. 3

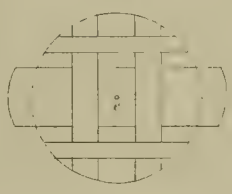


FIG. 3

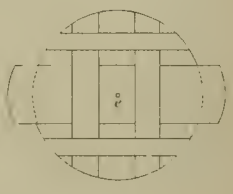


FIG. 6

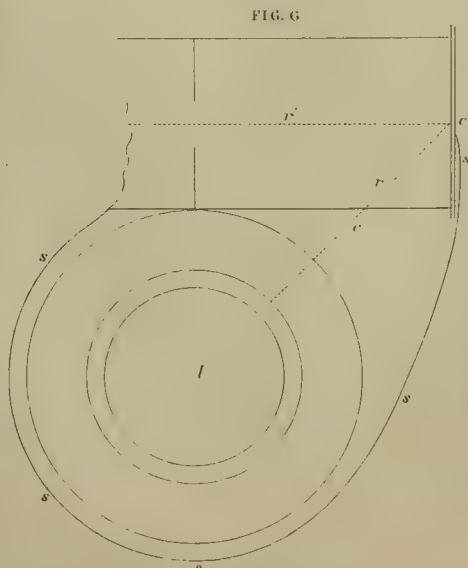


FIG. 4

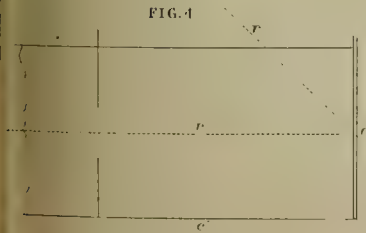
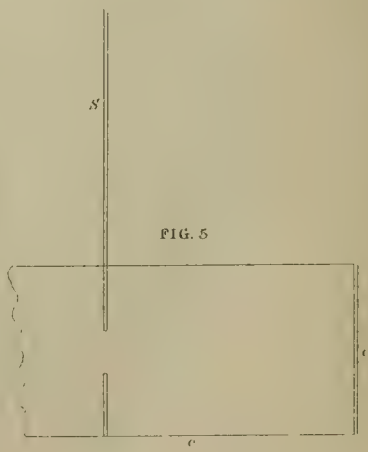


FIG. 5



W. & A.K. Johnston, Edinburgh.

XXV.—*On the Possibility of combining two or more Probabilities of the same Event, so as to form one Definite Probability.* By the Right Rev. Bishop TERROT.

(Read 17th March 1856.)

(1.) The inquiry which, with its results, I propose to lay before the Society, was suggested by the following passage in the very popular Treatise on Logic by Dr WHATELY, now Archbishop of Dublin.

"As in the case of two probable premises, the conclusion is not established except upon the supposition of their being *both* true, so in the case of two (and the like holds good with any number) distinct and independent indications of the truth of some proposition, unless *both* of them *fail*, the proposition must be true: we therefore multiply together the fractions indicating the probability of the failure of each—the chances against it—and the result being the total chances against the establishment of the conclusion by these arguments, this fraction being deducted from unity, the remainder gives the probability *for* it.

"*E. g.* A certain book is conjectured to be by such and such an author, partly, 1*st*, from its resemblance in style to his known works; partly, 2*d*, from its being attributed to him by some one likely to be pretty well informed. Let the probability of the conclusion, as deduced from one of these arguments by itself, be supposed $\frac{2}{5}$, and in the other case $\frac{3}{7}$; then the *opposite* probabilities will be respectively $\frac{3}{5}$ and $\frac{4}{7}$, which multiplied together give $\frac{12}{35}$ as the probability against the conclusion; *i. e.*, the chance that the work may *not* be his, notwithstanding the reasons for believing that it is; and, consequently, the probability in *favour* of the conclusion will be $\frac{23}{35}$, or nearly $\frac{2}{3}$" (WHATELY's *Logic*, 8th Ed., p. 211.)

(2.) Now, this reasoning appears to me erroneous, because it can be so applied as to bring out two inconsistent conclusions. It must be observed, that there is no such generic difference between the chances *for* and *against* the truth of a proposition, as can require or justify any difference in the laws and methods applied to them. A negative can always be turned into an affirmative by a change of verbal expression, without any change of meaning. Thus the chance of not hitting a mark is the same as the chance of missing it. The chance of a life not falling before sixty, is the chance of its continuance up to sixty. The chance that A was not the author of the book, is the chance that some one else was the author.

Let us then take as the proposition whose probability is to be found, the negative —he did not write it—the partial probabiilties *for* which are by the data $\frac{3}{5}$ and $\frac{4}{7}$.

The opposite probabilities are now $\frac{2}{5}$ and $\frac{3}{7}$, and their product is $\frac{6}{35}$, the probability *against* the conclusion whose probability we are now seeking. Consequently, $1-\frac{6}{35}=\frac{29}{35}$ is the probability *for* our conclusion, namely, that he did not write the book. But by the former calculation, the probability of the same conclusion was found to be $\frac{12}{35}$: and, as these incompatible results follow from the same principle and method, the principle and method must be erroneous.

(3.) The only mathematical attempt at the solution of this problem which I have met with, is at section 15 of the Article Probability, in the *Encyclopædia Metropolitana*. It is given there as follows:—

"It is an even chance that A is B, and the same that B is C; and, therefore, 1 to 3 on these grounds alone, that A is C. But other considerations of themselves give an even chance that A is C. What is the resulting degree of evidence (or the probability) that A is C?" There is a previous solution which I omit, and then the passage proceeds as follows:—"Let us now treat the preceding question as having two contingencies, the compound argument 1 to 3 *for*, and the independent evidence an even chance. We have, therefore, four possible cases.

	Prob. A is C.
"Argument and Evidence both true, . .	$\frac{1}{4}\times\frac{1}{2}=\frac{1}{8}$
Argument false, Evidence true, . . .	$\frac{3}{4}\times\frac{1}{2}=\frac{3}{8}$
Argument true, Evidence false, . . .	$\frac{1}{4}\times\frac{1}{2}=\frac{1}{8}$
Argument and Evidence both false, . .	$=0$

"The sum of these is $\frac{5}{8}$ as before (for the resulting probability that A is C). The above generalized is as follows:—Let a and $(1-a)$ be the probabilities for and against the argument (the conclusion from the argument); and ϵ and $(1-\epsilon)$ be the probabilities from any other source. Then the chance that both are wrong is $(1-a)\,.\,(1-\epsilon)$, and of the contradictory, namely, that (A is C) follows from the one or the other, is $1-(1-a)\,.\,(1-\epsilon)=a+\epsilon-a\,\epsilon$."

This is the formula adopted by Whately; and it is open to the same objection, namely, that by applying it we can arrive at two contradictory conclusions. But, further than this, what is the meaning of *Argument true, Evidence true?* The argument and the evidence are here treated as two independent events having respectively the probabilities of $\frac{1}{4}$ and $\frac{1}{2}$; and their coincidence is represented by $\frac{1}{8}$. But nothing corresponding to this goes forward in the mind. The argument merely affords the information, that for every reason for believing that A is C, there are three equivalent reasons for believing that A is not C. This information we

are supposed to believe absolutely; there is no question as to the probability of its truth, or the possibility of its falsehood. The only matter in question is whether A is C, or is not C.

The falsity of the expression $a+\epsilon-a\,\epsilon$ will be evident, if we give to a and ϵ the values $\frac{2}{7}$ and $\frac{3}{7}$. Then $a+\epsilon-a\,\epsilon=\frac{2}{7}+\frac{3}{7}-\frac{6}{49}=\frac{1}{2}+\frac{10}{49}$. That is to say, while each of the independent probabilities is less than $\frac{1}{2}$, and, therefore, in favour of the negative, their compound force is much above $\frac{1}{2}$; and, therefore, in favour of the affirmative. If then we found from internal evidence and external evidence severally, that the chances were against the truth of the proposition A is C, we ought to conclude from their united force, that the chances are in favour of the proposition. But the human mind is incapable of coming to such a conclusion.

It may be well to notice in passing, that the problem under consideration is altogether different from that of finding the compound force of two identical assertions made by two witnesses, whose veracity, that is, the probability of their speaking truth, is expressed by a and ϵ. In that problem, we possess among the data the fact, that each witness *makes the same assertion.* But in the problem we have been considering, there is no such *assertion.* Neither the argument nor the evidence assert or deny that A is C. What they give as data, is merely that the reasons for believing that A is C, are in a given ratio to those for believing that A is not C. And as the data of the two problems are of totally different character, the methods to be applied must of course be different. I have mentioned this, because some good mathematicians whom I have consulted, were at first disposed to consider $\frac{a\,\epsilon}{a\,\epsilon+\overline{1-a}\,.\,\overline{1-\epsilon}}$ as the proper expression for the conjoined force of the argument and evidence.

(4.) Let us now consider the Problem under the following form. *A, whose veracity is undoubted, states that, from his knowledge of the facts of the case, the probability of the event* E *is* $\frac{p}{q}$. *B, under the same conditions, states, that it is* $\frac{r}{s}$. *Supposing the facts known by each to be altogether distinct, what is the proper measure of the expectation formed in a third mind by these two statements?*

(5.) Before attempting to show how a solution of the problem ought to be sought, it may be well to observe, that the mind cannot admit two probabilities of the same event as co-existent probabilities. Thus, if A tells me that the probability of rain to-morrow is $\frac{2}{5}$, and B that it is $\frac{4}{7}$, I cannot admit both of these as probabilities; for that would be equivalent to believing on the authority of A, that it is *less* likely to rain than not, and at the same time to believe on the authority of B, that it is *more* likely to rain than not.

What really takes place is this. The two fractions are received as indications

of the effects reasonably produced upon the minds of the informants, by the knowledge of certain facts which they have not communicated to us. The fractions which they give are admitted as true exponents of the results of their respective partial knowledge, no doubt resting either upon their veracity, or upon the accuracy of their inferences. We admit, that each states the probability as he ought, under *his* circumstances: and the question is, how ought we to state it under *our* circumstances, knowing as we do something *more*, and also something *less* than either of our informants.

(6.) In attempting to answer this question, I shall have recourse to the ordinary illustration of an urn and balls. Let us suppose that A has seen p white and $q-p$ black balls introduced into an urn, which he believes to have been previously empty. He properly infers that the probability of drawing a white is $\frac{p}{q}$. B, under the same circumstances, has seen r white, and $s-r$ black balls introduced, and infers that the probability of drawing a white is $\frac{r}{s}$. If they communicate to each other only their *inferences*, there is an apparent contradiction, and no combination or agreement can take place. But if they communicate the *facts* from which the inferences were deduced, then each knows that the urn contains $p+r$ white, and $q+s-p-r$ black balls, and agree in making the probability of drawing a white $\frac{p+r}{q+s}$. If the number of balls whose introduction has been seen by the two observers be equal, then $\frac{p+r}{q+s}=\frac{p+r}{2q}=\frac{1}{2}\left(\frac{p}{q}+\frac{r}{q}\right)=\frac{1}{2}$, the sum of the several probabilities.

It may be observed that $\frac{p+r}{q+s}$, as the expression of the combined probabilities $\frac{p}{r}$ and $\frac{q}{s}$, is not exposed to the objection of admitting contradictory results, for, if we take the negative as the conclusion whose probability is to be found, then A gives for the probability of the conclusion $1-\frac{p}{q}=\frac{q-p}{q}$, while B gives $1-\frac{r}{s}=\frac{s-r}{s}$, therefore the combined probability against the event is $\frac{q-p+s-r}{q+s}$. But the combined probability for the event was $\frac{p+r}{q+s}$, and $\frac{p+r}{q+s}+\frac{q-p+s-r}{q+s}=\frac{q+s}{q+s}=1$, as it ought to be.

(7.) But what we have to consider, is the impression made upon the mind of a third person, who is informed by A that, from his observation, the probability of drawing a white is $\frac{p}{q}$, and by B that, from his observation, it is $\frac{r}{s}$, and to whom no farther information is given, except that the observations were totally distinct. Now, as these data give only the ratio of white to black balls at each introduction, there may have been, in the first, p white and $q-p$ black, or there

may have been np white and $nq-np$ black, where n is any whole number from one to infinity. In like manner, the second may have consisted of nr white and $ns-nr$ black, where n is any number from one to infinity. Any one assumed state of the first introduction may have co-existed with any assumed state of the second; and thus assuming that the first contained p white and $q-p$ black, we have the infinite series of probabilities,

$$\frac{p+r}{q+s-p-r}, \frac{p+2r}{q+2s-p-2r} \cdots\cdots \frac{p+nr}{q+ns-p-nr}.$$

Again, assuming that the first contained $2p$ white, and $2q-2p$ black, we have

$$\frac{2p+r}{2q+s-2p-r}, \frac{2p+2r}{2q+2s-2p-2r} \cdots\cdots \frac{2p+nr}{2q+ns-2p-nr},$$

and so on *ad infinitum*.

This infinite series of infinite series I cannot sum. If they can be summed, then their sum divided by the infinite of the second order n^2, is the probability required.

In no case, except when $\frac{p}{q}=\frac{r}{s}$, so far as I see, can the sum of their sums, or the whole probability, be determinately expressed. When $\frac{p}{q}=\frac{r}{s}$, the fractions being in their lowest terms, $p=r$ and $q=s$. The two pieces of information are then identical; the same information is given by both observers; and the information, unaffected by the repetition, is absolutely received by the third party: and this is the result, if, in the foregoing series, we substitute p for r and q for s.

(8.) If we revert to the expression (3) given in the *Encyclopædia Metropolitana*, where the separate probabilities are a and ϵ, and their conjoint force is stated to be $a+\epsilon-a\epsilon$, it would follow that the effect produced by two observers making the same statement as to the probability of an event should be twice the asserted probability *minus* its square. Now, in the case of a repetition of the same probability by two observers, it must, I think, be allowed that my result is conformable to that of which we are all conscious. If, for example, the Northampton and the Carlisle Tables both give $\frac{1}{2}$ as the probability that a man of thirty will live to the age of fifty, and are both implicitly believed, we believe that there is an even chance of his living to fifty, and not, as would follow from the expression given in the Encyclopædia, that the chances are three to one in his favour.

(9.) It perhaps deserves to be noticed, that when a second series of observations or experiments is added to one previously admitted, the probability is not increased by the mere preponderance of favourable over unfavourable cases in the second series. To increase the probability, the ratio of favourable to unfavourable cases must be greater in the second series than in the first. For the first received probability is $\frac{p}{q}$, and the composite is $\frac{p+r}{q+s}$. (6.)

$$\text{Now } \frac{p+r}{q+s} > \frac{p}{q}, \text{ when } rq > sp, \text{ or } rq-rp > sp-rp,$$

$$\text{or } \quad r(q-p) > p\,.\,(s-r), \text{ or } \frac{r}{s-r} > \frac{p}{q-p}\,.$$

When the ratios only are given, any conceivable case of the grounds upon which the probabilities are given may be represented by mp, mq, nr, and ns. Hence the original probability is $\frac{mp}{mq}$, the composite is $\frac{mp+nr}{mq+ns}$, and this is greater than $\frac{mp}{mq}$, when

$$m^2pq+mnqr > m^2pq+mnps$$
$$\text{or } rq > ps, \text{ as before.}$$

(10.) Valid objections may, I think, be made to the last paragraph of the section in the Encyclopædia already referred to. As this is not long, I quote it entire. "The following theorem will be readily admitted on its own evidence. *If any assertion appear neither likely nor unlikely in itself, then any logical argument in its favour, however weak the premises, makes it in some degree more likely than not.* In the manner in which writers on Logic apply the calculus of probabilities, this is never the consequence of their suppositions. For what we have called a is their resulting probability of the argument. Suppose, for instance, a writer on logic presumed that the argument from analogy gave $\frac{3}{10}$ to the probability that there is vegetation in the planets, which must be regarded as a thing neither likely nor unlikely in regard of evidence from any other source, he would take $\frac{3}{10}$ to be the probability of this result, that is, *less after an argument in its favour than it was before.* We substitute $\frac{1}{2}+\frac{1}{2}\,.\,\frac{3}{10}=\frac{13}{20}$."

This numerical equation is the value of the expression $(a+\epsilon-a\,\epsilon)$, when $a=\frac{1}{2}$ $\epsilon=\frac{3}{10}$. I have already shown that this expression does not truly represent the composite force of the two probabilities a and ϵ. But farther than this, the argument from analogy, giving $\frac{3}{10}$ as the probability of the affirmative, is an argument, not *in favour of*, but *against* the proposition that there is vegetation in the planets. It implies that for every three reasons for believing that there *is*, there are seven for believing that there *is not;* and, consequently, the effect of the argument ought to be to diminish our disposition to believe the proposition, or, in other words, to diminish its probability.

(11.) But it may be worth while to examine whether the fraction $\frac{1}{2}$ be, after all, a true available expression for the probability of an event, which is neither likely nor unlikely to happen, or to have happened, there being no evidence, no reasons for belief, either for or against it.

Probability, as Mr Boole in his *Laws of Thought*, properly defines it, is "Expectation founded upon partial knowledge." Events, therefore, of which we possess *complete* knowledge, and events of which we possess *no* knowledge, are equally, by the terms of the definition, excluded from the class of probable events, that is to say, of events to which the calculus of probabilities can be applied. If we are *certain* that an event has happened, we totally neglect and are unaffected by any subsequent information, which, but for that certainty, would have given to the event a definite probability expressed by a proper fraction; and never think of looking for a form by which to combine such fraction with the unit expressing the certainty. If, again, we derive from experience or observation a definite probability of any event, such, for example, as the probability for drawing a white ball from an urn, whose contents are given; namely, the fraction whose numerator is the number of white balls, and its denominator the total number of balls contained, we never think of combining this with the $\frac{1}{2}$ which is assumed as the probability when nothing whatever was known, except that the ball drawn must be either white or not white. *Complete* knowledge comprehends all previous partial knowledge; and, therefore, all fractional expressions for probability derived from the latter, are virtually contained in the unit, which is the expression adopted for the certainty produced by the former. On the other hand, *partial* knowledge destroys total ignorance, and any inference that may be drawn from it. It comprehends the hypothesis that the event may, and that it may not happen, with a definite probability to each, which do not supplement but supersede the probabilities of $\frac{1}{2}$ previously assumed for each. I cannot conclude without suggesting a doubt, whether $\frac{1}{2}$ be at any time the proper expression for the probability of an event which is "neither likely nor unlikely in regard of evidence."

It seems more analogous with the practice in other cases to express such probability by the indefinite fraction $\frac{0}{0}$. If this expression be applied to either of the probabilities constituting the compound probability $\frac{p+r}{q+s}$, the compound probability will be reduced to the remaining simple probability, for $\frac{0+r}{0+s}=\frac{r}{s}$. And this agrees with the necessary action of the mind, which takes no note of its original ignorance, after it has arrived at a definite probability from partial knowledge.

(12.) Hitherto, I have been speaking of the combined result of two probabilities of the same event, derived from distinct sources of partial knowledge; and I have shown that to obtain a definite result, the mere ratio in such case is insufficient, and that the actual number of favourable and unfavourable cases in each of the data is requisite.

But when the given probabilities are of different events, and the quæsitum is the probability of their joint occurrence, the ratio alone is sufficient, because as factors $\frac{m\,p}{m\,q}$ and $\frac{n\,p}{n\,q}$ give the same results.

(13.) To sum up the propositions proved in the foregoing paper:—

1. If the *ratio only* of equally probable cases, in two or more probabilities for the same event be given, no definite probability can be derived from their composition. (7.)

2. If the two given probabilities $\frac{p}{q}$, $\frac{r}{s}$, indicate not merely the *ratio*, but also the *actual number* of favourable and unfavourable hypotheses or cases, their conjoined force is properly expressed by $\frac{p+r}{q+s}$. (6.)

3. Under both of these conditions, the second given probability increases or diminishes the force of the first, according as the fraction expressing the second is greater or less than that expressing the first. When the ratios only are given, then the extent of increase or diminution is indefinite. When the actual numbers are given, it is definite. (9.)

4. The *a priori* probability derived from absolute ignorance has no effect upon the force of a subsequently admitted probability. (11, 12.)

XXVI.—*Researches on Chinoline and its Homologues.* By C. Greville Williams, Assistant to Dr Anderson, University of Glasgow.

(Read 7th April 1856.)

Twenty-two years have now elapsed, since Runge first published his remarkable experiments on coal naphtha,* and it would, perhaps, be difficult to instance any chemical investigation which has formed the point of departure of a greater number of researches. When we consider the vast quantity of bodies which have, first and last, been obtained from coal-tar, it might appear that little more remained to be done,—that the mine was exhausted,—but so far from this being the case, the discovery of one substance has only served to pave the way for the isolation of others.

Among the bodies examined by Runge, there was one which apparently possessed comparatively few features of interest; indeed its very name (the first syllable derived from λευκός) was intended to express its supposed inability to produce coloured reactions, a feature which, in the chemistry of the time, militated greatly against its claims to notice. I have used the expression "supposed inability," because I shall show further on, that this substance is capable, under certain conditions, of affording extremely brilliant colorations. Eventually, Gerhardt,† by acting on quinine, cinchonine, and strychnine, with hydrate of potash, obtained the same body. The first chemist who succeeded in procuring any of its compounds in a state of tolerable purity was Hofmann, whose analysis of the platinum salt is very nearly exact. But, at the time of that analysis, he was of opinion that the products obtained from coal and chinoline were essentially different, an opinion which he subsequently retracted. In the mean time, the alkaloid, as obtained from cinchonine was examined by Bromeis‡ and Laurent,§ their results, however, not elucidating the composition of the basic fluid obtained in the manner alluded to.

Some time since, I undertook the examination of the bases produced by destructive distillation of the bituminous shale of Dorsetshire, and found them to be identical with those from bone-oil.‖ I now began to see the great probability that all processes of destructive distillation of nitrogenous matter at very elevated

* Poggend. Annal., Bd. xxxi., p. 65 und 513; und Bd. xxxii., p. 308 und 328.
† Revue Scientif., x., 186. Compt. Rend. des Trav. de Chim. 1845, p. 30.
‡ Liebig's Annal., Bd. lii., p. 130; and Ann. der Chem. u. Pharm. lii., 130.
§ Ann. de Chim. et de Phys. [3] xxx., 368.
‖ Quart. Jour. Chem. Soc. Lond., July 1854.

temperatures would result in the formation of the same classes of alkaloids, and subsequent researches* have only tended to confirm this view. In a little paper, "On some of the basic constituents of Coal-Naphtha, and on Chrysène,"† I have given a table, showing the extraordinary similarity of the basic products derived by dry distillation from DIPPEL's oil, coal, the Dorset shale, and cinchonine. The last of these researches was undertaken in the endeavour to throw light upon the discrepancies in the results of the chemists who had previously examined chinoline, the experiments being embodied in a paper which appeared in the Transactions of the Society last year. In that communication,‡ I showed that the fluid usually known as chinoline, and supposed to have the formula C_{18} H_7 N, had, in fact, a very complex constitution, and contained in addition to that base, six others. As my chief object at that time, was to demonstrate the real nature of the decomposition which cinchonine undergoes at an elevated temperature in the presence of alkalies, I did not make a minute examination of the chinoline itself, as I conceived it to be sufficient for the purposes of that investigation to show that a base of the formula C_{18} H_7 N did really exist in the fluid. This fact was by no means a matter of course, for the analyses of the chinoline from cinchonine previously published were so conflicting, that it was a difficult matter to derive a formula from them. HOFMANN's analyses were made upon a product from coal-tar, and the formula he gave as the expression of his results, was C_{18} H_8 N. But as an even number of atoms of hydrogen in a body containing an equivalent of nitrogen, was incompatible with views now almost universally received of the constitution of organic bodies, C_{18} H_7 N was taken by most chemists as the true formula of the base from coal-tar. But the wide differences in the analyses of the chinoline obtained by distilling cinchonine with potash, induced GERHARDT § to express doubts, as to whether C_{18} H_7 N, or C_{20} H_9 N was the correct formula, although he appears to lean towards the latter, for he places it at the head of the section, but, nevertheless, shows that the formula is open to doubt, by annexing a note of interrogation to it. I have shown the cause of the variable nature of the results obtained by other experimenters, and have proved the existence of a homologous series, of which, until I commenced this investigation, only one member was known.

Many circumstances conspire to render a detailed examination of chinoline a problem of interest, for, perhaps, no other body, known for an equal length of time, and investigated by so many hands, is so erroneously described in the manuals of organic chemistry. In fact, there are few things stated regarding it, that are not more or less incorrect.

* I take this opportunity of expressing my sense of Dr ANDERSON's kindness, in permitting me to make use of his laboratory and apparatus, during my endeavours to realize this idea.

† Chem. Gazette, Nov. 1, 1855; and Edin. Phil. Jour., Oct. 1855.

‡ Trans. Roy. Soc. Edin., vol. xxi., part ii.

§ Traité de Chimie Organique, troisième partie, p. 148.

Chinoline has, however, been invested with an artificial interest, from a supposed intimate connexion between it and quinine, and an equally supposititious parallelism between the action of heat upon the last named alkaloid, and upon the hydrated oxide of tetramethylammonium, while the real points of attraction which it possesses have been neglected, or supposed not to exist.

The first incorrect idea of the connexion between chinoline and quinine, may be very briefly disposed of. It was founded upon the supposition that chinoline was the sole product of the action of hydrate of potash, at a high temperature, upon quinine. In this manner, it was easy to construct an equation by which it was made to appear, that quinine, *minus* a certain number of equivalents of carbon, hydrogen, and oxygen, yielded chinoline.

Another supposed connexion between the two alkaloids was a very beautiful one, and one that, at the time when $C_{18}\ H_8\ N$ was the received formula for chinoline, could scarcely have failed to suggest itself to the eminent chemist, whose particular train of research led him to examine the action of iodide of methyl upon the natural and artificial alkaloids. It is well known, that iodide of tetrethylammonium, by treatment with oxide of silver, yields hydrated oxide of that base, which is rendered obvious, by a glance at the following equation :—

$$N\left\{\begin{matrix}C_4\ H_5\\C_4\ H_5\\C_4\ H_5\\C_4\ H_5\end{matrix}\right\}I + Ag\ O + HO = N\left\{\begin{matrix}C_4\ H_5\\C_4\ H_5\\C_4\ H_5\\C_4\ H_5\end{matrix}\right\}O, HO + Ag\ I;$$

and if we follow out the same equation, substituting iodide of methyl-chinoline-ammonium,* for iodide of tetrethylammonium, we find that at first sight,

$$\underbrace{C_{20}\ H_{11}\ N, I}_{\text{Iodide of Methyl-chinolineammonium.}} + Ag\ O + HO = \underbrace{C_{20}\ H_{12}\ NO_2}_{\text{Quinine.}} + Ag\ I$$

appears a reaction likely to take place;† unfortunately, however, there are two reasons why it is impossible, the first being, that iodide of methyl-chinoline-ammonium is represented by $C_{20}\ H_{10}\ N, I$, instead of $C_{20}\ H_{11}\ N, I$; and the other, that the action of oxide of silver upon the methyl and ethyl compounds of the nitryl bases of this class is more complex than would be supposed from the first equation, and the known success of the reaction with the iodides of the ammonium bases derived from the alcohol radicals alone.

So much has been said about the artificial formation of quinine from the leukol of coal-tar, that I have appended a few reasons for concluding that it is impossible by any analogous process to that previously described. Quinine, accord-

* Supposing for the moment the old formula for chinoline ($C_{18}\ H_8\ N$) to be correct.

† I have vainly searched through the Chemical Journals for any paper by Dr Hofmann, tending to show the real nature of the action of oxide of silver upon iodide of methylchinoline. This has led me to make the experiments detailed at page 392.

ing to STRECKER's experiments,* takes up only one equivalent of the alcohol radicals, and is therefore concluded with safety to be a nitryl base. Chinoline affords still more complete evidence of belonging to the same class, for not only is it incapable of taking up more than one equivalent, but, by the operation, it becomes converted into a fixed alkaloid. Now, any process for making artificial quinine by means of the reactions mentioned above, would result in the formation of an ammonium base, which must of necessity have a totally different constitution to quinine. It may be worth while, for a moment, to glance at the formidable difficulties by which the artificial formation of such a base as quinine is surrounded. In the first place, in the present state of our information, it appears to consist of three radicals, united and having one equivalent of nitrogen and two of oxygen attached. Now, to acquire a knowledge of the constitution of these three radicals (one of which, in all probability, is oxidized) is a problem involving a new mode of research, the key to which appears, for the present, to be hidden. And even supposing the three radicals known, they have to be formed; and then to combine them with the addition of an equivalent of nitrogen, without destroying the group, presents a task of no ordinary difficulty.

I should not have entered upon this branch of the subject, had it not been for the manner in which the possibility of the formation of quinine, by the method above alluded to, has been accepted as a reality, which is the more remarkable from the manner in which HOFMANN cautioned chemists against placing too much reliance upon the success of the process.

As it is my wish to correct, as far as my information will permit me, the erroneous views which have been formed of the relations between chinoline and quinine, I return to the supposed similarity between the action of heat upon quinine and the hydrated oxide of tetramethylammonium. This part of the subject is the more interesting, as it appears to have formed one of the links in the chain of reasoning, which led to a belief in the possibility of converting leukol into quinine, by the successive actions of iodide of methyl and oxide of silver. As the fixed base, hydrated oxide of tetramethylammonium, by heating yields trimethylamine, the difference being $C_2 H_4 O_2$, so quinine, less $C_2 H_4 O_2$ yields the old formula of leukol† thus,—

$$\underbrace{C_8 H_{13} NO_2}_{\text{Hydrated Oxide of Tetramethyl-ammonium.}} - C_2 H_4 O_2 = \underbrace{C_6 H_9 N}_{\text{Trimethyl-amine.}}$$

$$\underbrace{C_{20} H_{12} NO_2}_{\text{Quinine.}} - C_2 H_4 O_2 = \underbrace{C_{18} H_8 N}_{\text{Leukol.}}$$

The identity in kind of the above equations presupposes two conditions, neither

* Researches in Organic Chemistry, by ADOLPH STRECKER. Compt. Rend. xxxi. 49. Chem. Soc., Quart. Jour. 1854, vol. vii., p. 278.

† Quart. Jour. Chem. Soc., vol. iv., p. 328.

of which exist, the first being the correctness of the old formula for chinoline (or leukol), the second, its being the sole basic product in the distillate, from the cinchona alkaloids.

The next prevalent error with regard to chinoline is, that its salts have less tendency to crystallize than the generality of nitryl bases; whereas, in fact, the reverse (with some exceptions) is the truth. I have seldom seen salts more easy to crystallize than the nitrate, oxalate, and bichromate of chinoline, while its double salts, with platinum, gold, palladium, uranium, and cadmium, are beautiful substances, the same may be said of the iodides of the methyl, ethyl, and amyl compounds. The erroneous idea alluded to arose from previous experimenters, working on an impure substance.

The only means for determining the constitution of chinoline up to the present time, has been Dr HOFMANN'S analysis of the platinum salt, from a base extracted from coal-tar; for the combustions of the base itself yet made, are very unsatisfactory. Annexed are the results as yet obtained.*

	Hofmann.			Bromeis.		$C_{18}H_7N$	$C_{20}H_9N$
Carbon, . . .	82·67	82·88	82·34	82·74	82·78	83·70	83·91
Hydrogen, . .	6·56	6·25	6·10	6·11	5·88	5·41	6·29
Nitrogen, . .	11·28	...	...	...	...	10·89	9·80
						100·00	100·00

A glance at the above numbers shows that no conclusion can be drawn from them; and when it is considered, that chinoline and lepidine only differ by ·21 in their percentage of carbon, it becomes evident, that careful analyses of the salts of these bases are the only means by which their history and composition can be rendered certain. The platinum salt of chinoline possesses characters which render it peculiarly well adapted for this purpose, inasmuch as it differs totally from the corresponding compound from the Dippel and aniline series, in its great insolubility. I therefore selected this compound as a means of ascertaining the purity of the various fractions obtained in the course of the investigation, and which were intended for conversion into the various salts described further on; sometimes I was contented with merely a platinum determination, at others, I ascertained by combustion with chromate of lead, the percentage of carbon and hydrogen, and in this manner, the analyses quoted below were obtained. In my former paper, I gave the result of three combustions of the platinum salt of chinoline, and three platinum determinations; the salts analysed were obtained from fractions boiling at a somewhat lower temperature than those the details of the analyses of which are given below. The following analyses were made with salts obtained from fractions boiling about 460°, which is, probably, very nearly the boiling point of chinoline.

* GERHARDT, Traité, troisième partie, p. 149.

I. 8·900 grains of platinum-salt of chinoline from fractions boiling between 450° and 460° F. gave
10·559 ... carbonic acid and
2·191 ... water, and
6·235 ... platinum salt of chinoline, gave
1·827 ... platinum.

II. 6·057 ... platinum-salt of chinoline gave
1·772 ... platinum.

III. 5·907 ... platinum-salt of chinoline gave
1·731 ... platinum.

or, per cent.—

	I.	II.	III.	Mean.
Carbon, . .	32·36	...	...	32·36
Hydrogen, . .	2·74	...	...	2·74
Nitrogen, . .	...	...	...	...
Chlorine, . .	...	...	...	...
Platinum, . .	29·30	29·26	29·30	29·29

In the following table, the result of all my analyses (including those in the former paper) is compared with the numbers required by theory; the analysis just quoted being the fourth in the series :—

	I.	II.	III.	IV.	V.	VI.	Mean.	Theory.
Carbon, . .	31·93	32·24	32·52	32·36	...	...	32·26	32·19
Hydrogen, .	3·09	2·62	2·58	2·74	...	...	2·76	2·39
Nitrogen, . .	...	...	...	...	...	...	...	4·17
Chlorine, . .	...	...	...	...	...	...	...	31·74
Platinum, .	29·44	29·30	29·60	29·30	29·40	29·26	29·38	29·51
								100·00

It will be seen that there is a slight excess both in the carbon and hydrogen of these analyses. This arises from the presence of a small quantity of lepidine, the platinum salt of the two bases being too nearly of the same degree of solubility to allow of separation by fractional crystallization. This source of error is much lessened in the other salts, their formation, in most cases, being a process of purification. Platinochloride of chinoline is very sparingly soluble in cold water, requiring 893 parts for solution at 60° F.

It is to be remembered, that all the chinoline compounds mentioned in this paper were made from a base procured by distillation of cinchonine with potash, the coal-chinoline requiring a tedious series of purifications, in addition to the fractional distillations, before it could be obtained pure enough for conversion into compounds fit for analysis. The platinum-salt is, however, more easily obtained in a pure state from the coal bases, than most other compounds of this alkaloid.

In the following table, the mean result of my analyses of the platinum-salt of chinoline is compared with those obtained by other observers,* whose numbers have been recalculated according to the present atomic weight of carbon.

* Gerhardt, loc. cit.

	Hofmann.		Gerhardt.			Bromeis.			Grev. Williams. Mean.	Calcul.
Carbon, .	32·06	...	32·99	32·46	32·51	33·31	33·42	33.33	32·26	32·19
Hydrogen,	2·58	...	3·14	3·14	3·28	2·71	2·83	2·68	2·76	2·39
Nitrogen,	...	...	4·42	...	...	3·98	4·21	4·00	...	4·17
Chlorine,	30·96	...	...	...	...	...	...	...	...	31·74
Platinum,	29·27	29·11	27·80	28·08	27·69	28·23	28·34	28·81	29·38	29·51
										100·00

Only the first of these analyses was made from a base extracted from coal-tar; all the others were obtained from chinoline, produced by destructive distillation of cinchonine with potash.

Laurent, by mixing hot alcoholic solutions of hydrochlorate of chinoline and bichloride of platinum, obtained, after twenty-four hours, fine yellow needles; but, on examination under the lens, it was found not to be a homogeneous crystallization, for a small quantity of little grains had also deposited.* I have not found that any observer, except myself, has subjected the bases produced from cinchonine to a systematic fractionation, before forming the platinum salt. The fraction analysed by me had been rectified fourteen times, and was nearly constant between 460° and 470°.

Aurochloride of Chinoline.—The only account of this beautiful salt I have been able to find is in Dr Hofmann's paper, on the bases of coal-tar, where he merely states, that it corresponds in colour and other properties with the gold salt of aniline, but the latter appears† to be a yellow precipitate which rapidly becomes brown in the air, and, therefore, differs considerably from the chinoline salt, which is quite permanent under the same circumstances. As obtained by me from a specimen of chinoline of considerable purity, it was in the form of slender canary-yellow needles, sparingly soluble in cold water, and precipitating instantly on the addition of a solution of terchloride of gold, to a moderately strong solution of hydrochlorate of chinoline.

{ 3·883 grains of aurochloride of chinoline dried at 212° gave, on ignition,
{ 1·625 ... of gold.

or, per cent.—

Experiment.	Theory. $(C_{18}\,H_7\,N,\,HCl + Au\,Cl_3)$
41·85	42·00

Palladiochloride of Chinoline.—Dr Hofmann describes this salt in his paper, previously quoted, as resembling that from aniline, but M. Müller‡ states the latter to be *yellow;* I found, however, that when moderately concentrated solutions of chloride of palladium and hydrochlorate of chinoline are mixed, a copious

* Gerhardt, loc. cit., and Laurent, Ann. de Chim. et de Phys. [3] xxx., 368.
† Gerhardt, Traité, tome troisième, p. 86. ‡ Ann. der Ch. u. Pharm. lxxxvi., 368.

deposit of *chestnut-brown* crystals takes place. This salt is moderately soluble in water. It requires a very strong heat to give pure metallic palladium.

{ 3·423 grains of palladiochloride of chinoline gave, on powerful ignition in a porcelain capsule,
{ ·725 ... palladium.

or, per cent.—

Experiment.	Theory. $(C_{18}\ H_7\ N,\ HCl, + Pd\ Cl$
21·18	20·96

Cadmium Salt Chinoline.—The experiments of Croft, and more especially Von Hauer, have shown that cadmium forms well-defined crystalline salts, with the chlorides of the alkalies, alkaline earths, and the chloride of ammonium. Before I became acquainted with the results of the latter chemist, I had been engaged in a series of experiments made with a view of extending our knowledge of the double salts formed by the hydrochlorates of the alkaloids with metallic chlorides. The information at present in our possession on this subject is very limited. The only salts of the class alluded to which have been analysed, are those formed with platinum, gold, palladium, and mercury. Now the salts of the latter metal vary greatly in constitution, and are, moreover, somewhat troublesome to analyse. I have, therefore, made a few experiments with a view to ascertain what other metals than those mentioned above, yield chlorides capable of combining with the alkaloids, to form well crystallized double salts. In the present communication, however, I only notice those formed by the chlorides of cadmium and uranyl with chinoline.

When moderately concentrated solutions of hydrochlorate of chinoline and chloride of cadmium are mixed, the fluid solidifies with rise of temperature to a snow-white mass of crystals. If the solutions are not too strong, they are obtained in the form of needles occupying a great bulk when in the mother liquor, but shrinking very much when pressed. They are less soluble in alcohol; I therefore used that fluid to wash them. The alcoholic washings, when kept for some time, deposit needles, often an inch long, but so silky and fragile as to be preserved of their original size with difficulty. They retain their colour perfectly, and, with the exception of losing two equivalents of water of crystallization, are quite unaltered by drying for a few hours at 212°. The salt volatilizes at a considerably higher temperature, without residue. The quantity at my disposal was very limited, and being, therefore, obliged to work on small quantities, I found it inconvenient to estimate the cadmium as oxide by the usual process, as the carbonate of cadmium, when precipitated from solutions containing chinoline at the boiling heat, not only has a strong tendency to pass through the filter, but adheres to the paper so strongly as to cause a loss of metal by reduction and volatilization during incineration. The precipitate was too light to be collected by decantation.

I obtained, however, an accurate result by precipitating with sulphuretted hydrogen, and collecting the sulphide of cadmium on a weighed filter.

I. { 7·911 grains of cadmium salt, dried at 212°, gave
8·976 ... carbonic acid, and
1·765 ... water.

II. { 8·875 ... cadmium salt, dried at 212°, gave
10·053 ... carbonic acid, and
2·060 ... water.

III. { 10·534 ... cadmium salt, dried at 212°, treated by PELIGOT's process, gave
·42295 ... nitrogen.

IV. { 6·997 ... cadmium salt chinoline, dried at 212°, gave
2·885 ... sulphide cadmium.

	I.	II.	III.	IV.	Mean.	Calculation.		
Carbon, .	30·94	30·89	...	...	30·92	30·99	C_{18}	108
Hydrogen, .	2·48	2·58	...	...	2·53	2·29	H_8	8
Nitrogen, .	...	...	4·02	...	4·02	4·02	N	14
Chlorine, .	...	...	...	...	...	30·56	Cl_3	106·5
Cadmium, .	...	...	...	32·07	32·07	32·14	Cd_2	112
						100·00		348·5

It is evident, therefore, that the formula for the salt dried at 212° is—

$$C_{18}\,H_7\,N,\,H\,Cl + 2\,Cd\,Cl.$$

Several examples of salts of the same constitution occur in inorganic chemistry, some of which have been examined by M. VON HAUER, who terms them chlorobicadmiates. The chinoline salt, if merely dried by exposure to the air, contains two equivalents of water, for at 212° it loses 5·41 per cent.; theory requires 4·91. The excess arises from a little moisture adhering somewhat tenaciously to the crystals.

Hydrochlorate of Chinoline and Chloride of Uranyl.—If double carbonate of uranium and ammonia, dissolved in hydrochloric acid, is added to a strong solution of hydrochlorate of chinoline, the fluid rapidly becomes filled with short, brilliant, yellow needles, and in a few minutes the whole fluid solidifies, so that the vessel may be inverted without the contents escaping. From more dilute solutions, prismatic crystals are deposited, sometimes of considerable size. The salt is of a rich yellow colour, and is very soluble in water. It was quite free from any trace of ammonia. The mother liquid was removed by washing with alcohol. The quantity at my disposal was too small to allow of a complete examination of all its properties.

I. { 6·760 grains of uranium salt of chinoline, dried at 212°, gave
7·900 ... carbonic acid, and
1·686 ... water.

II. { 5·135 ... uranium salt, dried at 212°, gave
4·352 ... chloride of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	31·87	...	32·05	C_{18}	108
Hydrogen, . , .	2·77	...	2·37	H_8	8
Nitrogen, . . .	...	...	4·15	N	14
Chlorine, . . .	...	20·97	21·07	Cl_2	71
Uranium, . . .	...	...	35·61	Ur_2	120
Oxygen, . . .	...	...	4·75	O_2	16
			100·00		337

The formula—

$$C_{18}\,H_7\,N,\,H\,Cl,\,+\,(Ur_2\,O_2)\,Cl,$$

appears, therefore, to be the correct expression of the analysis, and it agrees in constitution with the anhydrous ammoniochloride of uranyl of PELIGOT. It is my intention to examine the double compounds of uranium with other organic bases.

Binoxalate of Chinoline.—The great discrepancy in the results of RUNGE and HOFMANN with regard to the oxalate of chinoline, made me desirous of ascertaining the nature of this salt. According to the former chemist, leukol (chinoline) has such a great tendency to form a crystalline oxalate, that this property is its marked characteristic; HOFMANN, on the other hand, could only obtain it in the form of a confused, radiated, glutinous mass, deposited when the solution had reached a certain state of concentration. I found, however, that if 24·3 parts of chinoline are added to 16·5 parts dry oxalic acid, dissolved in a small quantity of water, the whole solidifies to a white crystalline mass, of the consistence of soft cheese. The salt cannot be obtained pure by a random admixture of the ingredients, as, although the chief tendency appears to be to form the binoxalate, yet other compounds are also formed in sufficient quantity to prevent constant analytical results from being obtained, unless the above proportions are used. The salt, before being employed for analysis, must be recrystallized from alcohol once or twice, when it forms fine silky needles. It is partially decomposed by exposure for two days to 212°, with evolution of chinoline, a salt being formed intermediate in composition between the binoxalate and quadroxalate. It is necessary, therefore, to dry it for analysis *in vacuo* over sulphuric acid.

6·995 grains of binoxalate of chinoline, dried *in vacuo*, gave
15,407 ... carbonic acid, and
2,798 ... water.

	Experiment.	Calculation.		
Carbon, ...	60·07	60·27	C_{22}	132
Hydrogen, ...	4·44	4·11	H_9	9
Nitrogen, ...	...	6·39	N	14
Oxygen, ...	...	29·23	O_8	64
		100·00		219

Nitrate of Chinoline.—In some respects my experiments on this salt tally with those of Dr HOFMANN, in others they differ considerably. It was obtained by the last-named chemist by allowing a mixture of leukol and dilute nitric acid to rest under a bell-jar; after some time the salt crystallized in confused concentric needles, which were obtained white and dry by pressure between folds of filtering paper. He does not appear to have analysed it. I found that if, after an excess of nitric acid slightly diluted was added to chinoline, the fluid was evaporated on the water-bath, a pasty mass was obtained, which solidified on cooling. From a hot alcoholic solution fine white needles soon deposited, which were infusible at 212°, and unalterable in the air. Dr HOFMANN, on the contrary, found his salt to fuse on moderate heating, and to rapidly become blood-red by exposure to the air; these are, evidently, the characters of an impure substance. The nitrate of chinoline was burnt with oxide of copper, a long column of copper turnings being placed in the front of the tube.

6·590 grains of nitrate of chinoline, dried at 212°, gave
13·516 ... carbonic acid, and
2·530 ... water.

	Experiment.	Calculation.		
Carbon, . . .	55·94	56·25	C_{18}	108
Hydrogen, . . .	4·27	4·17	H_8	8
Nitrogen, . . .	...	14·58	N_2	28
Oxygen, . . .	...	25·00	O_6	48
		100·00		192

Chinoline gives a very marked reaction with strong fuming nitric acid, and which also shows its great stability. If a few drops of the base are allowed to trickle down the side of a test-tube, and a small excess of the acid is added, the two combine with violence, the portion of alkaloid adhering to the sides is converted by the fumes of the acid into long needles, and, when cold, the whole fluid solidifies to a beautifully white crystalline mass of pure nitrate. No nitrochinoline, or any other decomposition product, is formed, if the base be free from impurities.

Bichromate of Chinoline.—GERHARDT, in describing this salt,* merely states, that chromic acid in solution gives, with pure chinoline, an orange-yellow crystalline precipitate, and that the dry acid decomposes the base with inflammation, Dr HOFMANN, in his paper on the coal bases, previously referred to, states that, a short time after he had commenced the investigation of leukol, he was inclined to consider it the same as that GERHARDT obtained by the action of hydrate of potash on quinine, cinchonine, and strychnine. He says, however, that he soon convinced himself that they were totally distinct, their behaviour towards a

* Traité de Chimie Organique, troisième partie, p. 150.

solution of chromic acid being quite dissimilar, for while chinoline and its salts gave a beautiful orange-yellow crystalline precipitate, leukol was oxidized and converted into a black resinous oil. Subsequently,* LIEBIG announced, upon the authority of experiments made by HOFMANN, that perfectly pure leukol gave the same crystalline precipitate with chromic acid. I have not been so successful as M. HOFMANN; for, although the chinoline and lepidine procured by destructive distillation from cinchonine have, in my hands, given salts of extreme beauty and purity with chromic acid, I have failed to obtain the same result with either the chinoline or lepidine from coal-tar. I have also boiled the bases from the latter source with dilute chromic acid, to destroy impurities, and then separated them by distillation with potash, but they merely gave an oily precipitate with chromic acid. When dissolved in hydrochloric acid, and bichromate of potash is added, the same result occurs. I even took a platinum salt of coal-lepidine, which yielded, on combustion, the numbers detailed in Analysis I., p. 398; and, after having reobtained the base by distillation with potash, endeavoured to procure from it a crystalline chromate, but in vain, a red oil being the only product. It is true, that when I added dilute chromic acid to chinoline from coal-tar, the sides of the tube acquired a coating of very minute brilliant points, which reflected light with a peculiar satin-like lustre; but the lens resolved them into oily globules. The following experiments were, therefore, made upon chinoline from cinchonine. Neither GERHARDT nor HOFMANN have analysed the salt.

The beauty of the bichromate of lepidine described in my last paper,† induced me to ascertain the composition and properties of the homologue next below it, in the anticipation that its outward appearance would be equally striking. But there are some slight differences in the two bodies; bichromate of chinoline is still less soluble than the other salt, and this prevents the crystals from being readily procured of so large a size. When dry, it is much more violently decomposed by heat than the lepidine compound. The fixed product is, however, the same, namely, green oxide of chromium and carbonaceous matter. If the dry salt be placed in a capsule, and heat be very gradually applied, no change at first takes place, but suddenly it takes fire with explosive violence, and the greater part of the green oxide and carbon is projected. I prepared the salt for analysis by adding dilute chromic acid in excess to pure chinoline; at first the product is somewhat resinous, but immediately it is touched with a glass rod, it becomes gritty and crystalline. The solid is then filtered off, the mass slightly washed, dissolved in boiling water, filtered to remove traces of an oily impurity, and, on cooling, the fluid becomes filled with brilliant yellow needles arranged in groups. It may be dried at 212° with safety, provided adhering moisture has been removed as much as possible by pressure between folds of filtering paper. If the

* Chem. Gaz., vol. iii., p. 251 (1845). Proc. of Chem. Soc., April 7, 1845.
† Trans. Roy. Soc. Edin., vol. xxi. part ii.

salt is previously moistened with hydrochloric acid, it may be ignited without explosion, and the green oxide estimated with accuracy. The combustion was made with oxide of copper.

I. { 6·827 grains of bichromate of chinoline, dried at 212° gave
11·284 ... carbonic acid, and
2·146 ... water.

II. { 6·381 ... bichromate of chinoline, dried at 212°, gave, on ignition,
2·072 ... green oxide of chromium.

III. { 5·534 ... bichromate of chinoline, dried at 212°, gave, on ignition,
1·787 ... green oxide of chromium.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon,	45·08	...	...	45·11	C_{18}	108
Hydrogen,	3·49	...	...	3·34	H_8	8
Nitrogen,	...	...	...	5·85	N	14
Chromium,	...	22·40	22·28	22·31	Cr_2	53·4
Oxygen,	...	...	...	23·39	O_7	56·0
				100·00		239·4

Density of the Vapour of Chinoline.—In Dr HOFMANN'S paper on the coal bases, he states that a determination of the density of the vapour of leukol (chinoline) failed, owing to its leaving a yellow residue on distillation. I have not found this circumstance to operate sufficiently in the case of chinoline from cinchonine, to cause more error than is usually found in determining the vapour densities of bodies obtained by fractional distillation, and having so high a boiling point. The specimen used, boiled in the fourteenth rectification between 460° and 470° F.

Temperature of air,	13° centigrade.
... vapour,	277° ...
Pressure,	751 millimetres.
Capacity of balloon,	330 cent. cub.
Residual air,	17·5 ...
Excess of weight of balloon,	0·4980 grammes.

The formula

$$C_{18}\,H_7\,N$$

requires

18 volumes carbon vapour,	0·829 . 18 =	14·922
14 ... hydrogen,	0·0692 . 14 =	·9688
2 ... nitrogen,	0·9713 . 2 =	1·9426
		17·8334

$$\frac{17{\cdot}8334}{4} = 4{\cdot}4583$$

Experiment.	Theory.
	$C_{18}\,H_7\,N$ = 4 volumes.
4·5190	4·4583

Action of Iodide of Methyl on Chinoline.

Hydriodate of Methyl-Chinoline.—When an excess of iodide of methyl is added to chinoline, and the mixture, inclosed in a pressure tube, is heated for

ten minutes to 212°, combination is perfectly effected, a finely crystallized hydriodate resulting. If this salt, which is perhaps more correctly called iodide of methyl-chinoline-ammonium, is treated in the cold with excess of oxide of silver, a strongly alkaline solution is obtained, containing the hydrated oxide of the ammonium base. The solution possesses little stability; on heating with potash, an excessively pungent odour is evolved, acting strongly on the eyes and mucous membrane of the nose. The solution reddens turmeric paper as powerfully as solution of caustic potash, and instantly restores the colour of reddened litmus. The reactions of this base, generally, are the same as those of the ethyl compound next to be described. The smell of a volatile base, a product of the decomposition of methyl-chinoline, is evolved from the moment of its formation; it appears to be methylamine.

By alternate precipitation of the hydriodate by nitrate of silver, hydrochloric acid, and bichloride of platinum, after removal of the chloride of silver, a sparingly soluble platinum salt was obtained. The following is the result of its analysis:—

I. { 8·930 grains of platinochloride of methyl-chinoline, gave
11·350 ... carbonic acid, and
2·496 ... water.

II. { 8·515 ... platinochloride of methyl-chinoline gave
10·777 ... carbonic acid, and
2·303 ... water.

III. { 7·861 ... platinochloride of methyl-chinoline gave
2·217 ... platinum.

IV. { 5·165 ... platinochloride of methyl-chinoline gave
1·456 ... platinum.

	Experiment.				Mean.	Calculation.		
	I.	II.	III.	IV.				
Carbon,	34·66	34·52	...	...	34·59	34·33	C_{20}	120
Hydrogen,	3·11	3·00	...	...	3·06	2·86	H_{10}	10
Nitrogen,	...	...	...	...	...	4·01	N	14
Chlorine,	...	...	...	...	...	30·47	Cl_3	106·5
Platinum,	...	...	28·20	28·19	28·20	28·33	Pt	99
						100·00		349·5

Methyl-chinoline is, therefore, isomeric with lepidine, but has no other point of resemblance. The decompositions of the hydriodate almost exactly resemble those of the ethyl base, and, as the atomic weight of the latter, being higher, gave it an advantage for experiment, I selected it for the purpose.

Action of Iodide of Ethyl on Chinoline.

Hydriodate of Ethyl-Chinoline.—No action takes place on the mere addition of excess of iodide of ethyl to chinoline, but if the tube containing the mixture be sealed and exposed for some hours to a temperature of 212°, the whole becomes a

mass of crystals. When this is the case, and the tube cools, the end may be cut off, a tube bent twice at right angles attached by means of a cork, and, the pressure-tube being immersed in the water-bath, the excess of iodide of ethyl distilled over. The crystals are then dissolved out in a small quantity of hot alcohol, and the solution allowed to cool. The first crop is of a rich yellow colour, becoming of a pale lemon tint on recrystallization. In the state in which the salt is thus obtained it possesses the property of becoming a deep blood-red at 212°, and regaining its normal tint on cooling; this peculiarity becomes much lessened by a repetition of the process. The crystals appear to be cubic, and are easily obtained of considerable size. They dissolve more readily in water than alcohol, but the latter is the best solvent for the purposes of crystallization.

I. { 6·336 grains of hydriodate of ethyl-chinoline, dried at 212°, gave
10·810 ... carbonic acid, and
2·513 ... water.

II. { 6·684 ... hydriodate of ethyl-chinoline, dried at 212°, gave
5·457 ... iodide of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	46·53	...	46·32	C_{22}	132
Hydrogen, . .	4·41	...	4·21	H_{12}	12
Nitrogen, . .	...	...	4·91	N	14
Iodine . .	...	44·12	44·56	I	127
			100·00		285

Platinum Salt of Ethyl-Chinoline.—After adding nitrate of silver to a solution of hydriodate of ethyl-chinoline, as in making the iodine determination last mentioned, the excess of silver was removed by the addition of hydrochloric acid, the liquid filtered, and evaporated to a moderate bulk, on the addition of bichloride of platinum, a rich golden-yellow precipitate of sparing solubility was obtained; it was first washed with a little water, and afterwards with alcohol.

{ 6·813 grains of platinochloride of ethyl-chinoline gave
1·847 ... platinum.

Agreeing with the formula

$$C_{22}\,H_{11}\,N,\,H\,Cl + Pt\,Cl_2.$$

	Experiment.	Calculation.		
Carbon, . . .	...	36·31	C_{22}	132
Hydrogen, . .	...	3·30	H_{12}	12
Nitrogen, . . .	...	3·85	N	14
Chlorine, . . .	...	29·30	Cl_3	106·5
Platinum, . . .	27·11	27·24	Pt	99
		100·00		363·5

It is evident that ethyl-chinoline is isomeric with cryptidine, the new base to be described further on.

Action of Oxide of Silver on Iodide of Ethyl-Chinoline.

Hydrated Oxide of Ethyl-Chinoline-Ammonium.—A solution of iodide of ethyl-chinoline is decomposed with ease by oxide of silver, even in the cold, a colourless strongly alkaline fluid being formed, containing the fixed base corresponding to the hydrated oxide of tetrethylammonium. The solution instantly reddens turmeric paper, and restores the colour of reddened litmus. It precipitates solutions of sulphate of copper, sesquichloride of iron, acetate of lead, and corrosive sublimate. The addition of a small quantity to a red solution of bichromate of potash renders it yellow, by neutralizing the second equivalent of chromic acid. The solution of the base decomposes chloride of ammonium, liberating the ammonia freely.

The heat of a water-bath decomposes the solution of the hydrated oxide, with production of a splendid crimson colour, the sides of the basin where the liquid has dried becoming a brilliant emerald green, passing in a few seconds to a blue of great beauty and intensity. These colours, like those to be mentioned presently, evidently depend upon oxidation, and would require a very large amount of material to follow out in detail. When the solution of hydriodate of ethyl-chinoline is heated on the water-bath with excess of oxide of silver, a volatile product is evolved, acting strongly upon the eyes.

Action of Sulphate of Silver upon Hydriodate of Ethyl-Chinoline.

If hot solutions of sulphate of silver and hydriodate of ethyl-chinoline are mixed, double decomposition ensues, without any further action taking place, the solution of sulphate of ethyl-chinoline remaining colourless, and the iodide of silver separated being of the normal tint; but, if it be attempted to concentrate the solution by evaporation on the water bath, it undergoes a curious metamorphosis, the sides of the dish, where the solution has dried, become a deep pure blue, but, as the evaporation proceeds, the solution becomes crimson, and when dry, the mass is so deep in tint, as to be nearly black. The dry substance has a slight coppery lustre, like that which indigo possesses when rubbed. It dissolves in water, the solution being of the most gorgeous crimson, becoming rose-coloured by addition of ammonia, while hydrochloric or nitric acids convert it to a scarlet. The colour is tolerably stable, requiring a considerable excess of bromine water to decompose it, the fluid then becoming reddish-brown.

The crimson liquid undoubtedly contains the sulphate of a new base, apparently a product of oxidation of ethyl-chinoline. The reactions upon which this supposition is founded, are the following:—If solution of potash be added to the crimson solution, the colouring matter is almost entirely precipitated, and, if the experi-

ment be successful, the solution merely retains a slight blue tinge. This precipitate appears to be the new base, although probably in a very impure state. When first thrown down, it has a beautiful reddish-violet colour, like that of the crystallized sesquichloride of chromium. It may be washed with water on a filter, being sparingly soluble; it dissolves readily in alcohol, forming a fine crimson fluid, which precipitates a spirituous solution of chloride of mercury. The base dissolves readily in hydrochloric acid, the solution giving a voluminous precipitate with bichloride of platinum. The platinum salt, after well washing with water, was burnt for the percentage of metal, to ascertain whether its atomic weight was higher or lower than that of ethyl-chinoline.

{ 2·081 grains of platinum salt gave
{ ·492 ... platinum.

212·2	157·0
Atomic weight of new base.	Atomic weight of ethyl-chinoline.

The atomic weight which is derived from this experiment is so excessively high, that no simple relation is apparent between the red product and ethyl-chinoline. The single platinum determination, although carefully made, is evidently insufficient to enable any speculation to be made as to the nature of the decomposition.

It is known that most nitryl bases (those composed solely of the alcohol radicals being the chief exceptions) yield colours by the action of oxide of silver on the ammonium compounds formed with methyl, ethyl, and amyl, but I think none yet worked on yield such magnificent tints as those mentioned in this paper. The subject has another and much greater point of interest than the mere formation of coloured reactions, however beautiful they may be, inasmuch as the careful following out of the decompositions on the large scale promises to assist us in acquiring a knowledge of the constitution of alkaloids of this class. I have therefore promised myself to examine the matter more fully, when the other investigations with which I am now occupied are concluded.

Action of Iodide of Amyl on Chinoline.

Hydriodate of Amyl-Chinoline.—Iodide of amyl reacts with comparative slowness upon chinoline. It is necessary to keep the materials in a pressure-tube for some hours at 212° to effect combination. The iodide crystallizes from alcohol with extreme readiness, and when evaporated slowly upon flat surfaces presents under the lens very peculiar and beautiful forms. On one occasion, after dissolving the iodide in alcohol in a beaker, the fluid was poured into another vessel, and the solution remaining on the sides crystallized in the manner I have endeavoured to illustrate in the annexed sketch. The figure represents the forms about four times the natural size.

The hydriodate of amyl chinoline gave, in a determination of the percentage of iodine, the following numbers.

9·882 grains hydriodate amyl-chinoline gave
7·086 ... iodide silver.

	Experiment.	Calculation.		
Carbon, ...		51·38	C_{28}	168
Hydrogen, ...		5·50	H_{18}	18
Nitrogen, ...		4·28	N	14
Iodine, . .	38·75	38·84	I	127
		100·00		327

The fluid from which the iodine had been precipitated was treated with hydrochloric acid in excess, the chloride of silver removed by filtration, and the fluid evaporated to a moderate bulk, excess of bichloride of platinum was then added, and the precipitated platinum salt washed, first with a little water, and then with a mixture of alcohol and ether. The platinochloride of amyl-chinoline is only sparingly soluble in water, it was dried at 212°, and burnt with chromate of lead and copper turnings.

6·756 grains of platino-chloride of amyl-chinoline, gave
10·233 ... carbonic acid, and
2·813 ... water
7·150 ... platinochloride of amyl-chinoline gave
1·733 ... platinum.

	Experiment.	Calculation.		
Carbon, . .	41·31	41·43	C_{28}	168
Hydrogen, . .	4·63	4·44	H_{18}	18
Nitrogen, . .	...	3·45	N	14
Chlorine, . .	...	26·26	Cl_3	106·5
Platinum, . .	24·24	24·42	Pt	99
		100·00		405·5

Action of Chlorine on Chinoline.—According to GERHARDT,* chlorine converts chinoline into a black resin, but my experiments show that it acts in a very different manner, if care be taken to prevent rise of temperature. On dropping chinoline into a large glass vessel of the gas, and leaving it for twelve or fourteen hours, a yellow oil is produced, which, on treatment with water, leaves a white insoluble matter, which I have not yet had an opportunity of studying more in detail.

Action of Chloride of Acetyl on Chinoline.—Chloride of acetyl on being added

* Traité, troisième partie, p. 150.

to chinoline, develops much heat, and on evaporation at 212°, a crystalline mass was obtained, but so deliquescent, as to be unfit for examination.

On the Chinoline Series as it occurs in Coal-Tar.

In my paper "On some of the Basic Constituents of Coal-Naphtha and on Chrysène," I ventured to express a belief, that chinoline was not the only member of the group to which it belongs present in coal-tar; and feeling assured that other homologues remained to be discovered, I was desirous of testing the accuracy of the supposition. Owing to the kindness of Mr George Miller of Dalmarnock, I was enabled to obtain fifty gallons of coal-oil of a very high boiling point, and of a density greater than that of water. It was shaken with sulphuric acid to extract the alkaloids, and the acid fluid after dilution with water was treated with excess of lime, and distilled as long as any came over. As the amount of bases of the Dippel series present, was not large, the product being of such a high boiling point, I did not add potash to separate the more soluble portion, but only collected that part which, from its density and insolubility, sank to the bottom of the fluid accompanying it in the distillation. By means of a tap funnel, the basic-oil was separated from the chief part of the water accompanying it, which contained some of the pyridine series in solution. The bases thus obtained are exceedingly impure, and contain aniline and some non-basic substances. It being probable, that toluidine and even other members of the same series might be present, I thought that as apparently insurmountable difficulties prevented their separation from the chinoline series by means of oxalic acid, or similar methods, their presence might nevertheless be made manifest through their products of decomposition. With this view, I treated the mixed bases with nitrite of potash and hydrochloric acid, in the manner indicated by Hunt,* and by this means effectually decomposed all traces of the aniline group present. But the amount of oil heavier than water containing the hydrates of phenyl and cresyl was too small to allow of my further examining them. I propose, however, to return to the subject at a future time.

The acid fluid, after decantation from the heavy oil last alluded to, was then placed in a retort, and a jet of steam sent through the tubulature to the bottom of the liquid; by this means, many non-basic impurities were removed, and amongst them a white crystalline solid distilled over with the steam, and which eventually proved to be naphthaline. The acid fluid in the retort, after being filtered through pulverized charcoal, to separate resinous matters not volatilized, was treated with potash, to liberate the base, which was then separated by a tap funnel, and completely dried by digestion with sticks of potash. It is proper to

* Silliman's Journal 1849; Chem. Gaz., Jan. 1850; Gerhardt, Traité, tome 3me, p. 83; Hofmann, Quart. Jour. Chem. Soc.

mention, that by this treatment, the boiling point of the bases was considerably raised, the aniline being removed, which boils 100° below chinoline.

The bases, as purified by the above method yield, on distillation, fractions from 350° to 525°. Considerably more than one hundred distillations were made before sufficient separation had taken place, to justify me in making any analyses.

Chinoline having already been proved to exist in coal-tar, I began the experiments by searching for lepidine. This base, which was discovered by me* among the volatile alkaloids procured by distilling cinchonine with potash, has the formula,

$$C_{20}\,H_9\,N,$$

which was established by analyses of the platinum salt, nitrate, hydrochlorate, and bichromate, confirmed also by a determination of its vapour density.

As obtained from coal-tar, lepidine is in the form of an oil, having an odour almost exactly the same as that from cinchonine. But it is impossible by distillation alone, even after the treatment of the crude base with nitrous acid, to procure it in the same state as from the source where it was first found. Coal-lepidine, therefore, yields salts which, for the most part, crystallize with incomparably greater difficulty than that from cinchonine. When dissolved in an acid, although the solution is perfect, there is always an after-odour evolved, somewhat like naphthaline, unless the base has been purified with great care. I was unable, by any means at at my disposal, to obtain the bichromate in a crystalline state, whereas the lepidine from cinchonine yields a beautiful salt, in brilliant yellow needles half-an-inch long. Even when coal-lepidine is boiled with diluted chromic acid for an hour, the base, when separated and redistilled, gives an oily precipitate with chromic acid; moreover, the same occurs if the base be dissolved in hydrochloric acid, and a solution of bichromate of potash is added, as has been previously mentioned in describing the bichromate of chinoline. The red oil obtained in this manner from coal-lepidine may be kept for weeks without showing any tendency either to crystallize or decompose; even the base obtained from a platinum salt of considerable purity, behaved in the same way. On the other hand, coal-lepidine reacts with nitric acid and some other re-agents, like that from cinchonine.

If it were not for the decisive manner in which the fact of Dr HOFMANN's having obtained the crystalline precipitate with leukol and chromic acid has been announced, I should have felt justified in asserting that the bases derived from the two sources were isomeric but not identical; but as the last-named chemist's well-known accuracy prevents me from entertaining a belief in the possibility of an error of experiment, I can only express my regret, that I am ignorant of the method of

* Trans. Royal Soc. Edin., vol. xxi., pt. 27.

purification adopted by him. The most satisfactory mode of explanation of the differences in the properties of the coal and cinchonine series, as obtained by me, is that they are in a peculiar molecular condition, analogous in some respects to the phenomena known in the cases of quinine, the amylic alcohol, and many other bodies, instances of which are daily becoming more numerous. Chemists are aware that even variations in the density and boiling point of the same fluid, when in different states, have been observed; and I may mention, as corroborative of this, that with bases distilled the same number of times, lepidine as pure as I could procure it from both sources differed in boiling point by 25° F.; for the lowest fraction of coal-lepidine that gave correct results on analysis distilled between 485° and 495°, whereas the lowest fraction of the same base from cinchonine, boiled between 510° and 520° F. Another fact which seems corroborative of the supposition that Dr HOFMANN obtained the chinoline from coal-tar in the state in which I procured it from cinchonine, is found in the circumstance that the density of chinoline from coal-tar was ascertained by him to be 1·081, or very near the same number as in my determination of the density of the same base from cinchonine, viz., 1·085. But the coal bases examined by me were lighter than this; for even the lepidine from the the last source had a density of only 1·072 at 60° F., being actually lighter than the homologue, one step below from cinchonine.

I have observed with the pyridine series, as obtained from bone-oil, coal-naphtha, and bituminous shale, that considerable differences are found in their power of forming crystalline salts, and it is, therefore, most probable that the same distinctions exist between them that are met with in the case of the bases from coal and cinchonine. I trust eventually to be able to elucidate some of these points, by subjecting chinoline from both the above substances to the action of polarized light.

The lepidine platinum salt, from cinchonine, precipitates at once in a pulverulent state; but from coal it is for a few seconds soft and resinous, but soon becomes hard and crystalline. The following are my analyses of it from the latter source:—

I. 9·216 grains crystallized platinochloride of lepidine from fraction boiling 485°-95° 14th rectification, dried at 212°, gave
11·652 ... carbonic acid, and
2·466 ... water.

II. 5·954 ... platinochloride lepidine from fraction 485°-95°, gave
1·673 .. platinum.

III. 8·754 ... platinochloride lepidine from fraction 495°-505°, 14th rectification, gave
11·101 ... carbonic acid, and
2·452 ... water.

IV. 5·554 ... platinochloride lepidine, same as last, gave
1·575 ... platinum.

V. 5·047 ... platinum salt, 485°-95° crystallized, gave
1·417 ... platinum.

	Experiment.					Mean.	Calculation.		
	I.	II.	III.	IV.	V.				
Carbon,	34·48	...	34·58	...	...	34·53	34·33	C_{20}	120
Hydrogen,	2·97	...	3·11	...	...	3·04	2·86	H_{10}	10
Nitrogen,	...	...	...	...	...	...	4·01	N	14
Chlorine,	...	...	...	...	...	...	30·47	Cl_3	106·5
Platinum	...	28·09	...	28·36	28·08	28·17	28·33	Pt	99
							100·00		349·5

In the following table, the mean of these results is compared with my analyses of the platinum salt from the cinchonine bases.

	Coal-Tar. Mean.	Cinchonine. Mean.	Theory.
Carbon, . . .	34·53	34·04	34·33
Hydrogen, . . .	3·04	2·95	2·86
Nitrogen, . . .	...	...	4·01
Chlorine, . . .	...	...	30·47
Platinum, . . .	28·17	28·13	28·33

Action of Iodide of Ethyl on Lepidine.

Hydriodate of Ethyl-Lepidine.—As it was evident that the process for preparing this compound, and from it the platinum salt, was one of purification, I thought that I should, by this means, obtain nearer results on analysis than was the case with the experiments last quoted; and the following may be considered as confirming the truth of the supposition.

Coal-lepidine, sealed in a tube with excess of iodide of ethyl, and exposed for some hours to 212°, yields a mass of brown needles, which, on recrystallization from alcohol, are of a brilliant canary yellow. They have the same property of becoming red at 212° as the corresponding salt of chinoline, although scarcely to the same degree.

{ 7·111 grains iodide ethyl-lepidine gave
{ 5·595 ... iodide silver.

or per cent.—

	Experiment.	Calculation.		
Carbon, . .	...	48·16	C_{24}	144
Hydrogen, . .	...	4·68	H_{14}	14
Nitrogen, . .	...	4·68	N	14
Iodine, . .	42·52	42·48	I	127
		100·00		299

Platinum Salt of Ethyl-Lepidine.—This salt was obtained in the same manner as the corresponding one of ethyl-chinoline. It is, at the first moment of precipitation, somewhat soft, but soon becomes hard and crystallized. It was pulverized and well washed with a mixture of alcohol and ether previous to analysis.

I. { 8·374 grains platinochloride ethyl-lepidine gave
11·667 ... carbonic acid, and
2·880 ... water.

II. { 6·377 ... platinochloride ethyl-lepidine gave
1·688 ... platinum.

III. { 4·952 ... platinochloride ethyl-lepidine gave
1·317 ... platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . .	38·00	...	...	38·14	C_{24}	144
Hydrogen, . .	3·82	...	...	3.71	H_{14}	14
Nitrogen, . .	...	...	...	3·71	N	14
Chlorine, . .	...	...	...	28·21	Cl_3	106·5
Platinum, . .	...	26·47	26·60	26·23	Pt	99
				100·00		377·5

Density of Vapour of Lepidine.—In my former paper on the chinoline bases, I gave 5·14 as the density of the vapour of lepidine, as found by experiment, and I was desirous of ascertaining that of the same base, as extracted from coal-tar, in order to serve as a comparison. It is remarkable to observe the difference which an increment of $C_2 H_2$ has in modifying the power of substances to resist the decomposing influence of heat. While, in taking the density of chinoline at 531° F., being 71° above its boiling point, the fluid condensed in the balloon was almost colourless, lepidine after exposure under the same circumstances to only 523° F., or 28° above its boiling point, had become nearly black; this darkening, caused by separation of carbon, prevented me from making the experiment at a temperature as much above the boiling point as in the case of chinoline, and the two sources of error have the effect of making the density come out somewhat too high.

Temperature of air, . .	15° centigrade.
... vapour, .	273° ...
Pressure, ...	755 millimetres.
Capacity of balloon . .	331 cent. cub.
Excess of weight of balloon, .	·6745 grammes.
Residual air, . . .	6 cent. cub.
Density, . . .	5·15

The formula

$$C_{20} H_9 N$$

requires the following numbers:—

20 volumes carbon vapour, . .	=0·8290 . 20=16·580
18 ... hydrogen, . . .	=0·0692 . 18= 1·2456
2 ... nitrogen, . . .	=0·9713 . 2= 1·9426
	19·7682
	$\frac{19·7682}{4}$ = 4·94205

Density of vapour of lepidine from cinchonine.	Density of vapour of lepidine from coal.	Theory. $C_{20} H_9 N$ = 4 volumes.
5·14	5·15	4·94

On Cryptidine, a new Volatile Alkaloid homologous with Chinoline.

In examining the highest fractions of the bases from coal-tar, I have ascertained the presence of a new volatile base, to which I have given the above name.* The quantity at my disposal was so exceedingly small, that the platinum salt is the only compound I have been able to obtain in a state of tolerable purity; but the analyses of this substance leave no doubt whatever of the constitution of this the third homologue of the chinoline series.

If a solution of bichloride of platinum be added to a solution in hydrochloric acid of the fraction boiling about 525°, a pasty yellow mass precipitates, and, on stirring, adheres to the rod. In a few seconds the precipitate becomes crystalline, and is no longer adhesive, and, if it is now dissolved in boiling water, it separates on cooling in groups of yellow needles, sparingly soluble in cold water. Two specimens of salt prepared in this manner, and well washed, first with water, and after with a mixture of alcohol and ether, yielded on combustion the numbers following:—

I. { 8·018 grains platinochloride cryptidine, dried at 212°, gave
10·535 ... carbonic acid, and
2·493 ... water.

II. { 6·066 ... platinochloride of cryptidine, gave
1·645 ... platinum.

III. { 8·958 ... platinochloride of cryptidine (another preparation), gave
11·807 ... carbonic acid, and
2·647 ... water.

IV. { 5·990 ... platinochloride of cryptidine, gave
1·631 ... platinum.

	Experiment.				Mean.	Calculation.		
	I.	II.	III.	IV.				
Carbon, . . .	35·83	...	35·95	...	35·89	36·31	C_{22}	132
Hydrogen, . .	3·45	...	3·28	...	3·37	3·30	H_{12}	12
Nitrogen, . .	...	...	...	...	...	3·85	N	14
Chlorine, . .	...	...	...	...	...	29·30	Cl_3	106·5
Platinum, . .	...	27·12	...	27·23	27·18	27·24	Pt	99·0
						100·00		363·5

If the fraction boiling at 515°-25° is treated with ordinary nitric acid, it dissolves with a purple coloration, and, if the solution is evaporated to dryness, and redissolved in water, an insoluble yellow powder becomes apparent. To the filtered solution bichloride of platinum being added, an adhesive precipitate is formed, having the properties previously assigned to the platinochloride of cryptidine, as obtained from coal-tar. On solution in boiling water and subsequent cooling, a fine crop of orange-yellow needles was obtained, which, on combustion with chromate of lead and copper turnings, gave the result annexed.

* From κρυπτος.

I. { 7·921 grains platinochloride of cryptidine, after treatment with nitric acid, &c., dried at 212°, gave
10·603 ... carbonic acid, and
2·453 ... water.

II. { 5·108 ... platinochloride of cryptidine, same as last, gave
1·392 ... platinum.

	I. & II.	Calculation.
Carbon, . .	36·51	36·31
Hydrogen, . .	3·44	3·30
Nitrogen, . .	...	3·85
Chlorine, . .	...	29·30
Platinum, . .	27·25	27·24
		100·00

A very perceptible increase in the carbon is, therefore, obtained by the removal of the impurity rendered insoluble by means of nitric acid.

Chinoline is, therefore, the first of a series of homologous nitryl bases, of which three members are now known, viz.:—

Chinoline, . .	$C_{18} H_7 N$
Lepidine, . .	$C_{20} H_9 N$
Cryptidine, . .	$C_{22} H_{11} N$

And of which a greater number might possibly be obtained by an extension of the inquiry.

XXVII. *On Fermat's Theorem.* By H. F. Talbot, Esq., F.R.S., &c.

(Read 7th April 1856.)

It is well known that no satisfactory demonstration has ever been given of Fermat's celebrated theorem, which asserts that the equation $a^n = b^n + c^n$ is impossible, if a, b, c, are whole numbers, and n is any whole number greater than 2. In Legendre's *Théorie des Nombres*, he demonstrates the cases of $n=3$, $n=4$, and $n=5$, the latter only in his Second Supplement. In Crelle's *Mathematical Journal*, ix. 390, M. Dirichlet, a mathematician of Berlin, has demonstrated the case of $n=14$, but I am not aware whether his demonstration is considered successful. Legendre informs us (Second Supplement, p. 3) that the Academy of Sciences, with the view of doing honour to the memory of Fermat, proposed, as the subject of one of its mathematical prizes, the demonstration of this theorem; but the Concourse, though prolonged beyond the usual term, produced no result.

It is a remarkable circumstance, however, that Fermat himself was in possession of the demonstration, or at least believed himself to be so, and he describes his demonstration as being a wonderful one—*mirabilem sanè*.* He does not say that the theorem itself is wonderful, but his demonstration of it; from which I think it likely that he meant to say that it was very remarkable for its shortness and simplicity.

Since, however, subsequent mathematicians have failed to discover any demonstration, much less an extremely simple one, of this celebrated theorem, it has been surmised that Fermat deceived himself in this matter, and that his demonstration, if it had been preserved to us, would have proved unsatisfactory. Legendre says, "Fermat a pu se méprendre sur l'exactitude ou la généralite de sa démonstration."

Nevertheless, in considering this question attentively, I have found that there is one case in which Fermat's theorem admits of a singularly simple demonstration; and as I do not find it noticed in any mathematical work to which I have been able to refer, I think it worthy of being brought under the notice of mathematicians. It may possibly prove to be a step in the right direction towards the recovery of Fermat's lost demonstration. It is, moreover, in itself a very extended and remarkable theorem, although less so than that of Fermat.

* "Cubum autem in duos cubos aut quadrato-quadratum in duos quadrato-quadratos et generaliter nullam in infinitum, ultra quadratum, potestatem in duos ejusdem nominis fas est dividere. Cujus rei demonstrationem mirabilem sanè detexi. Hanc marginis exiguitas non caperet."—Fermat, *Notes sur Diophante*, p. 61.

The case which admits of this simple demonstration, is that in which one of the three numbers a, b, c, is a prime number; and it divides itself into the two following theorems:—

Let a be any prime number, then,

Theorem I. If n is any odd number greater than 1, the equation $a^n=b^n+c^n$ is impossible.

Theorem II. If n is any number, odd or even, greater than 1, the equation $a^n=b^n-c^n$ is impossible.

But Theorem II. admits a case of exception, viz., that whenever $b-c=1$, the theorem remains undemonstrated. When $n=2$, this case of exception actually occurs, because $a^2=b^2-c^2$ is possible, although a be a prime number. For example, when $a=3$, $3^2=5^2-4^2$.

Such a case of exception, however, does not occur when $n=3$, or $n=4$, or, $n=5$, as LEGENDRE has demonstrated. But that is no reason why it should not occur with other values of n. And therefore it appears that the generality of FERMAT'S theorem is assailable in this direction; a fact which deserves the attention of mathematicians, especially as FERMAT himself does not appear to have adverted to it.

In order to demonstrate these propositions, I will recall to mind some of the leading principles of the Theory of Numbers.

1. If a prime number does not divide either of the whole numbers A or B, it does not divide their product AB. LEGENDRE, p. 3, gives a very rigorous demonstration of this important theorem.

2. When a number has been divided into its prime factors, it cannot be divided into other prime factors different from the first ones.

3. The product of any number of primes, cannot be equal to the product of any number of other primes different from the first ones.

And here it may be observed, that although these products cannot be equal, nothing prevents them from approximating as closely as possible to equality, *i. e.*, differing by a single unit. For example, the product of the

three primes	$7 \times 17 \times 83$	$= 9877$
that of	$2 \times 11 \times 449$	$= 9878$
that of	$3 \times 37 \times 89$	$= 9879.$

These things being premised, we may proceed as follows:—

Demonstration of Theorem I.

Let us suppose, if possible, $a^n=b^n+c^n$. Then because n is an odd number, b^n+c^n is divisible by $b+c$. Let the quotient be Q. Therefore $a^n=\overline{b+c}\,.\,Q$. Now since a is prime, the first side of the equation is the product of the n factors $a\times a\times a\times$ &c. Consequently the second side of the equation is the product of

the same n factors. And it cannot possibly have any other. Therefore, since it has the factor $b+c$, this factor must itself be either $= a$, or divisible by a.

But we shall now proceed to show that $b+c$ cannot possibly be divisible by a, and therefore the original hypothesis, viz., that $a^n = b^n + c^n$, must be impossible.

In order to show this, we will first observe that $(b+c)^n$ is greater than $b^n + c^n$, since it exceeds it by the quantity $n\,b^{n-1}c + \frac{n \cdot \overline{n-1}}{2} b^{n-2}\,c^2$ + &c.

But $b^n + c^n = a^n$ by hypothesis.

$$\therefore \quad \overline{b+c}|^n > a^n, \text{ and } b+c > a.$$

On the other hand, since evidently

$$b < a \quad \text{and} \quad c < a, \quad b+c < 2a.$$

But since $b+c$ is greater than a and less than $2a$, it cannot possibly be divisible by a. Which was to be shown. And it therefore follows that the equation $a^n = b^n + c^n$ is impossible, if n is an odd number > 1, always supposing, however, that a is a prime number.

Demonstration of Theorem II.

Let us suppose, if possible, that $a^n = b^n - c^n$. Then since $b^n - c^n$ is always divisible by $b-c$, let the quotient be Q. Therefore $a^n = \overline{b-c} \,.\, Q$. Now since a is a prime number, the first side of this equation is the product of the n factors $a \times a \times a \times$ &c.

Consequently the second side of the equation is the product of the same n factors, and it cannot possibly have any other. Therefore, since it has the factor $b-c$, this factor must itself be either $= a$ or divisible by a. But, on the other hand, it can be shown, as follows, that it is not divisible by a.

Since $a^n = b^n - c^n$, therefore $b^n = a^n + c^n$, and b is the greatest of the three numbers. Now since $\overline{a+c}|^n > a^n + c^n$, and $a^n + c^n = b^n$, $\therefore \overline{a+c}|^n > b^n$, and $a+c > b$, and $a > b-c$.

Since, therefore a is greater than $b-c$, it cannot possibly divide it. And therefore the original hypothesis that $a^n = b^n - c^n$ is impossible, if n is any number > 1, always, however, on the supposition that a is prime.

But in reviewing this demonstration, we find a case of exception; for it will be seen that we assert that $b-c$ can have no factor different from a. This is correct in one sense, but not so in another, since it may have the factor *unity*, which is usually disregarded, though it is here a consideration of the greatest importance. And since we have shown that $b-c$ cannot have either the factor a or any other factor, it follows that it can have no factor except *unity*, that is to say, it must be itself equal to unity, and can have no other value.

The above two theorems form together a conclusive demonstration of FERMAT'S theorem, in the case of a prime number.

Extension of FERMAT'S *Theorem.*

Always supposing that a is a prime number, and that $b-c$ is greater than unity, the theorem $a^n=b^n-c^n$ (impossible), may be extended to a much more general theorem, viz., that $a^m=b^n-c^n$ is impossible, provided that m is less than n.

Demonstration. Let $a^m=k^n$, where we no longer suppose k to be an integer. Therefore since $m<n$, a must be greater than k. But $k^n=b^n-c^n$ by hypothesis; therefore $b^n=c^n+k^n$, which is less than $\overline{c+k}|^n$; therefore b is less than $c+k$, and $b-c$ is less than k. *A fortiori*, $b-c$ is less than a.

But in the given equation $a^m=b^n-c^n$, since a is prime, and $b-c$ divides b^n-c^n, therefore $b-c=a$, or else is divisible by a, a number which we have shown to be greater than itself, which is impossible. Therefore $a^m=b^n-c^n$ is impossible unless $m>n$. But if $m>n$, it is possible.

Example. $3^3=6^2-3^2$, where a is prime and $b-c$ greater than 1, but $m>n$.

By an analogous method we obtain the extended theorem No. II.

If n is an odd number, $a^m=b^n+c^n$ is impossible, provided that a is prime, and $m<n$.

We have hitherto supposed a to be prime, whereas FERMAT'S theorem has no such limitation; it remains, therefore, to enquire how far the present extended theorems are true when a is not a prime number.

In conclusion, we may oberve that the ancients themselves had discovered the possibility of the equation $a^2=b^2+c^2$.

But from what precedes, we may deduce the following theorems concerning it.

1. If $a^2=b^2+c^2$, and c is a prime number, then $a-b$ is always $=1$.

2. If $a^2=b^2+c^2$, b and c cannot *both* be prime numbers. For because c is prime, it follows that $b=a-1$.

And because b is prime, therefore $c=a-1$, therefore $b=c$, and $a^2=2b^2$.

But this is impossible, since one square cannot be double of another, in integer numbers.

Examples. $5^2=4^2+3^2$, and 3 being prime, we have $5-4=1$.

Again, $13^2=12^2+5^2$, and 5 being prime, we have $13-12=1$.

Again, $25^2=24^2+7^2$, and 7 being prime, we have $25-24=1$.

The converse, however, is not true. For if $a^2=b^2+c^2$, and $a-b=1$, it by no means follows that either b or c is a prime. For example, $221^2=220^2+21^2$, none of which numbers are primes.

XXVIII.—*On a Proposition in the Theory of Numbers.* By Balfour Stewart, Esq., of the Kew Observatory.

(Read 21st April 1856.)

Problem. If p be one of the roots of the equation $x^m-1=0$, (not 1,) then $(1-p)(1-p^2)\ldots(1-p^{m-1})=m$, provided m is a prime number.

If m be not a prime number, and if $p_1=\cos\frac{2\pi}{m}+\sqrt{-1}\,\sin\frac{2\pi}{m}$, the same will hold for all roots $p=p_1^{\alpha}$, where α is a number $<m$ and prime to m. But for all roots $p=p_1^{\alpha}$, where α, or one of its prime factors, is also a prime factor of m, the product $(1-p)(1-p^2)\ldots(1-p^{m-1})$ will be equal to 0.

Preliminary Propositions.

I. If m be a prime number, and α, β two numbers, each less than m; and if $\alpha\beta=\gamma m+\delta$, where δ is less than m; then δ is neither equal to α nor to β.

For if $\delta=\alpha$, we shall have $\alpha(\beta-1)=\gamma m$, where α and $\beta-1$ are both less than m, which clearly violates the well-known theorem that a number cannot be made up in two ways of prime factors.

II. Again, if $\alpha(\beta+\beta_1)=\gamma_1 m+\delta_1$ (where $\beta+\beta_1<m$); then δ_1 is not equal to δ.

For if $\delta_1=\delta$, then $\alpha(\beta+\beta_1)=\gamma_1 m+\delta$

$$=(\gamma_1-\gamma)m+\gamma m+\delta$$

$$=(\gamma_1-\gamma)m+\alpha\beta$$

therefore $\alpha\beta_1=(\gamma_1-\gamma)m$,

or a number is made up in two ways of prime factors, which is impossible.

III. If, therefore, we arrange in a horizontal row all the numbers in order of magnitude from 1 to $\overline{m-1}$ inclusive, and the same in a vertical row downwards, so that the two columns shall form the adjacent sides of a square, and if we multiply each successive number in the top horizontal column by the number in the vertical (as in the multiplication table), divide the product by m, and write down the remainders: then none of these remainders will be the same, for they will not be the multiplier itself (Prop. I.), nor, 2°, will any two be alike (Prop. II.). The multiplier and remainders will contain all the numbers from

1	2	3	4	5	6
2					
3	6	2	5	1	4
4	1	5	2	6	3
5					
6					

1 to $\overline{m-1}$ inclusive, though not in order of magnitude. We have exhibited this in the margin for $m=7$, the multipliers being 3 and 4.

IV. If m be not a prime, but composed of the prime factors $a \times b \times c \times d \times e \times f \times \ldots$, and if a or one of its prime factors (f) be also one of the prime factors of m, then in some case will $a\beta = \gamma m$, where β is one of the numbers $1, 2, 3, \ldots \overline{m-1}$.

For $\frac{m}{f}$ and $\frac{a}{f}$ are whole numbers $< m$; and $a \cdot \frac{m}{f} = \frac{a}{f} \cdot m$, which satisfies the condition.

V. If neither a nor any of its prime factors is also a prime factor of m, then there will be a remainder $\delta < m$ whatever be β.

For if not, let $a\beta = \gamma m$; then since $m > \beta \therefore \gamma < a$. Therefore γ is either prime to a, or there is a factor in a which is not in γ: but this factor is, by hypothesis, not in m: it is, consequently, not in γm, which is absurd.

VI. The remainders will be different for every different value of β. For if possible, let

$$a\beta = \gamma m + \delta$$
$$a(\beta + \beta_1) = \gamma_1 m + \delta$$
$$= (\gamma_1 - \gamma) m + a\beta$$

therefore, $a\beta_1 = (\gamma_1 - \gamma) m$ which, as in the last Prop., is impossible.

VII. If, then, we arrange the numbers as in Prop. III., we shall have 0 amongst the remainders, for all values of a which either divide m, or have a prime factor in common with it; whilst, for all other values of a, we shall have the same results as in Prop. III. This is shown for $m=8$ in the margin, for the multipliers 3, 4, and 6.

1	2	3	4	5	6	7
2						
3	6	1	4	7	2	5
4	0	4	0	4	0	4
5						
6	4	2	0	6	4	2
7						

Problem 1. If m be a prime number, the roots of the equation $x^m - 1 = 0$ are $1, p_1, p_1^2, \ldots p_1^{m-1}$ where $p_1 = \cos \frac{2\pi}{m} + \sqrt{-1} \sin \frac{2\pi}{m}$.

For, from the theory of equations,

$$x^m - 1 = (x-1)(x-p_1)(x-p_1^2) \ldots (x-p_1^{m-1})$$

or
$$x^{m-1} + x^{m-2} \ldots + 1 = (x-p_1)(x-p_1^2) \ldots$$

putting 1 for x,
$$m = (1-p_1)(1-p_1^2) \ldots (1-p_1^{m-1}).$$

If $p = p_1^{\alpha}$, since $p_1^m = 1$, $p_1^{\gamma m} = 1$;

and if $\alpha\beta = \gamma m = \delta$, $p_1^{\alpha\beta} = p_1^{\delta}$

hence, $(1-p_{1\alpha})\,(1-p_1{}^{2\alpha})\,\ldots\,(1-p_1{}^{(m-1)\alpha})$ will be the same as

$$(1-p_1{}^{\delta_1})\,(1-p_1{}^{\delta_2})\,\ldots\,(1-p_1{}^{\delta_{m-1}}),$$

where δ_1, δ_2, &c. are the remainders. But, by Prop. III., δ_1 δ_2, &c., are all different, and are the numbers 1, 2, . . . $\overline{m-1}$: hence,

$$\begin{aligned}(1-p)\,(1-p^2)\,\ldots\,(1-p^{m-1}) &= (1-p_1{}^{\alpha})\,(1-p_1{}^{2\alpha})\,\ldots \\ &= (1-p_1)\,(1-p_1{}^2)\,\ldots\,(1-p_1{}^{m-1}) \\ &= m.\end{aligned}$$

Problem 2. If m be not a prime number, the same equation is true, by Prop. VII., for $p=p_1{}^{\alpha}$, where α is less than, and prime to m; but if α or one of its prime factors is a prime factor of m, then one of the factors $(1-p_1{}^{\alpha})\,(1-p_1{}^{2\alpha})\,\ldots\,(1-p_1{}^{(m-1)\alpha})$ will be of the form $1-p_1{}^{\gamma m}=0$; and, consequently, the product itself will be equal to 0.

XXIX.—*On the Prismatic Spectra of the Flames of Compounds of Carbon and Hydrogen.* By William Swan, F.R.S.E.

(Read 21st April 1856.)

The phenomena presented by the prismatic spectra of flames have occupied the attention of many and excellent investigators. In most instances, however, no attempt has been made to procure accurate measurements of the positions of the bright lines which many of the spectra exhibit; and much in this field of observation, therefore, remains to be accomplished. I purpose, from time to time as I shall have leisure, to make a series of observations, whose object shall be the actual numerical determination of the positions of the bright lines in the spectra of flames; and I have commenced the series with an examination of the spectra of the flames of compounds of carbon and hydrogen. In an investigation into the phenomena of flames, the compounds of carbon and hydrogen claim our first attention, as constituting the most important means of artificial illumination; for it is scarcely necessary to remark, that, with the grand exception of sun-light, the combustion of these substances is the source of nearly all the light and heat from which we derive such extensive benefits in the arts and in domestic economy. It will be found, moreover, that the spectra of carbohydrogen flames possess, in common, remarkable features, which seem as yet to have received little attention, but which promise to be of service in explaining the general phenomena of artificial light.

If we examine the spectrum of the brightest part of the flame of an oil-lamp or a tallow-candle, it will be found that it exhibits no dark intervals, and that its colour and brightness vary gradually from point to point with scarcely any breach of continuity. If, however, we observe only the light proceeding from the blue part of the flame, which surrounds the upper part of the wick, a totally different result is obtained. The extreme red and violet rays become nearly or altogether invisible, and the intermediate portion of the spectrum exhibits a series of bright lines separated by dark intervals.* Similar lines occur in the flames of alcohol, sulphuric ether, and wood spirit. They are seen, however, with great difficulty in the flame of impure wood spirit, and are scarcely, if at all visible, in the more luminous flames of oil of turpentine and coal naphtha. Before offering an explanation of these differences, it will be necessary to premise some particulars regarding the nature of flame.

* For descriptions of lines in the spectrum of an oil lamp flame, see Fraunhofer, Astronomische Abhandlungen, 1823, p. 16; Herschel, Edin. Trans., vol. ix., p. 455, and article Light, Encyc. Metrop., art. 522. The spectrum of the blowpipe cone is described by Fraunhofer, Brewster's Journal of Science, vol. vii., p. 7; and by Draper, Phil. Mag., vol. xxxii., p. 111.

On the Nature of Flame.

We owe to Hooke, probably the first careful inquiry into the constitution of flame. More recently, the subject has been studied by Sir Humphry Davy and Professor Draper.*

The flame of coal gas or of a common candle, as is well known, consists of several portions, readily distinguishable by the eye, and in which the matter composing the flame exists in very different conditions. There is, first, the interior non-luminous portion, composed of gases not yet ignited; secondly, a blue conoidal shell, near the wick or burner, which, as it extends upwards, seems gradually to change its colour to a brilliant yellowish white; and, thirdly, an outer mantle or envelope of faintly luminous matter. On a careful examination, it will be found that the blue cone envelopes the white one; the blue, gradually thinning out towards the top, and the white, towards the bottom of the flame.

It has been supposed that, in the blue portion of the flame, the supply of oxygen is sufficient to insure the complete combustion of the gases, so that, in a carbohydrogen flame, there is the immediate production of water and carbonic acid.† The bright white light of the upper portions of the flame was proved by Sir Humphry Davy to proceed from the separation of solid carbon, which becomes brilliantly incandescent at the high temperature to which it is exposed, and which, when not converted into carbonic acid, escapes in the form of smoke.

The external mantle of the flame, according to Professor Draper, derives its light chiefly from incandescent carbonic acid and aqueous vapour.

While in the ordinary flames of coal gas and oil, solid carbon is separated, it is well known that by burning a mixture of gas and air, the separation of carbon may be entirely prevented, and a smokeless flame obtained. My attention was at first accidentally directed to the subject of this paper while using a species of gas lamp in which this object is effected in a very simple manner. As this lamp—the invention of Professor Bunsen of Heidelberg—has only lately been introduced‡ into this country, and as I have made extensive use of it in my experiments, it may be proper to explain its construction.

It consists of a common "union" or "bat-wing" gas burner, which, when used in the ordinary manner, would produce a flat, fan shaped flame. The burner is surrounded by a brass tube, 0·4 inch in diameter, and about 3 inches in height, having apertures immediately below the burner, which can be opened or closed, so as to admit a regulated supply of air. The gas issuing from the burner in a fan shape strikes obliquely the walls of the tube, and being reflected from

* Works of Sir H. Davy, vol. vi., Lond. 1840. Draper on the Production of Light by Chemical Action, Lond. Phil. Mag., 1848, vol. xxxii., p. 100.

† Kane's Chemistry, p. 289.

‡ By Dr Robert Ferguson, whose interesting account of the lamp will, I believe, appear in the Transactions of the Royal Scottish Society of Arts.

them, becomes effectually mixed with the air which enters at the bottom; and the mixture of gas and air, when lighted, burns at the top of the tube with a voluminous flame, without smoke. A thorough mingling of the gas and air is essential to the success of the arrangement, which will be found to fail when a plain burner is substituted for the "*union jet.*"

The flame of the Bunsen lamp consists of at least two distinct portions,—a luminous hollow cone of a strong bluish green colour, about two inches in height, and a very diffuse outer mantle, about 6 inches in height, reddish towards the interior, but externally of a pale lavender tint. In this, also, as in the ordinary flame of coal gas, but in much greater profusion, there is a perpetual scintillation of yellow sparks, arising apparently from foreign matter suspended mechanically, partly in the gas, and probably more abundantly in the air which enters the tube and mingles with the gas before combustion. This matter is projected continually through the walls of the flame, where it becomes for a moment incandescent.*

The light of the exterior envelopes of flames, is, I conceive, chiefly due to the presence of minute particles of solid matter, derived partly from the substance undergoing combustion, and often, as in the case of the Bunsen lamp, from the air which enters the flame. The outer envelope of the flame of the Bunsen lamp possesses so little inherent luminosity, that it is peculiarly susceptible of having its colour influenced by the accidental presence of foreign matter. As the salts of sodium are well known to be remarkably energetic in producing homogeneous yellow light, I made the following experiment, in order to ascertain how small a portion of matter could in this way render its presence sensible.

One-tenth of a grain of common salt, carefully weighed in a balance indicating $\frac{1}{100}$ of a grain, was dissolved in 5000 grains of distilled water. Two perfectly similar slips of platinum foil were then carefully ignited by the Bunsen lamp, until they nearly ceased to tinge the flame with yellow light; for to obtain the total absence of yellow light is apparently impossible. One of the slips was dipped into the solution of salt, and the other into distilled water, the quantity of the solution of salt adhering to the slip, being considerably less than $\frac{1}{20}$ grain, and both slips were held over the lamp until the water had evaporated. They were then simultaneously introduced into opposite sides of the flame; when the slip which had been dipped into the solution of salt, invariably communicated to a considerable portion of the flame a bright yellow light, easily distinguishable from that caused by the slip which had been dipped into pure water. It is thus proved, that a portion of chloride of sodium, weighing less than $\frac{1}{1,000,000}$ of a grain is able

* If the air be *dusty* from any cause, these scintillations become very abundant. Thus, if the floor be swept, or a piece of charcoal be scraped with a knife at a little distance from the lamp, minute particles are carried by the current of air into the tube, and cause a profusion of sparks, which exhibit a very beautiful appearance, while they confirm the opinion that the ordinary sparks are occasioned chiefly by particles of dust carried by the air.

to tinge a flame with bright yellow light; and as the equivalent weights of sodium and chlorine are 23 and 35·5, it follows, that a quantity of sodium not exceeding $\frac{1}{2,500,000}$ of a troy grain renders its presence in a flame sensible. If it were possible to obtain a flame free of yellow light, independently of that caused by the salt introduced in the experiment, it is obvious that a greatly more minute portion of sodium could be shown to alter appreciably the colour of the flame. It therefore follows, that much caution is necessary in referring the phenomena of the spectrum of a flame to the chemical constitution of the body undergoing combustion. For the brightest line in the spectrum of the flame of a candle,—the yellow line R* of FRAUNHOFER,—can be produced in great brilliancy, by placing an excessively small portion of salt in a flame, in whose spectrum that line is faint or altogether absent. The question then arises, whether this line in the candle flame is due to the combustion of the carbon and hydrogen of which tallow is chiefly composed, or is caused by the minute traces of chloride of sodium contained in most animal matter. When indeed we consider the almost universal diffusion of the salts of sodium, and the remarkable energy with which they produce yellow light, it seems highly probable that the yellow line R, which appears in the spectra of almost all flames, is in every case due to the presence of minute quantities of sodium.

The view, which would attribute a great portion of the light of the envelopes of flames to the adventitious presence of minute traces of foreign matter, may possibly serve to explain certain anomalous diversities of colour which are observed in the envelopes of flames arising from the combustion of the same elements. Thus tallow, coal gas, anhydrous alcohol, and weak spirit of wine, all contain the same combustible substances, carbon and hydrogen: yet the envelope of the flame of a candle is bright yellow, that of a coal gas flame is purple, and those of strong alcohol and weak spirit differ greatly in luminosity.

It is important also to remark, that while the luminosity communicated to the exterior envelope of a flame by such substances as the salts of sodium or of copper, may be so great as to disguise that of the inner bright cone of the flame, or in some cases to render it altogether invisible; yet I have ascertained that the light of the blue portion of the flame, or of the inner cone, remains absolutely unchanged in colour and intensity. The proof of this curious property of flame will be given in the sequel.†

Prismatic Analysis of Flame.

Reserving, meantime, a more complete description of the apparatus I have employed, it may be sufficient to premise, that, in what I shall have to say regarding the spectra of flames, the object observed is supposed to be a narrow illuminated slit, viewed through a glass prism mounted before a telescope, which has been adjusted to focus on the slit.

* SCHUMACHER's Astronomische Abhundlungen, 1823, p. 18. † See p. 419.

It has already been stated, that certain carbohydrogen flames afford spectra exhibiting bright lines separated by dark spaces. In no spectrum are these lines more easily observed than in that of the Bunsen gas lamp. In order to distinguish the phenomena of the spectrum which are due to different portions of the flame, it is sufficient to place the lamp before a narrow vertical slit not exceeding 0·2 inch in *height*, through which its light passes to the prism.* If the lamp be gradually raised before the slit, the spectrum first seen will be derived exclusively from the envelope of the flame, which reaches high above the top of the interior cone. This spectrum is tolerably bright, extending without the least interruption, from the line C, nearly to the line H of FRAUNHOFER, and exhibiting no bright line whatever except the yellow line R. That line, however, is extremely flickering, so as often to disappear completely; and it seems due entirely to the yellow scintillations which abound in the exterior envelope. When the flame is raised still higher, so as to bring the top of the green cone into view, four other bright lines begin to appear; and as we continue to raise the flame so as to derive light from lower and lower portions of the flame, the bright lines become more and more clearly defined, owing to the intervening spaces becoming darker; and some fainter lines become visible. At length, when light passes through the slit only from the lowest portions of the flame where the exterior envelope nearly disappears, the bright lines become so sharply defined as to admit of their places being ascertained by actual measurement, with almost the same accuracy which is attainable in observations of the dark lines of the solar spectrum.

From the facility with which the lines of the carbohydrogen spectrum are obtained, I conceived they might be of use in optical researches; and I soon found them of great service in the prosecution of my experiments. I was, therefore, anxious to ascertain whether they belonged really to the gas flame, or were caused by the accidental presence of foreign matter; for it is well known that some metals, such as copper, when present in a flame, produce bright lines in its spectrum. For this purpose I burned a mixture of coal gas and air, successively, from an iron tube, a glass tube, a tube formed of a coil of platinum foil, and the brass tube of the Bunsen lamp; but in every case the lines remained unchanged in number and position, proving that they arose entirely from the combustion of the gas, and not from any matter derived from the lamp.

On the Apparent Diversity of the Spectra of Compounds of Carbon and Hydrogen.

Having thus studied the general phenomena of the spectra of carbohydrogen flames, some of which exhibit continuous, and others interrupted spectra, we may now resume the question, Whence do these differences arise?

It has been found by Professor DRAPER that an incandescent solid body emits

* Or we may adopt Professor DRAPER's ingenious mode of observing flames through a *horizontal* slit, Phil. Mag., vol. xxxii. p. 106.

light of every degree of refrangibility between limits varying with its temperature. Thus, when carbon burns in oxygen, or when a strip of platinum is heated, to a temperature of 2130° Fahr., by the passage of a current of electricity, a perfectly continuous spectrum is produced, without any bright lines or dark spaces, and extending at least from the line B to the line H of the solar spectrum.* This enables us to explain why we may see, in the spectrum formed by the blue portion of a flame, bright lines and dark spaces, which are totally invisible in the spectrum of the bright inner cone. For the light of the inner cone arises from incandescent solid carbon deposited in the flame, which, as we have just now seen, must produce a brilliant continuous spectrum. The light of this spectrum overpowers the comparatively faint illumination of the bright lines of the spectrum formed by the blue part of the flame, while it fills up the dark intervals between them; and both causes conspiring render the lines invisible. Again, lines may easily be seen in the spectrum of the blue part of a spirit lamp or candle flame, which fail to show themselves when we examine the flames of oil of turpentine or coal naphtha; for the latter bodies contain so much carbon that it begins to be deposited almost at the very bottom of the flame. The blue conoid is thus reduced to an extremely narrow ring; and it is practically impossible, however small the aperture through which light passes to the prism, to obtain the spectrum of the blue light separated from that of the incandescent carbon.

We can also similarly explain why lines may be visible in the spectrum of alcohol, which may not be easily seen in that of weak spirit of wine, or of impure wood spirit. The exterior envelope, like the interior bright cone, derives most of its light from incandescent solid matter, and produces a continuous spectrum, as was shown in the case of the Bunsen lamp. Now the exterior envelope of the flame of weak spirit of wine, or of wood spirit, is very voluminous and fully developed, and hence of unusual thickness near the bottom of the flame. The light derived from the incandescent matter it contains, will therefore operate precisely like that of the interior luminous cone, in rendering the bright lines of the spectrum invisible.

On Methods of Observing the Spectra of Carbohydrogen Flames.

Since the continuous spectra due to the light of incandescent matter offer no distinguishing features, it follows, that in searching for phenomena characteristic of the chemical constitution of bodies undergoing combustion, we must examine that part of the flame in which any solid molecules, which are being deposited, have not been able to collect into masses. This state of things exists in the blue portion of the carbohydrogen flames, where the supply of air is sufficient completely to consume the gases. In flames, such as that of oil of turpentine, $C_{10}H_8$, where there is much carbon, it becomes necessary to burn the carbon by an

* Phil. Mag., vol. xxx., p. 349.

artificial supply of air. Two methods occurred to me of effecting this object, so as to convert the carbon into carbonic acid, without its intermediate separation in a solid form. One was to burn the vapours of the substances under examination in the Bunsen lamp; but this I rejected as inconvenient, and perhaps even in some cases dangerous, from the risk of explosion, where it would have been necessary to boil highly volatile liquids in close vessels. The other was simply to pass a stream of air through the flame by means of a table blowpipe. By means of the latter expedient I succeeded so completely in preventing the separation of solid carbon, as to obtain spectra with bright lines and dark spaces, in the case of every compound of carbon and hydrogen which I have as yet submitted to examination.

Comparison of the Spectra of the Flames of various Substances containing Carbon and Hydrogen.

The hydrocarbon compounds which I have examined, and which are enumerated in the following table, may be divided into two classes; one consisting of substances containing only carbon and hydrogen, of which the general formula is $C_r H_s$, and the other of substances containing carbon, hydrogen, and oxygen, represented by the formula $C_r H_s O_t$.

	$C_r H_s$
Light carburetted hydrogen,	$C H_2$
Olefiant gas,	$C_2 H_2$
Paraffin,	$C_{20} H_{20}$
Oil of turpentine,	$C_{10} H_8$

	$C_r H_s O_t$
Methylic alcohol,	$C_2 H_4 O_2$
Alcohol,	$C_4 H_4 O_2$
Ether,	$C_4 H_5 O$
Methylic ether,	$C_2 H_3 O$
Glycerine,	$C_6 H_8 O_6$
Spermaceti,	$C_{64} H_{66} O_4$
Camphor,	$C_{10} H_8 O$
Wax, Tallow, Coal gas, Coal naphtha,	Of indefinite composition.

Of these substances, the light carburetted hydrogen was made by heating acetate of soda, hydrate of potassa, and quicklime; and the methylic ether from wood spirit and sulphuric acid. The gases were generally burned from a platinum jet, immediately after passing through a tube filled with pieces of quicklime. The glycerine, a substance which burns with difficulty, was heated in a platinum capsule; and the paraffin, camphor, and spermaceti, which were colourless, crystalline, and apparently pure specimens, were also similarly treated, in order to corroborate the conclusion stated at p. 415, that the lines observed in the spectra were

all due to the combustion of the carbohydrogen compounds, and not to the presence of foreign matter. The alcohol, ether, and other liquids, were burned in lamps made of small phials,—a glass tube furnished with a cotton wick serving as a burner.

Taking the spectrum of the Bunsen lamp as a standard, the spectra of the other flames were compared with it, by viewing both simultaneously,—the light from the two flames passing through the same narrow slit.

The result of this comparison has been, that, in all the spectra produced by substances, either of the form $C_r H_s$ or of the form $C_r H_s O_t$, the bright lines have been identical. In some cases, indeed, certain of the very faint lines, which occur in the spectrum of the Bunsen lamp, were not seen. The brightness of the lines varies with the proportion of carbon to hydrogen in the substance which is burned, being greatest where there is most carbon. Thus, in the spectra of light carburetted hydrogen, pyroxylic spirit, and glycerine—substances which contain comparatively little carbon—certain of the fainter lines of the Bunsen lamp spectrum were not seen; but all those that *were* seen were identical with the lines of the coal-gas flame. I have no doubt that the fainter lines were really present, but were invisible, merely owing to their feeble luminosity; and this is rendered more probable by the fact that the number of lines visible in any spectrum varies with the brightness of the light. Thus in the solar spectrum, or in that of the Bunsen lamp, the fainter lines disappear when the intensity of the light is diminished.

The absolute identity which is thus shown to exist between the spectra of dissimilar carbohydrogen compounds is not a little remarkable. It proves, 1*st*, that the position of the lines in the spectrum does not vary with the proportion of carbon and hydrogen in the burning body; as when we compare the spectra of light carburetted hydrogen, $C H_2$, olefiant gas, $C_2 H_2$, and oil of turpentine, $C_{10} H_8$; and, 2*dly*, that the presence of oxygen does not alter the character of the spectrum; thus, ether, $C_4 H_5 O$, and wood spirit, $C_2 H_4 O_2$, give spectra which are identical with those of paraffin, $C_{20} H_{20}$, and oil of turpentine, $C_{10} H_8$.

In certain cases, at least, the mechanical admixture of other substances with the carbohydrogen compound does not affect the lines of the spectrum. Thus I have found that a mixture of alcohol and chloroform burns with a flame having a very luminous green envelope—an appearance characteristic of the presence of chlorine—and no lines are visible in the spectrum. When, however, the flame is urged by the blowpipe, the light of the envelope is diminished, and the ordinary lines of the hydrocarbon spectrum become visible.

Comparison of the Carbohydrogen and Solar Spectra.

Having ascertained, that probably all substances of the forms $C_r H_s$ and $C_r H_s O_t$ produce, when burning, spectra which are absolutely identical, I was desirous to compare their spectra with that of sun light.

For this purpose I at first attempted to view the solar spectrum and that of the Bunsen lamp simultaneously, but the great comparative faintness of the latter rendered that mode of comparison exceedingly difficult. I therefore determined to measure separately the minimum deviations for the principal lines of the solar and gas spectra; the intervals between the adjacent smaller lines of the latter spectrum being ascertained by means of a micrometer.

The instruments I employed were an excellent theodolite by ADIE, and a very fine flint-glass prism by SECRETAN of Paris, whose faces have an area of four square inches, and which shows, with great distinctness, the finest lines in FRAUNHOFER's map of the solar spectrum. I am indebted for the use of these instruments to the kindness of Mr JOHN ADIE and Professor FORBES.

The prism being placed in its position of minimum deviation, the indices of refraction given in the sequel were calculated by the formula

$$\mu = \frac{\sin \frac{1}{2}(I + D)}{\sin \frac{1}{2} I};$$

where I is the angle of the prism, and D the deviation of the transmitted light.

I have denoted the five brightest lines of the carbohydrogen spectrum by the letters, α, β, γ, δ, ζ; and the fainter lines by which they are accompanied by β_1, β_2, γ_1, &c. In the tables, D_A, D_γ, μ_A, μ_γ, &c., denote respectively the minimum deviation of the rays, and the index of refraction for the lines A and γ of the solar and flame spectra.

A comparative diagram of the spectra of sunlight and the hydrocarbon flames is given in Plate VIII., fig. 1, where α is the double yellow line R of FRAUNHOFER. I have thought it advisable to introduce this line in the diagram, as it is almost constantly visible in ordinary artificial light, although, for reasons already fully stated, I conceive it is not peculiar to the spectra of carbohydrogen compounds. This conclusion is strongly corroborated by the remarkable phenomena pointed out at p. 414, namely, that the salts of sodium tinge the exterior envelope of the Bunsen lamp flame with so brilliant a yellow light, as completely to overpower the comparatively feeble blue light of the inner cone, and to render it altogether invisible; while yet the light of that portion of the flame remains absolutely unchanged. This remarkable property of flame is easily demonstrated by holding a slip of platinum, with some salt placed on it, in the flame, while the spectrum is observed through a telescope. The instant the salt reaches the flame, the yellow line R or α, which before may have been extremely faint, or altogether

imperceptible, shines out in great brilliancy; while the lines β, γ, δ, and ζ remain totally unchanged in position, colour, and intensity.

While the line α is thus exceedingly variable in its brightness, the lines β, γ, δ, and ζ, on the other hand, are perfectly steady; and being never absent in carbohydrogen spectra, there is every reason to believe that they are really characteristic of the body undergoing combustion. Beyond α on the less refracted side there is a faint trace of red light, which, as it becomes so feeble as almost to disappear when the light is derived from the lowest point of the flame of the Bunsen lamp, is probably due to the exterior envelope of the flame, and not to the interior cone. The line α is separated from β by an extremely dark space, almost destitute of light. The line β is of a faint yellowish green colour, but well defined, and is accompanied by four almost equidistant lines β, β_2, &c., which diminish in brightness as their distance from β increases. After another very dark interval, the extremely beautiful line γ follows, which is exceedingly brilliant, and of such absolutely definite refrangibility as, like α, to form a perfectly sharp image of the slit through which the light passes. Its colour is a fine slightly bluish or pea green, and it is accompanied by a fainter line γ_1. The next line δ is the less refracted edge of a broad band of light containing four fine lines. This group, which is of a pale ashy colour, is separated by dark intervals from γ and ζ. The line ζ belongs to a brilliant but not very well defined band of a fine purple tint, which is accompanied by a fainter line ϵ.

I have completed observations of the minimum deviations for the lines α, β, γ, δ, and ζ; and also for the principal lines of the solar spectrum, which are given in Series 1, Tables II. and III., pp. 427, 428. From an examination of these tables it appears, that while several lines in the carbohydrogen spectrum coincide nearly in position with remarkable lines in the solar spectrum; yet in no case, if we except the line α, has the observed coincidence been exact. The observations, therefore, rather tend to prove that the *bright lines* of the carbohydrogen spectrum coincide, not with the *dark lines*, but with the *bright spaces* of the spectrum of sun light.

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*Postscript added since the preceding Paper was read.**

From the well known coincidence discovered by FRAUNHOFER, to exist between the line R, in the spectrum of a lamp, and D of the solar spectrum, taken in connection with similar phenomena, which have since been observed, it might be inferred, as a general law of the spectra of flames, that their bright lines always coincide with dark lines of the solar spectrum.

The result of the investigations which have now been detailed, is obviously unfavourable to such a conclusion. In publishing observations bearing on a question of so much interest and importance, I was anxious, if possible, to leave no

* Printed by permission of the Council.

doubt as to their accuracy; and since the preceding paper was read, I have made a much more extensive series of experiments, than the limited time I can devote to such researches, had then enabled me to overtake, involving the determination, with more or less accuracy, of the positions of all the bright lines in the carbo-hydrogen spectrum, whose presence I have been able to detect. These experiments, with some account of my methods of observation, I have deemed it desirable to append to the preceding paper.

Methods of Observation.

The theodolite A, fig. 2, which was used in measuring the deviations of the refracted rays, has a limb 7·5 inches in diameter, with two verniers reading 10″, and a telescope B, of 1·6 inch aperture, furnished with a parallel wire micrometer. The stage carrying the prism P, furnished with screws to render its faces perpendicular to the divided circle, was mounted over the centre of the theodolite; and, in order to avoid parallax, the object viewed was an extremely narrow slit placed in the principal focus of the object glass of a 30 inch telescope C L, which thus acted as a collimator.* The telescope rested in Ys, in a solid cast-iron stand, D D, which also carried the theodolite: so that the collimator preserved an invariable position in relation to the theodolite, notwithstanding any instability of the floor of the room in which the observations were made; and the zero of the circle was found to remain exceedingly constant. A diaphragm with a vertical slit was placed before the collimator lens, so as to limit the aperture in the plane of refraction to 0·4 inch, and thus to allow only a nearly central pencil of rays to fall on the prism. Any errors, which might have arisen, either from imperfect adjustment of the collimator to sidereal focus, or from defective aplanatism in its lens, were thus avoided as much as possible.

The deviations of the refracted rays were observed first to the right, and then to the left,—the prism being always adjusted to its position of minimum deviation,—so that the difference of the readings of the verniers in the two positions of the prism, gave double the minimum deviation of the refracted rays.

The angle of the prism was ascertained, by first turning it with its edge towards the object glass of the telescope, as represented in fig. 3, where ABC is the prism, and T the telescope. The stage carrying the prism was rigidly connected with the telescope, so that when the telescope was moved, the prism moved along with it; and being left undisturbed, the inclinations of its faces AB, AC, to the line of collimation of the telescope remained invariable. The telescope was then turned, until the image of the illuminated slit of the collimator, seen by reflection, successively in the two faces of the prism, was made to coincide with the telescope wires; and, at each intersection, the verniers were read off. The difference

* I have described this mode of observation in my paper on the Ordinary Refraction of Iceland Spar, Edin. Trans., vol. xvi.

of the readings then gave double the angle of the prism. For, if DGI, FHK represent the course of the reflected rays, since the telescope has been adjusted to sidereal focus, GI and HK, must be parallel; and the angle DEF, will obviously be double the angle BAC. Now, since DE and EF, are at the times of observation successively in the *same* direction, namely, that of the parallel rays emerging from the collimator, it follows that the telescope must have been turned through the angle DEF. Hence the difference of the readings is DEF, or twice BAC.

In order to test the adjustment of the collimator to sidereal focus, I made two series of observations of the angle of the prism given in Table I.; Series I. having been made by means of the collimator, and Series II. on a definite point of the tower of St Stephen's Church, distant about 2240 feet, where the parallax due to any difference in the directions of the rays incident on the two faces of the prism could not have caused an error exceeding 4″ in the measured angle.

These results agree so closely as to show that any want of parallelism in the rays emerging from the collimator, arising from want of perfect adjustment to sidereal focus, could not have appreciably affected the observations of the absolute deviations of the refracted rays. I may also observe, that since, during the observations of the carbohydrogen and solar spectra, the whole apparatus remained unaltered, any want of parallelism in the rays incident on the prism, whether arising accidentally from imperfect adjustment of the collimator, or necessarily from the unavoidable want of perfect achromatism in its lens,—for either cause might modify the apparent direction of the observed object, if the pencil of rays incident on the prism were not accurately *central*,—would affect the observed deviations in the two spectra alike. The accuracy of the observations, viewed merely as affording a comparative view of the relative positions in the scale of refrangibility occupied by the lines in the two spectra, would thus remain entirely unimpaired.

I have ascertained, however, by actual experiment, that the observations of *absolute* deviation cannot have been sensibly affected by any want of achromatism in the lens of the collimator. Having caused the telescope wires to coincide accurately with the image of the collimator slit, I illuminated the slit alternately with the extreme red and the extreme violet rays of the solar spectrum formed by a flint-glass prism. I then found that the image of the slit did not in the slightest perceptible degree alter its apparent position; so that, while the illumination was changed from red to violet light, the wires continued to bisect the slit with perfect accuracy.

As the spectrum of the Bunsen lamp is so faint that the telescope wires, when projected on all but its brightest lines, are invisible, it became necessary to illuminate the wires; but I speedily found that, from the feeble luminosity of the spectrum, observations with an illuminated field were nearly impracticable, and I was therefore obliged to observe with illuminated wires on a dark field. The arrangement for illuminating the wires which I devised is so simple, and

proved so successful, that I venture to describe it, in the hope that it may prove useful in similar researches. A hole, *a* (see fig. 4), 0·1 inch in diameter, was drilled in the side of the tube in which the eye piece slides, at a point between the field lens of the eye piece and the wires *w*; a small lamp, L, furnished with a condensing lens *c*, and a conical tube with a small aperture *e*, through which alone light was allowed to pass, was attached to the telescope, so that the light, indicated by *r r*, emerging from the conical tube, and entering the hole in the eye piece tube, crossed the axis of the telescope at an angle of about 70°, so as to illuminate the intersection of the wires at *w*, on the side *next* the eye, while all the rest of the field remained perfectly dark. By slightly varying the position of the lamp, the illumination of the wires could be adjusted with the utmost nicety to suit the brightness of that portion of the spectrum which was under examination.

Notwithstanding the most careful adjustment of the illumination of the wires, I still found the observation of the fainter lines of the carbohydrogen spectrum extremely difficult. The brightness of the lines in the spectrum of the Bunsen lamp is, however, considerably augmented by urging the flame by the blowpipe; and I found it useful to employ three jets placed one behind another, so that the combined illumination of three blowpipe cones might fall upon the prism. This apparatus, which is useful in exhibiting the fainter lines of the carbohydrogen spectrum, is easily constructed by forming three blowpipe jets of glass tube, about 0·2 inch in diameter, in the ordinary manner, and placing them, side by side, in a perforated cork. The cork is then inserted in a short piece of wide tube, having at its other end a second cork, connected with a flexible tube conveying a current of air from a table blowpipe.

I have also carefully compared, by simultaneous observations, the spectrum of the Bunsen lamp flame urged by a jet of oxygen gas, with the spectrum obtained by means of the triple air blowpipe. The lines in the two spectra were almost equally bright, and differed neither in number nor in position.

In the observations, Series I., Tables II. and III., I used an eye piece giving a magnifying power of 11, which was afterwards superseded by another magnifying 21 times, with which Series II. was made.

Comparison of the Carbohydrogen and Solar Spectra.

The second series of observations having been made with a higher magnifying power, and in some other respects also in more favourable circumstances than the first, is to be regarded as more trustworthy; yet the results of both agree so closely, that any additional accuracy which might have been obtained by ascertaining, separately, the probable errors of the two series, and their most probable result, when combined, could scarcely have repaid the labour of the necessary computation. I have, therefore, deemed it sufficient to give all the observa-

tions equal weight, and to take simply the arithmetical mean of the whole. The mean results of the two series, and the number of observations in each, being tabulated separately, the reader will be able to form some judgment regarding the probable accuracy of the final determinations obtained from the two series combined. In Fig. 1, which is a graphic construction of the observations in Tables II. and III., the lines were drawn by the engraver through points laid down by me on the copper to a scale,—adopted to suit the size of the plate,—of one inch to 2200″. I have ascertained the errors in the positions of these lines to amount, in one case only to ·01 inch (corresponding to 22″), and to be generally much less; so that the spectra are represented in the figure with tolerable fidelity.

In addition to the observations of the carbohydrogen and solar spectra contained in Table II., where the deviation for each line of either spectrum was separately determined by the theodolite or micrometer, I have also made simultaneous observations of the spectra of sun light and of olefiant gas. The gas, which was prepared by heating alcohol with sulphuric acid, was conducted through wash bottles containing caustic potash and sulphuric acid, to a gas holder; from which it afterwards passed, through a tube filled with pieces of quicklime, to a platinum jet where it was burned.

The lines in the spectrum of olefiant gas are very distinct, being well seen without using the blowpipe; but like the lines in the other carbohydrogen spectra, they are not sufficiently luminous to be seen when projected on the solar spectrum, unless the latter is made so faint, that its lines have disappeared. I succeeded, however, in observing the spectra simultaneously, by intercepting the sun light which fell upon one half of a narrow slit, and illuminating the whole slit with the flame of olefiant gas. The gas spectrum then appeared immediately over that of the sun, and the *brighter* lines in it were well seen, especially when the flame was urged by the blowpipe. The intervals between the lines of the gas spectrum and the nearest lines of the solar spectrum, given in Table V., were measured by the micrometer, with a magnifying power of 21; and the observations for the brighter line β, γ, and δ, agree well with those of Table III.

The line α was rarely visible in the spectrum of olefiant gas, and its appearance was only momentary, which confirms the opinion already stated, that it does not properly belong to the carbohydrogen spectra. To the proof already adduced in support of this opinion, I may here also add, that I have found it permanently absent in the flames of carbonic oxide, and of light carburetted hydrogen.* The continued invisibility of so brilliant a line of the spectrum, coupled with its

* I have found that the column of heated air rising from the flame of a spirit lamp with a salted wick, is most energetic in communicating yellow light to the exterior envelope of the flame of the Bunsen lamp. This effect is apparently confined to the outer, or oxidizing portion of the flame, where there is no excess of hydrogen, to decompose the chloride of sodium; and the experiment is interesting, as tending to prove that the yellow light may be caused by simple *incandescence*, without the actual *combustion* of sodium.

almost instantaneous appearance at very long intervals,—for it did occasionally appear for a moment,—satisfactorily proved it to be due merely to foreign matter which had accidentally entered the flame.

From an examination, either of Table IV., or of Plate VIII., fig. 1, it will be seen that certain of the lines in the carbohydrogen spectrum occupy nearly the same places in the scale of refrangibility with dark lines in the solar spectrum. These are the lines α, γ, δ_3, and ζ, which coincide more or less exactly with the lines D, b_2, F_1, and G. The first of these coincidences has been long known, having been discovered by FRAUNHOFER;* and similar remarkable relations have been observed by Sir DAVID BREWSTER to exist between certain lines in the spectrum produced by "deflagrating nitre," and the corresponding lines of the solar spectrum.†

From these singular coincidences occurring in so many different cases, the inference might be drawn, that *all* bright lines in the spectra of flames coincide with dark lines in the solar spectrum; and the extremely close proximity of the lines γ and b_2, δ_3 and F_1, ζ and G indicated in Table IV., might at first sight seem to confirm such an opinion. For it might be argued, that so close agreements in the ascertained deviations indicate absolute identity; the minute differences observed being attributed simply to errors in the observations. It will be seen, however, that the observed deviations of the lines b_2 and γ differ by no less a quantity than 40″, which is quite beyond the sum of the probable limits of error in the observations for these lines, which I have ascertained to be only about 5″; and thus their coincidence is shown to be highly improbable.

But any remaining doubt on the subject is completely removed, by the simultaneous observations of the spectra of sun light and olefiant gas, given in Table V., where the micrometrical measurement of the interval between the lines b and γ differs only by 11″ from that obtained by the theodolite observations. In fact, the bright line γ was *seen* when the spectra were viewed simultaneously, to coincide, not with the dark line b_2, but with the clear space immediately beyond it. If we omit the line α, which, for reasons already fully stated, I do not regard as properly belonging to the carbohydrogen spectrum, not one of the other twelve lines which I have observed in that spectrum occurs near any conspicuous dark line of the solar spectrum, with the exception of the lines γ, δ_3 and ζ, which fall near b_2, F_1 and G. Now, of these, γ has been proved beyond doubt, not to coincide with b_2, but with a bright space in its vicinity; and from the simultaneous observation of the spectra of sun light and of olefiant gas, as well as from the results of the theodolite observations, I believe that the other *bright lines* of the carbohydrogen spectrum also coincide not with *dark lines*, but with *bright spaces* in the solar spectrum.

* SCHUMACHER's Astronomische Abhandlungen, 1823, p. 29. See also BREWSTER's Edinburgh Journal of Science, vol. viii., p. 7. M. FOUCAULT has lately verified this result with the double yellow line seen in the spectrum of the voltaic arc, between charcoal electrodes. See DE LA RIVE's Electricity, vol. ii., p. 322.

† Report of British Association, 1842, p. 15.

From the fact just stated, that most of the lines in the carbohydrogen spectrum occupy positions where, in the solar spectrum, no conspicuous dark lines occur, direct comparison of the spectra, by simultaneous observation, seems almost impossible; for, before the fainter lines of the carbohydrogen spectrum become visible, the solar spectrum must be rendered so faint, that its finer lines have disappeared. On the other hand, to make a complete comparison of the spectra by actually measuring with the theodolite, the positions of the finest lines of the solar spectrum, would be a most formidable task. For when we consider that FRAUNHOFER has represented on his map of the solar spectrum, 350 lines, while Sir DAVID BREWSTER, by the aid of very excellent optical means, has observed the spectrum to be "divided into more than 2000 visible and easily recognized portions, separated from each other by lines more or less marked,"* it follows, allowing 5° for the angular dispersion of the extreme rays of the spectrum,—that the average interval between the lines observed by him is only 9″. Extremely delicate theodolite measurements would therefore be required, in order to determine, whether or not any bright line of a flame spectrum was or was not coincident with one or other of the numerous small lines of the solar spectrum; and even where a coincidence was ascertained, it might be fairly attributed to chance, just as a binary star may be only optically, and not necessarily physically double.

In cases, however, where there is a remarkable analogous configuration of two groups of lines, accompanied by exact coincidence, as between the double lines a and D; and more especially where we actually view the striking phenomenon of the lines in the spectra optically superimposed, the impression of some physical connection between the two groups becomes irresistible.

The coincidence of γ, the most brilliant line of the carbohydrogen spectrum, with the clear space immediately beyond b_2,—the most refracted line of a group, which, whether we regard the singular configuration or the strength of the lines which compose it, is perhaps the most notable in the solar spectrum,—is a phenomenon which seems deserving of attention as probably indicating also some physical relation.

In conclusion, I may observe, that from the facility with which, by means of the Bunsen lamp, the carbohydrogen spectrum may be obtained, and from the definite and readily identifiable character of the lines which compose it, these lines may be useful in optical researches, where, from any cause, sun light cannot be employed. It will be seen, from Table IV., that, for most practical purposes, the lines a, γ, δ_3, and ζ, may be assumed as identical with D, b_2, F_1, and G of the solar spectrum; any error in the index of refraction calculated on that assumption, affecting only the fourth or fifth place of decimals.—*7th June.*

* Edinburgh Trans., vol. xii., p. 528.

TABLE I. *Observations of the Angle of the Prism.*

SERIES I.		SERIES II.		Total Number of Observations.	Mean of all the Observations.
Number of Observations.	Mean of the Observations.	Number of Observations.	Mean of the Observations.		
5	60° 0′ 15″	3	60° 0′ 18″	8	60° 0′ 16″

TABLE II. *Observations of the Solar Spectrum.*

Line of Spectrum.	SERIES I.		SERIES II.		Difference of Means I. and II.	COLLECTED OBSERVATIONS OF MINIMUM DEVIATIONS.	
	Number of Observations.	Mean I.	Number of Observations.	Mean II.		Total Number of Observations.	Mean of all the Observations.
A	3	47° 20′ 27″	3	47° 20′ 21″	0′ 6″	6	47° 20′ 24″
a	2	47 29 54	4	47 30 2	0 8	6	47 29 59
B	2	47 39 52	3	47 39 51	0 1	5	47 39 51
C	2	47 50 22	3	47 50 10	0 12	5	47 50 12
C_1*	...	...	1	48 0 50	...	1	48 0 50
D	2	48 18 15	4	48 18 12	0 3	6	48 18 13
E	2	48 55 24	4	48 55 26	0 2	6	48 55 25
b†	4	49 1 45	5	49 1 44	0 1	9	49 1 45
b_1	2	49 2 37	1	49 2 35	0 2	3	49 2 36
b_2	2	49 2 55	3	49 2 56	0 1	5	49 2 55
F	2	49 29 9	3	49 29 19	0 10	5	49 29 15
F_1*	...	...	4	49 49 2	...	4	49 49 2
G	2	50 35 2	3	50 35 5	0 3	5	50 35 4
H	1	51 34 50	6	51 34 43	0 7	7	51 34 44
	Mean Temp. of Prism, 53·3 F. Mean Barom. . . . 30·21		Mean Temp. of Prism, 64·2 F. Mean Temp. of Air, . 62·5 Mean Barom. . . . 29·98				

* C_1 and F_1 are used here to denote very strong lines adjacent to FRAUNHOFER's lines, C and F.

† FRAUNHOFER denotes by *b* the two most refracted lines of a remarkable group, represented by three strong lines in his map of the solar spectrum. I have here denoted these lines by b, b_1, b_2, in the order of their refrangibility. On 20th May, about $7^h\ 10^m$ P.M., when the sun was rather low in the horizon, but free from clouds, I observed with a power of 21, the line b_2 to be very finely but distinctly double, so that the group consists of *four* lines.

TABLE III. *Observations of the Carbohydrogen Spectrum.*

Line of the Spectrum.	Series I.			Series II.			Difference of Means I. and II.	Collected Observations of Minimum Deviations.		
	Number of Observations.	Mean I.		Number of Observations.	Mean II.			Total Number of Observations.	Mean of all the Observations.	
α	5	48° 18′	14″	2	48° 18′	13″	0′ 1″	7	48° 18′	14″
β	3	48 32	9	6	48 32	7	0 2	9	48 32	7
β_1	...	...		9	48 35	12		9	48 35	12
β_2	...	...		9	48 37	50		9	48 37	50
β_3	...	...		9	48 40	10		9	48 40	10
γ	9	49 3	35	4	49 3	31	0 4	13	49 3	34
γ_1	...	...		9	49 6	23				
δ	2	49 41	58	6	49 41	58	0 0	8	49 41	58
δ_1	...	...		9	49 44	48		9	49 44	8
δ_2	...	...		9	49 46	37		9	49 46	37
δ_3	...	...		9	49 48	41		9	49 48	41
ϵ	...	...		4	50 27	54		4	50 27	54
ζ	3	50 35	37	6	50 35	28	0 9	9	50 35	33
	Mean Temp. of Prism, 53° F. Mean Barom., . . . 30·02			Mean Temp. of Prism. 62·6 F' Mean Temp. of Air, 60·4 Mean Barom. . . 29·56						

In Table III., the deviations of the lines α, β, γ, δ, and ζ, were alone determined by the theodolite; the other lines were then referred to α, β, &c., by micrometer observations.

TABLE IV. *Comparison of Contiguous Lines in the Solar and Carbohydrogen Spectra.*

Solar Spectrum.					Carbohydrogen Spectrum.							
Line.	Deviation (D.)			Index of Refraction (μ.)	Line.	Deviation (D.)			Index of Refraction (μ.)			
D	48° 18′	13″	1·621079		α	48° 18′	14″	1·621083	$D_\alpha - D_D$	0′ 1″	$\mu_\alpha - \mu$	0·000004
b_2	49 2	55	1·628659		γ	49 3	34	1·628769	$D_\gamma - D_{b_2}$	0 39	$\mu_\gamma - \mu_{b_2}$	0·000110
F_1	49 49	2	1·636407		δ_3	49 48	41	1·636349	$D_{F_1} - D_{\delta_3}$	0 21	$\mu_F - \mu_{\delta_3}$	0·000058
G	50 35	4	1·644068		ζ	50 35	28	1·644147	$D\zeta - D_G$	0 24	$\mu\zeta - \mu_G$	0·000079

TABLE V. *Observations on the Spectrum of Olefiant Gas.*

Minimum Deviations for Lines in Solar Spectrum, from Table II.			Intervals between Lines in the Solar and Gas Spectra, measured by the Micrometer.				Minimum Deviations.		
				No. of Observations.					
D_D	48°	18′ 13″	$D_\beta - D_D$	4	14′	10″	D_β	48°	32′ 23″
			$D_{\beta_1} - D_D$	4	17	10	D_{β_1}	48	35 23
			$D_{\beta_2} - D_D$	3	19	58	D_{β_2}	48	38 24
			$D_{\beta_3} - D_D$	3	22	37	D_{β_3}	48	40 50
D_b	49	1 45	$D_\gamma - D_b$	3	1	38	D_γ	49	3 23
			$D_{\gamma_1} - D_b$	3	4	33	D_{γ_1}	49	6 18
D_F	49	29 15	$D_\delta - D_F$	3	12	44	D_δ	49	41 59
			$D_{\delta_3} - D_F$	1	20	29	D_{δ_3}	49	49 44

XXX.—*On the Laws of Structure of the more Disturbed Zones of the Earth's Crust.*

By Professor H. D. Rogers.

(Read 21st April 1856.)

Having, several years ago, in the course of a prolonged investigation of the geological structure of the Appalachian chain of the United States, conducted partly in co-operation with my brother, Professor W. B. Rogers, as a purely scientific inquiry, partly by myself, in connection with a Government Survey of the State of Pennsylvania, discovered what we deemed important laws, applicable generally to all corrugated tracts of strata; and being prepared, by observations since made in the United States and in Europe, to extend their application, and give them a more general expression, I have thought that I could not select a more suitable subject for my first communication to the Royal Society of Edinburgh, than this portion of descriptive and dynamic geology, which has engaged much of my attention, theoretically and practically, for these many years. In presenting an outline of the views already arrived at, and published by us as a necessary part of the further generalizations since reached, I will refrain from repeating, in historical detail, what we have already written, but will give our conclusions in the form and with the brevity most compatible with clearness, referring to the printed papers and communications where the special topics included in this more general summary may be seen.

Wave-like form of all Upraised Tracts of the Crust.

The first or most general fact which I would enunciate respecting any portion of the earth's crust that has suffered elevation or depression from the position or level in which its strata were originally deposited, is, that the displaced beds present invariably the form of one or many waves, even when within limited geographical areas they may seem to retain an approximate horizontality. This comprehensive statement respecting the wave-like structure of the earth's crust, is not invalidated by the instances of disordered dip seen in certain dislocated regions, such as some of the coal-fields of Great Britain; for it will generally be found that the breaks or faults in the strata only separate disarranged portions of what were originally continuous undulations.

In all large stratified areas, where the dip is both gentle and persistent in its direction throughout considerable spaces, and where this dip is genuine, the result, that is to say, of a true displacement of the mass, and not a consequence of the original obliquity of deposition called false bedding, the crust waves will be found to be of an amplitude proportioned to their flatness; but in

those districts where the prevailing inclinations are steep, and where they are directed to opposite points, it will be found invariably that the inclined masses are but the parts of successive arches, or rather waves, the denuded or broken crests of which approach each other the closer as the dips are steeper.

Parallelism of the Crust Undulations.

It is, therefore, another general fact regarding disturbed zones of the crust, that where the displacement from horizontality has been great, the strata are arranged in longitudinal tracts, or great belts of parallel waves. These, where their symmetry of structure is not marred by dislocations of the crust, or hid by overlapping superficial deposits, exhibit a remarkable and beautiful resemblance to those great and continuous billows which are called by seamen rollers, and by mechanicians *waves of translation*. Far more continuous in their crests, more strictly parallel, and more symmetrical in form than the wind-produced waves upon the waters of the globe, such great swells or rolling billows, engendered by wholly different forces, are, I conceive, the true archetypes of the undulations visible in the more corrugated portions of the earth's crust. Perhaps in no uplifted district of the surface are these crust-waves so symmetrically developed, or so readily recognised, as in the Appalachian Mountains of the United States. It was there that William B. Rogers and myself, analyzing their forms, and tracing and connecting their axes, detected those phenomena of shape and gradation which led us to the general laws of crust flexures which we have ventured to publish.

But we believe that all mountain zones, and all corrugated districts generally, which have been elevated, like the Appalachians, at one epoch, and by crust movements observing only one prevailing direction, will be found to possess this wave-like structure, under similar conditions of gradation, and in a like conspicuous manner. It is only those tracts which have been revisited several times by the elevating and undulating forces, and especially those where the successive disturbances have not coincided in direction, but have crossed each other, causing interference and intersection of the waves, as in what is called a chopped sea,—such districts, for example, as the Swiss Alps and the mountains of Cumberland and Wales,—that we fail readily to discern the wave structure of the strata, or, perceiving it in part, are unable, without extreme toil and patience, to connect the originally related outcrops of the rocks, and reconstruct in our minds, and represent to the eye, the undulations that actually exist in a broken and disguised condition.

Wherever we have been led, either from observations in the field, or from a careful perusal of the descriptions of geologists, to a clear recognition of the dip-structure of any corrugated zone, whether mountain chain or otherwise, not confused by different systems of elevatory movements of the crust, we have become

impressed with its marked resemblance in all the essential features of the undulations, both as respects the typical forms of the individual waves, and the grouping and gradation of the several sets of waves, to the flexures characteristic of the Appalachian chain of America. I was particularly convinced of this resemblance upon examining, in the summer of 1848, the structure of the Jura chain of Switzerland;* and scarcely less struck with the agreement I noticed between the phenomena on the borders of the Alps, especially in the Bernese Oberland, and the features which distinguish the most corrugated tracts at the south-eastern base of the chain of the Appalachians.

Relations of Flexures to each other.

If we regard now the flexures which constitute any great undulated or corrugated belt of strata, we shall find that these display the following laws or general facts of relationship:—

Parallelism.

1. When seen in their simplicity, or undisguised by cross breaks and undulations, those of a particular district show a remarkable degree of mutual parallelism. Not only are they parallel to each other, but to the general trend of the portion of the mountain system to which they belong, and especially to its chief igneous axes, where it possesses such.

2. The flexures or waves, where the undulated zones are wide and complex, occur in groups or lesser belts; those constituting such subordinate series observing the law of parallelism still more strictly than group does towards group. This remarkable parallelism of the adjacent flexures in an undulated region belongs not only to those waves and groups of waves which are rectilinear in their crests, but to such as curve even very considerably in their lineation. Nowhere, perhaps, is the constancy of this law so well displayed as in the Appalachians. This great mountain zone of the United States and Canada, about 1500 miles in length, and more than 150 in its maximum breadth, consists longitudinally of eleven different sections, *six* of which are straight, *three* curvilinear, and convex towards the north-west, and *two* also curvilinear, but convex towards the south-east. Three of the straight sections have an approximately east and west trend, and the other three an approximately north and south course. Notwithstanding the great windings in the direction of the chain thus indicated, it is remarkable that each division or segment of it, whether straight or curved, is made up of crust-waves and groups of waves, which are essentially in mutual parallelism; and wherever a seeming exception to this rule presents itself, as on the Upper Juniata in Penn-

* See Abstract of Communication to American Association for Advancement of Science, Cambridge, Mass., March 1849, p. 113.

sylvania, and in Northern Vermont, it will be found to arise from the interference or interlocking of the ends of the waves of different but adjacent segments.

3. Crossing any great belt of anticlinal and synclinal flexures, such as that of the Appalachians, or that of Belgium and the Rhenish Provinces, it will be noticed, when the undulations are carefully traced and compared, that these consist of more than one class as respects dimensions; indeed they will be found to be of two or three grades, when grouped according to their length, height, and amplitude. In most parts of the Appalachian chain, there are at least two prevailing magnitudes in the waves. The chief class, or primary undulations, are of great size, their length amounting to from 50 to 120 miles, and their breadth to several miles, except where they are closely compressed. The subordinate or secondary waves are seldom more than a fourth of a mile wide, nor do they usually exceed ten miles in length, and in many groups they are much shorter. Frequently a third class is to be met with, of still smaller and less persistent flexures,—rolls of the strata, as they are called in the coal-mining districts of Pennsylvania,—which seem to be only local corrugations of the more superficial rocks, and not true undulations of the crust pervading the entire thickness of the formations. The relations of the primary to the secondary waves will be enlarged upon hereafter. It will suffice, under the present head of parallelism of flexures, to state that, for the Appalachians at least, those of the second order are not necessarily parallel to those of the first, though within a given district they observe among themselves the same mutual parallelism which the larger or primary waves exhibit.

Forms of the Waves.

Symmetrical Flexures.

The individual waves or flexures of a belt of undulated strata occur under three essential varieties of form. The first, or most simple, is that of a convex or concave wave, or in technical geological language, an anticlinal or synclinal flexure, in which the two slopes of the wave are equal in their degree of incurvation or steepness. This symmetrical form is restricted chiefly to the gentler or flatter undulations, and especially to those of considerable amplitude. We do occasionally meet with steep waves of the strata, having a nearly equal inclination on both their sides; but these are generally broken curves, exhibiting a snap or sudden angle at the anticlinal or synclinal axis, in place of the gradual arching, which is the normal form of all regular crust undulations.

Normal Flexures.

Another and more prevailing form displays a more rapid incurvation, or steepening of the flexure, on one side than on the other. Waves of this type have been called *Normal Flexures* by my brother and myself, in our descriptions

of the Appalachian chain, where they are very common. They are to be seen abundantly in the Jura, and in the exterior hills of the Alps. They abound, too, in the undulated palæozoic region of Southern Belgium, and are a marked feature in the coal-basins of that country.*

These flexures prevail wherever the forces that disturbed the crust were neither excessively intense nor very feeble. They usually hold an intermediate position geographically, answering to the middle place they occupy as respects energy of undulation between the groups of flat symmetrical waves, and those which are closely folded, to the description of which I next proceed. Almost invariably, those of a simply undulated tract, exhibit their steeper slopes directed all to one quarter.

Folded Flexures.

This *third* and remaining class consists of flexures in which there is an inversion or doubling under of the steeper side of each convex curve or wave. When this structure is at a maximum, the folding back, downwards, of each convex or anticlinal arch amounts almost to a parallelism of the two branches or sides of these curves; and where there are several such foldings, alternately convex and concave, the strata may be said to be crimped or plicated into one dip, though the entire change of inclination through which the inverted portions have been bent, amounts to the supplement of the angle of the dip or the difference between the apparent dip and 180°. It is a necessary feature of all such folded flexures, that the approximately parallel sides of the folds dip obliquely and not perpendicularly to the horizon. They are, therefore, but exaggerated instances of the class of normal flexures, or those where one branch of the curve is steeper than the opposite. As in the case of the normal flexures, the more incurved sides of these folded waves all look the same way.

Axis Planes.

It is convenient, for the purpose of expressing the kind of flexure, its degree, and its direction, to make reference to the geometric planes which bisect or equally divide the anticlinal and synclinal bends. These imaginary planes we have called the axis planes of the undulations, being those which include all the horizontal lines or axes round which the individual concentric strata have bent in the act of undulating or folding. In the first-described class of flexures, or those of symmetrical curvature, each anticlinal and synclinal axis plane is necessarily perpendicular to the horizon. In the second class, or the normal flexures, these axis planes are necessarily not perpendicular, but steeply inclined to the horizon, and their deviation from the perpendicular is in proportion to the difference of inclination,

* See DUMONT's Memoir sur les Terrains Ardennais et Rhenan, &c.

or of incurvation of the two slopes of the wave, modified, according to a certain law of variation, with the dip. In the *third* class, or that of flexures with *inversion*, the axis planes are likewise not perpendicular; and it will be found that, in the great majority of instances, they dip with a less degree of steepness than the planes bisecting waves of the normal or other unsymmetrical type. Indeed, it may be stated generally, that, just in proportion as the flexure departs from the symmetrical wave form, through greater and greater inequality of dip, up to parallelism of the inverted with the uninverted branch of the curve, the axis plane departs from the perpendicular direction, to assume a less and less inclination to the horizon. In many districts of extreme plication of the strata, for instance in the Atlantic slope of the middle and southern States of North America, also in the Bernese Oberland, in the Ardennes, and in North Wales, the axis planes dip at an extremely low angle, consequent on the excessive amount of horizontal movement which the strata have undergone in the act of folding.

So nearly parallel are the inverted to the uninverted sides of the folds,—the axis planes all, of course, dipping one way,—in many districts of close plication, that the detection of the anticlinal and synclinal bends is not a little difficult, especially where the sections, natural or artificial, are not perfectly clear of superficial debris. In such cases the whole plicated mass looks as if it contained but one dip, or consisted of only one thick sequence of deposits, instead of am uch thinner formation many times reduplicated. To add to the liability of error, such bodies of folded strata are especially subject to that condition of jointage which is called *slaty cleavage*. In this structure, as I shall presently show, the divisional planes not only tend to obscure the original planes of sedimentation by their greater conspicuousness, but they often, by observing a very prevailing parallelism to the general dip of the folded beds, or, more strictly, to their axis planes, effectually disguise the anticlinal and synclinal curves. It is from these circumstances, and not from any erroneously supposed effect of truncation or denudation, actually to remove the anticlinal bends of the strata, that it is frequently so difficult to detect the true order of original superposition and the real thickness of closely plicated formations. Of course, no erosion upon an anticlinal axis, however closely folded it may be, can obliterate the bends in those beds which have their curves below the level reached by the denuding agent.

Crust Waves Straight and Curvilinear.

Regarding the great flexures of the crust as individual waves, which in truth they seem to be, we find them exhibiting, not only the above differences in the sloping of their two sides, but marked differences of form when viewed longitudinally. Thus, many are of extraordinary straightness; some of the larger simple anticlinals of the Appalachians being more than 100 miles in length, without any material or even perceptible horizontal crooking or deviation in their crest lines.

Others again are curved, some of these sweeping convexly towards the quarter of chief crust dislocation and metamorphism, others curving convexly from it, but we never find these two classes associated in the same group, and in the Appalachians, never even in the same segment of the undulated zone. In some districts of this and other chains, some of the principal curvilinear anticlinals and troughs are quite as extended in length as the great axes which are straight. They appear to be independent waves generated from curvilinear fractures of the crust, and not to be merely the bending terminations of adjacent rectilinear flexures. One of the most interesting features belonging to some of them is, their extent of curvature, and the graceful continuous smooth sweep which their curving axes present, often without jog or hitch, from one extremity to the other. This crescent-like form is developed in a high degree in those curving sections of the Appalachian chain, where the waves are convex north-westward, or from the quarter of maximum dislocation—the Atlantic slope. In the Juniata division of the chain in Pennsylvania, some of the curving anticlinals, 80 and 100 miles in length, change their trend between their two extremities as much as 40°; and in the Delaware division of the same chain, which also bends with a concave sweep to the north-west, the deflection in more than one great synclinal trough, and anticlinal axis, is not less than 60°. This fact of the curvilinear form of anticlinals and synclinals of great length in this was long ago offered by us, as a phenomenon incompatible with the generalization of the eminent French geologist M. Elie de Beaumont, who conceives that the lines of elevation of the crust have been great circles of the sphere, and that those of a given geological epoch have invariably observed one constant direction. The whole of the Appalachian chain having been demonstrably corrugated into its present undulations at one epoch, that of the end of the coal period, the simple fact that its different groups of waves deviate as widely in direction from each other as 60°, while those of each group are reciprocally parallel,—the whole chain indeed, if subdivided on this principle of direction, including not less than eleven conspicuous segments,—is itself enough to show that no particular constancy of relation can be established between the dates of elevations, and the mere directions of the lines or axes of the strata. But this other fact of so marked ach ange of direction in one and the same axis, as displayed by these crescent-shaped waves, is, if possible, in still more striking contradiction with that hypothesis. Another fact connected with the groups of curving waves in the Appalachian chain is particularly deserving of mention in this place, from the bearing it appears to have upon the question of the direction of the pulsatory or wave-like motion of the crust, at the time of the permanent production of the flexures. It is this—the individual waves in all the segments of the chain which are convex north-westward exhibit, as already said, a continuous symmetrical crescent-like curvature; those, on the contrary, which are included in the other curvilinear districts, convex to the south-

east, or towards the region of dislocation and metamorphism, present a much less regular incurvation along their anticlinals and synclinals, and a far greater amount of interference and of dislocation. These appear indeed to be the sections of the chain, where the greatest amount of tangential wrenching, rupturing, and warping of the crust has taken place, and where the greatest amount of transverse hitching and fracturing has happened to all the strata. The causes of this difference will, I think, be seen, when I shall have developed our theory of the mechanical forces which undulated the Appalachian strata, and set in motion the stupendous billows of the crust, which resulted in the elevation of these mountains. An inspection of the best maps and sections of the more disturbed European zones, leads me to believe, that a similar contrast prevails between the curvilinear waves which are convex to the districts of disruption, whence I suppose them to have proceeded, and those which are concave to the same quarters; but before this law in all its generality can be established, geologists must institute far more critical researches into the physical structure of those undulated and plicated districts than they have hitherto conducted.

Gradations in Flexures.

Succession from the Folded to the Symmetrical Waves.

Several phenomena of gradation will be found to display themselves when we cross any broad belt of plicated and undulated strata. Starting from the side of maximum disturbance and contortion, invariably the quarter of maximum igneous action,—displayed either in Plutonic eruptions through the crust, in crust dislocations, or in metamorphism of the sedimentary rocks,—the flexures first met with are of the obliquely plicated form. Advancing towards the middle of the zone, the folds become obviously less close, and proceeding still farther, they gradually open out, displaying more conspicuously their anticlinal and synclinal curves, until the inverted side of each wave becomes only perpendicular. This perpendicular altitude of the steep side soon becomes a dip towards the opposite quarter from that previously observed by both sides, and as we proceed, the steepness of the slope of the wave now rectified in position, grows progressively less and less, until on the far side of the zone, both slopes approximate to equality.

Expansion of the Waves as they pass from the Folded to the Symmetrical Form.

Concurrently with this gradation, there is a progressive opening out of the spaces between the crests of the successive waves, such indeed as to amount in the Appalachians, and sundry other broad regions of crust-undulation, to an enlargement by many times of the amplitude of the more compressed class of flexures.

Progressive Flattening down of the Flexures.

A third feature of gradation shows itself in the progressive sinking or flattening down of the successive individual flexures, until these finally pass into horizontality. These three types of form in the waves, as respects their expansion, their increase of relative distance or amplitude, and their declining height, are conspicuously discernible, wherever we cross the great Appalachian chain of the United States, by any section, in a direction from south-east to north-west. An inspection of the engraved sections illustrating our paper on the physical structure of the Appalachians,* or an examination of the more numerous similar diagrams explanatory of the geological surveys of New Jersey, Pennsylvania, and Virginia, will amply avouch for the correctness of this generalization. It is further borne out in the published reports of the Government Survey of Canada, where the plicated structure of the green mountain range of Lower Canada, along all its south-eastern border, and the universality of the south-eastward dip of the folded strata, —in other words, of the dip of the axis planes,—is very distinctly set forth by Sir WILLIAM LOGAN.

Not only does the entire chain in its breadth exhibit a general gradation in the several features here described, but each of its great component groups of flexures, presents the same progressive opening, recession, and flattening down of its waves in the same uniform north-west direction.

Similar phenomena of gradation will, we feel assured, disclose themselves in any section made from the Taurus range, north-westward through the Rhenish Provinces and Belgium, where, on the one side, the more ancient and much metamorphosed strata at the base of the Palæozoic system, according to the observations of MURCHISON and SEDGWICK, present much reversal of the dip, and where one and the same dip, namely, to the south-south-east, is continued with very few exceptions across a belt of 50 miles; whereas, on the opposite or northern side of the zone, as is well shown in the beautiful sections of M. DUMONT, the flexures of the Belgian coal-fields are of the normal type, and much more open and dilated.

Nowhere perhaps in Europe are these gradations in the undulations of strata more beautifully exposed than on the flanks of the Alps. Deep in, towards its higher central igneous chains, the plication of its stratified rocks is excessive, and the inclination of the axis planes remarkably low; but advancing outwards, the waves gradually lift their crests, throw forward their inverted sides, and assume that type of flexure which we have called the normal one; while, at the outward base of the mountains just before these undulations are concealed by the overlapping tertiaries of the plains of Switzerland, and of Northern Italy respectively,

* Transactions of the Association of American Geologists.

the curves become, in many instances, broad, depressed, and almost symmetrical in form.

From the descriptions here given of the structure of the Appalachian chain and other disturbed districts, it is obviously a general law, that the axis planes of the flexures are not only inclined all in one prevailing direction, though at different angles, but that they dip invariably towards the quarter or zone of maximum disturbance and rupture of the crust.

Fractures or Faults in Tracts of Undulated Strata.

Two classes of dislocations abound in all zones of plicated and undulated strata, where the crust waves exhibit much steepness, and especially where they have the inverted or folded form. By far the most numerous, though the shortest and least conspicuous class, are the breaks or faults which run approximately transverse to the strike of the anticlinal and synclinal axes. These may be extensively recognized in the Appalachians, where they are a primary cause of the deep ravines, or breaches through the ridges, which furnish passage to nearly all the rivers, and even lesser streams which drain this chain. Such ravines are especially frequent near the extremities of the large anticlinal waves, particularly where they have been cut through along their crests by denuding waters, and have given rise to valleys of elevation and erosion, inclosed by monoclinal, outward-dipping, sandstone ridges. It would seem as if the elliptical folding round of the strata towards the ends of the great denuded waves had caused the horizontal wrenching which resulted in these fractures. Mr William Hopkins, of Cambridge, has, in an able paper on the subject of dislocations affecting dome-shaped elevations of the earth's crust, indicated the true source, I conceive, of the double system of fractures to be met with in all elliptical anticlinal belts. An elongated anticlinal wave is, in truth, only a greatly lengthened elliptical dome, in which the radial cracks caused by a maximum tension in the strata transmitted from the more central portion of the crust-wave, are distributed, some of them longitudinally, others transversely, as respects the anticlinal axis, the transverse ones multiplying themselves where the elliptical strain has been greatest, towards the two extremities of the waves.

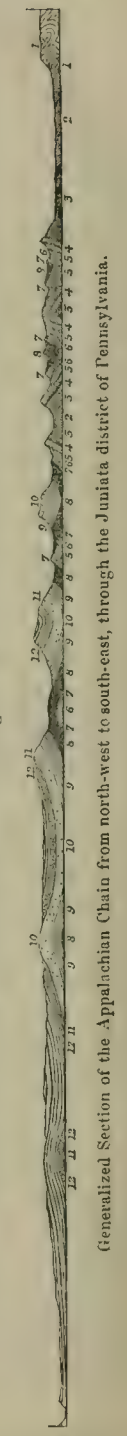


Fig. 1. Generalized Section of the Appalachian Chain from north-west to south-east, through the Juniata district of Pennsylvania.

The other far more conspicuous class of dislocations connected with these crust undulations, are the great longitudinal ones. These are of frequent occurrence in the more contorted portions of the Appala-

chian zone, especially in those where the chain is convex to the south-east, and in the straight sections of South-western Virginia and Eastern Tennessee. But I am persuaded, from the descriptions of geologists, and from my own observations that the fractures of this class are equally numerous in the Jura Mountains, in the Alps, in the district of the Ardennes, in Belgium, and in the mountain chains of Scotland. A leading feature of these great fractures is their parallelism to the main anticlinal axes, or lines of folding of the chains to which they belong. They are, in fact, only flexures of the more compressed type, which have snapped and given way in the act of curving, or during the pulsation of the crust. They coincide, in the great majority of instances, neither with the anticlinal nor the synclinal axis planes of the waves or folds, but with the steep or inverted sides of the flexures, and almost never occur on their gentler slopes. This curious and instructive fact may be well seen in the Appalachians of Pennsylvania and Virginia, by tracing longitudinally any one of their great faults from its origin on the steep flank of an anticlinal wave along the base of its broken crest to where the anticlinal form is resumed again. The following brief description, from our memoir on the Physical Structure of the Appalachians, taken from the Transactions of the American Association, will show the general phases through which these fractures pass:—

"From a rapidly steepening north-west dip, the north-western branch of the arch (or flank of the wave) passes through the vertical position to an inverted or south-eastern dip, and at this stage of the folding the fault generally commences.

"It begins with the disappearance of one of the groups of softer strata lying immediately to the north-west of the more massive beds, which form the irregular summit of the anticlinal belt or ridge. The dislocation increases as we follow it longitudinally, group after group of these overlying rocks disappearing from the surface, until, in many of the more prolonged faults, the lower limestone formation (Cambrian or Lower Silurian) is brought for a great distance, with a moderate south-easterly dip, directly upon the Carboniferous formations. In these stupendous fractures, of which several instances occur in South-western Virginia, the thickness of the strata ingulfed cannot be less, in some cases, than 7000 or 8000 feet."

One of these enormous faults in South-western Virginia has a length of more than 80 miles, and is almost perfectly straight. It follows the south-eastern slope of Brushy Mountain, from the head of Catawba Creek to the vicinity of the court-house of Smyth county, engulfing all the strata of the south-eastern half of a synclinal basin, of which the Brushy Mountain remains as the other half. Where the dislocation attains its maximum intensity, or shows the greatest displacement of the strata, the lower formation,—the Auroral Appalachian limestone, the equivalent of the Festiniog group of England,—of one side of the fissure, rests in an inverted attitude, with a gentle south-east dip, directly on the south-east

dipping Vespertine grits and shales—represented in Great Britain by the lowest Carboniferous strata,—forming the other wall of the fault.

General Parallelism of the Faults to the Axis Planes.

It is a very general feature of the great longitudinal faults, whether these coincide with the anticlinal and synclinal axis planes, or occur, as they more frequently do, on the steep sides of the flexures, to dip in the same direction with the axis planes. In the Appalachian chain their inclination, therefore, is almost invariably towards the south-east. A consideration of the nature of the forces which have folded and ruptured the strata, shows that such a direction of their dip is an almost inevitable consequence of the undulatory movement. It is only in districts of low symmetrical crust undulations, or those where the strata are absolutely flat, that the great fractures descend perpendicularly.

In corrugated zones, like those of the Appalachians, the Alps, and the Ardennes, the magnitude of these main longitudinal fractures, both as respects length and vertical displacement of the dislocated strata, is in proportion to the sharp bending or close folding of the waves to which they belong. Thus they invariably possess their grandest dimensions, in the south-eastern or most plicated belt of the Appalachians, or on that side of the zone where the crust movements have been most energetic.

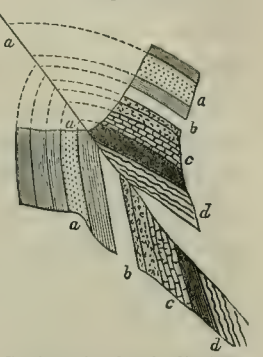
Uninverted side of Wave usually shoved over the Inverted.

This obliquity or dip towards the quarter whence the movement has proceeded, is evidently the cause of that overlapping of the newer, less-lifted side of the wave in which the fault lies, upon the steeper, more perpendicular, or inverted flank; for the forward or horizontal thrust which accompanied the propagated wave-movement resulting in the fracture, has, when this once occurred, found an inclined plane, up which the uninverted slope of the wave slid over the edges of the strata composing the inverted side. In many instances, as the Appalachian sections will prove, one flank of the wave has been shoved forward and upward, unconformably, upon the crushed and buried flank to an enormous distance. Subsequent erosion having cut down the higher strata of the updriven, gently-sloping side of the wave, its lower beds are exposed to view, in immediate contact with the unremoved upper strata of the other side. Where the lower formations, cut into by the water on both sides of the fault, have been equally easy of excavation, especially when they are all of identical composition, as in the case of the great lower Appalachian limestones, the denuding waters have so effectually planed down the great inequalities of surface at first caused by the dislocation, as sometimes to have left in the landscape almost no external traces of the gigantic rupture which lies beneath the soil. It is, then, only by a recognition of the ages of the respective strata, thus abruptly placed in contact, and

usually, though not always, by some sudden difference of dip, that we are enabled to detect the presence and the magnitude of the dislocation.

It seems also necessary, on this occasion, to explain the effects of those great longitudinal obliquely-dipping faults, when they occur directly in the anticlinal and synclinal axis planes, which are their occasional positions. The same forward upward-sliding, just described as having occurred where the fracture is between the anticlinal and synclinal curves, must have taken place where it has coincided with these, and as the movement must necessarily have been in the same direction, lifting, that is to say, the lower strata cut by the fault, upon the edges of higher and higher beds, in the forward propulsion of the flat side of the broken wave, we have no difficulty in understanding how fractures in these positions, as well as in the other already spoken of, must have given rise to that very common phenomenon of the dipping of newer formations under older ones in plicated and dislocated countries, like the Alps and Appalachians. This puzzling feature of stratification, long an enigma to geologists, can, I conceive, be explained upon no other analysis than that which is here given, namely, the oblique folding of undulated strata,—the obliquity of the planes of the faults, either coincident with, or parallel to, obliquely dipping axis planes,—and the forward upward thrust of the uninverted upon the inverted broken strata, through a tremendous tangential force incident to a wave motion.

Fig. 2.



Section of a longitudinal dissection showing the plane of fracture and the uninverted strata driven forward upon the inverted.

Exemplification of the Laws of Flexure, by the Phenomena of some of the Undulated Zones of Europe.

Belgium and the Rhenish Provinces.—Embracing in one view the undulated districts of Southern Belgium, the Rhenish Provinces, the Westphalian coal-field, the Ardennes, the Hundsruck, Taurus, and Hartz ranges, as described and mapped by M. Dumont and other geologists, we can discern most distinctly all the phenomena of flexure and of dislocation of the strata, here indicated as characteristic of the structure of the Appalachians. We there perceive a wide zone of crust undulations having its strata most invaded by igneous rocks, and most ruptured and metamorphosed, along its south-eastern side, and displaying its most ancient sedimentary formations in a state of close plication, with innumerable inversions of the dip, imparting to wide tracts one uniform parallel inclination towards the south-east. Crossing the zone north-westward, we enter newer and newer strata, until we come to the undulated coal-field of Westphalia or Belgium, our traverse taking us from the non-fossiliferous formations, at the very base of the Palæozoic system. In whatever meridian we make our section,

we find the north-west sides of the waves, with few exceptions, steeper than the south-east ones, not only where they are inverted, but where they have a normal dip. We find, moreover, as we advance, that the waves grow more and more open, and that the distances between them increase, that they subside in height, and that the two slopes approximate nearer to equality. These gradations are admirably disclosed in any traverse across the strike north-westward, from the water-shed of the Ardennes to the Belgian coal-fields of the Meuse. I can detect, in the features of DUMONT's exquisite map of Belgium and the neighbouring countries, the very same relations of the longitudinal faults to the flexures which have engendered them, as those above described in the fractures of the Appalachians. They evidently occur, for the most part, on the north-west sides of the anticlinal axes, and cause older strata to ride upon newer ones plunging under them, with approximately parallel dip. Even the phenomena of cleavage, presently to be described, will be seen to exhibit the very same laws in the more metamorphic southern half of this wide zone of plication, which they present along the south-eastern side of the Appalachian chain, and the Atlantic slope bordering it. This region of the Rhenish Provinces and Belgium further agrees with the Appalachians, in being a zone of undulations and plications, where the folding movement has been all in one direction.

The Jura Chain of Switzerland.—The Jura chain of Switzerland, as I pointed out in 1848, in communications to the Geological Society of London, and in 1849 to the American Scientific Association, is another very interesting belt of crust waves, displaying, in its structure, a close resemblance to the Appalachians.

It embraces, like the American mountains, many groups of waves differing in the directions of their axes in different districts of the chain, but the individual groups composed of waves which are remarkably parallel. Few of these undulations exhibit actual inversion of their steeper sides, the dip only in some instances passing the perpendicular, and generally not exceeding on an average 70°, the gentler or opposite slopes having a mean slant of about 40°. In four traverses which I made across this chain, I observed one almost invariable law as to the direction of the steep and gentle sides of the undulations, or, in other words, of the axis planes. Contrary to first anticipation, and to the belief of many Swiss geologists, I found the steeper curvature of the waves directed toward the Alps, and not from them, implying that the crust movement which lifted these grand and picturesque arches proceeded from the north-west, and not from the chain of the Alps. This also is a belt whose undulations are chiefly in one direction.

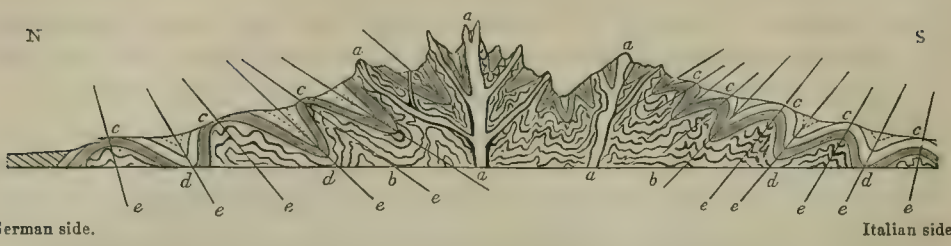
The Alps.—The great chain of the Alps is much more complex in its structure than either of the undulated zones yet described. It contains but few waves of the open or normal type, but innumerable close foldings or plications. Through-

out a great portion of its length, this lofty and rugged zone of mountains consists of two approximately parallel chief crests. The great feature in the geological structure of the whole zone is the presence of belts of closely plicated Mesozoic and Tertiary strata on both flanks of each of these great constituent ranges. But the most striking, and, at first view, perfectly enigmatical feature, is the inward plunge of the newer strata beneath the older, in the sides and at the base of both chains. When, however, the plicated strata are structurally arranged and traced, we find that this phenomenon assumes the character of a symmetrical folding of the rocks in two opposite directions from each high central axis. The individual foldings, with scarce an exception, lean outwards from the central tracts of the mountains, or from the quarters of igneous disturbance, rupture, and maximum metamorphism of the crust. In other words, the axis planes of the plicated strata of the flanks of the Alps dip inwards towards the centre of the chain; those nearest to it at a low angle, and those more remote at angles steeper and steeper as the waves recede, expanding to the outer base of the range. High on the flanks of the Alps, or, what is the same thing, deep in towards the roots of the mountain, where only the synclinal bends, of the flexed strata, have been protected from denudation by inward folding, these closely compressed troughs lie pinched in between the older strata in oblique inward inclination. The transverse sections expose these bendings, which are called Vs by some of the Swiss geologists. Here then we behold an exact counterpart in the stratification or structure of a single flank of the Alps, of that folding with inversion which characterizes the Appalachian chain, or that of the Ardennes, a single side of the Alps being the equivalent of the whole of either of those zones; it consists, that is to say, of a belt *undulated in one direction.* Crossing the Alps, or rather one of its component great chains, we find another *similar* belt of the same strata, plicated in the same way, with their axis planes dipping also under the crest or orographic axis of the mountain, but of course, to the opposite quarter of the compass as compared with the plicated zone of the other flank. This is, I conceive, a true picture of that feature which, hitherto imperfectly analysed, has been called by some of the geologists of Switzerland, expressively enough,

The Fan-like Structure of the Alps.

Viewed as a single chain, this mountain system consists, then, of two belts undulating in opposite directions; but, as already stated, it is for the most part of its length a double chain; and I think each range, especially, where these are widest apart has a plicated belt of strata upon each of its slopes, so that, for some districts at least, the fan-like structure is twice repeated; in other words, there are four belts of closely folded waves, each having its axis planes dipping towards the base of its own high mountain system.

Fig. 3.



Generalized Section of the Alps, displaying the dipping of the folds of the strata on both sides in towards the igneous axis.

a, Igneous rocks in central axis of the chain. *b*, Gneissic and other older strata.
c, Anticlinal flexures. *d*, Synclinal flexures, or Vs. *e*, Anticlinal and synclinal axis planes.

A conspicuous and pervading cleavage structure coincident in the direction of its dip, as I shall presently show, with the oblique axis planes of the folded rocks, contributes greatly, I conceive, to the illusive phenomenon of an inward dip of all the strata, or to that general feature which has been called fan-shaped.*

This inward dip is rendered still more obvious by the circumstance, that the foliation or crystalline lamination of the more altered strata, itself obeys very generally a similar law of parallelism to the axis planes of the flexures. Where this crystalline grain of the rocks does not coincide with the stratification, it exhibits a great tendency to a coincidence of dip with any system of cleavage planes belonging to the same or other parts of the mass. In either case, it will dip inwards towards the igneous axis of the chain, if the strata possessing it are themselves closely folded in conformity with the prevailing law. But the phenomena of cleavage and foliation will be noticed afterwards. We now proceed to discuss

General Phenomena of Slaty Cleavage in the Appalachians and other Zones of Plicated Strata.

Cleavage parallel with, but independent of the Dip of the Strata.

It is now a good many years since Professor Sedgwick and other geologists announced the important general fact, that the structure called *cleavage* pervades the altered strata affected by it, in directions independent of their bedding or laminæ of deposition. That eminent geologist further announced that these planes are approximately parallel to each other over large spaces of country, however contorted the dip of the rocks. He likewise enunciated a second general law of much importance, "That when the cleavage is well developed in a thick mass of slate rock, the strike of the cleavage is nearly coincident with the strike of the beds." Subsequently Professor Philips gave to this rule of the cleavage a still more

* From the analysis above given of the structure of the sides of the Alps, it will be seen, that I entirely concur with Professor James Forbes, and with all the more eminent of the Swiss geologists, in recognizing the fan-like dip of the newer strata, Tertiary and Mesozoic, conformably in appearance at least under the older strata, metamorphic and gneissic, of the higher more central tracts, and that I dissent entirely from the theoretical section offered by Mr Daniel Sharpe.

comprehensive and exact expression, when he stated to the British Association in 1843, that the cleavage planes of the slate rocks of North Wales were always parallel to the main direction of the great anticlinal axes. Other geologists have abundantly confirmed these generalizations. Since 1837, these phenomena of the close parallelism of the cleavage planes of a given district with each other, and with the main axis of elevation of the district, have been constantly observed and recorded by my brother Professor W. B. Rogers and myself, in our Geological Surveys of Virginia, Pennsylvania, and New Jersey.*

In 1849, I submitted to the American Association for the Advancement of Science, at the annual meeting held at Cambridge, Massachusetts, in a communication on the analogy of the ribbon structure of glaciers to the slaty cleavage of rocks, a statement of what I had for some years past regarded as the true law of the direction and position of the cleavage planes of a district of undulated and plicated strata.

In its simplest expression the rule is, that *the cleavage dip is parallel to the average dip of the anticlinal and synclinal axis planes*, or those bisecting the flexures. The generality of this rule was shown on the occasion mentioned, by sections exhibiting the flexures and cleavage in the Appalachians, in the Alps, and in the Rhenish Provinces; and I have since become convinced of its universality from the inspection of the phenomena of other districts, and from a study of the descriptions and sections of geologists. Want of time at present prohibits me from citing the abundant evidence for this law to be found in the best recently printed memoirs upon slaty cleavage; but I hope to be able ere long to give my own observations in support of the highest British geological authorities, who, unaware of the relationship itself, have furnished the most satisfactory data for the recognition of it. I cannot, however, refrain, in this place, from sustaining the generalization I am here venturing to put forth, by instancing the support it receives from the excellent descriptions recently given by Professors Harkness and Blyth of the Cleavage of the Devonians of the South-west of Ireland. In their paper in the Edinburgh New Philosophical Journal for October 1855, they not only establish an agreement between the strike of the cleavage planes with that of the several rolls (or anticlinals) which affect the island of Valentia, but they show that while the cleavage dip is southerly, the anticlinal "curves have been pushed over in a more or less northerly direction," inverting the carboniferous limestones and coal measures. Their general statement is, that the cleavage structure of rocks does not result from the simple rolling of the strata, but from this cause combined with a considerable amount of pressure; and this latter force acting from the south, has pressed over the strata in a series of oblique curves to the north, and given to the inclined cleavage its more or less of a southern dip. They support the doctrine of Mr Sharpe respecting the cleavage of

* See Ann. Reports on those Surveys, 1837–40, and other Essays.

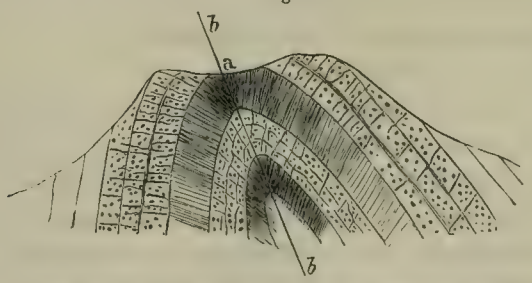
rocks,—"That there has been a compression in the mass in a direction everywhere perpendicular to the planes of cleavage, and an expansion of this mass along the planes of cleavage in the direction of a line at right angles to the line of incidence of the planes of bedding and cleavage," or, in other words, to the direction of the dip of the cleavage. From this view of the mechanical nature and direction of the force engendering cleavage, I beg leave respectfully but explicitly to dissent.

Fan-like Arrangement of the Cleavage at the Anticlinal and Synclinal Axis Planes.

A second general fact or law of direction of the cleavage planes in folded strata must be here enunciated. At first view it is in seeming contradiction with the universality of the primary rule above stated, of the invariable approximate parallelism of the cleavage planes to the axis planes of the flexures; but closely examined, it will be seen, I think, to be in beautiful accordance with that law and with my hypothesis of the origin of the cleavage structure. The rule is this, that where the cleavage is fully developed, and the anticlinal and synclinal flexures are also conspicuous and very sharp, the cleavage planes immediately adjoining those bendings are not parallel to the axis planes, but partially *radiate from them in a fan-like arrangement* upward in the anticlinals, and downward in the synclinals.

This aberration from the normal direction is furthermore different in degree upon the two sides of the geometric axis plane, being usually greatest upon the inverted or steep side of the wave.

Fig. 4.



Fan-like Arrangement of Cleavage at an Anticlinal Axis.
a, Cleavage in the Shale. *b*, *b*, Axis Plane.

Another aberration of the cleavage planes from their normal direction of parallelism to the axis planes, is their tendency to conform partially to the dip of the strata, when the two are nearly coincident. This operates to flatten the inclination of the cleavage upon the gentler slope of each wave, and steepen it upon the more inclined one; and as in every belt of uniform flexures closely plicated with inversions, the uninverted or normal dips greatly exceed the inverted ones, it produces in such cases a prevailingly lower inclination in the planes of cleavage than in the planes bisecting the flexures.

Relation of Cleavage to the Mechanical Constitution of the Strata.

There is yet another law respecting cleavage; it is the dependence of this structure upon the mechanical texture, and possibly upon the chemical composition, of the fissured rocks.

Geologists have for several years recognized the fact, that in formations composed of alternations of the coarser mechanical rocks, such as silicious grits and conglomerates, with fine-grained argillaceous beds, as slates, shales, or marls, the coarse beds are unaffected by cleavage, while the fine-grained ones are often pervaded by it. Indeed, one may observe in a given locality almost a strict proportion between the degree of intimate fissuring of the rocks by cleavage planes, and the degree of comminution of their particles.

Connected probably with this interruption in the distribution of the cleavage-condition through such heterogeneous groups of strata, I have observed another general fact of modification of the cleavage planes, which should not be passed unnoticed here. They tend in the fine grained argillaceous beds, to curve a little from the normal direction into an approach to parallelism with the surfaces of bedding of the adjoining coarser mechanical deposits, presenting, in a transverse section, a kind of gentle sigmoid or double flexure. This is well shown in the cleavage-traversed rocks at the base of the anthracite coal formation of Pennsylvania, especially in the transition or passage beds which connect the Umbral red shales of that region, with the base of the coal-sustaining conglomerate, and also where these shales alternate with the upper coarser members of the Vespertine sandstone. The small section here appended, showing the cleavage in one of these groups of alternation of red shale and sandstone, from a railway cut near Ashland, in the middle anthracite coal-field, exemplifies well the phenomenon referred to.

Fig. 5.

Beds of Red Shale with Cleavage alternating with beds of Sandstone without Cleavage; Cleavage curving towards parallelism with the bedding at its boundaries. Section near Ashland, Pennsylvania.

The tendency, here shown, in the cleavage planes to conform to the planes of bedding, where abrupt changes of composition interrupt the continuity of the fissures, is but another variety of the phenomenon already adverted to, of a de-

flexion of the cleavage in bands of plicated strata towards a parallelism with the gently dipping slopes of the anticlinal waves. This remarkable fact of an intimate dependence of the cleavage upon the composition and mechanical texture of the structure is, I conceive, of itself sufficient to refute the hypothesis somewhat in favour at present, of the purely mechanical origin and nature of the cleavage-producing force; for we cannot conceive how a mechanical force, either of compression or of tension, transmitted, as necessarily it must be, very equally through parallel layers of coarse and fine materials, should have exerted no fissuring action the moment it reached the surfaces of the coarser beds, and yet have been able to cleave into thin parallel slaty laminæ the whole body of the fine-grained argillaceous strata. One would more naturally suppose that the less firmly aggregated softer mud rocks or shales would have been even less easily fissured by sharp cleavage joints, than the more massive and better cemented grits. It is of importance to notice here, that subsequent disruption of the strata may change the normal position or dip of the cleavage, after its formation, and give rise to some of the apparent deviations from the general law of direction above enunciated.

The Cleavage Susceptibility alternately greater and less in Parallel Planes.

Cleavage is a susceptibility in rocks of a certain composition, and in a particular stage of metamorphism, to split in definite straight parallel planes. The cohesive force is obviously at a minimum of intensity in the direction perpendicular to these planes. In the other two rectilinear axes of the cube, one side of which is coincident with the cleavage plane, the force of cohesion next in degree of intensity is the horizontal one, or that in the direction of the strike of the cleavage, while the most intense cohesion of all is that in the direction of the cleavage dip. It is in this latter direction that the molecular forces of attraction engendering incipient crystallization seem to have been most powerfully awakened while the polarities have been feeblest in the lines perpendicular to the cleavage planes; but apart from these three directions and grades of corpuscular force, we have indications, in any homogeneous mass of cleavage-traversed slate, or other rock, of the presence of two grades of the minimum cohesion, constituting the cleavability, disposed side by side in alternate parallel order; in other words, where the cleavage is fully developed, the rock will be found to contain certain nearly equidistant closely contiguous planes of maximum cleavability, or, what is the same thing, of minimum lateral cohesion—the material of each thin plate of the slate cohering more strongly together than these adjacent plates cohere to each other. The existence of such planes is indicated by the manner in which any mass of very cleavable slate, long exposed to atmospheric agencies, invariably breaks up; as we may see in any naked outcrop. If the cohesion of the mass in a direction perpendicular to the cleavage planes were equally strong in all parallel planes that we can imagine pervading it, it is impossible to understand how any

uniformly acting disintegrating forces,—either expansion and contraction by heat, soakage and drying, or freezing and thawing,—could subdivide it by planes or fissures, so regularly distributed as we find them. These could only have arisen, I conceive, from the presence of parallel planes of weaker and stronger cohesion. In this interesting structure, we discern a striking analogy to that alternation of thin plates of solid blue crystal ice, and white porous ice of less cohesion, which is so distinct a feature in the fully developed ice of glaciers, and which has been expressively named by Professor JAMES D. FORBES, the ribbon structure.*

FOLIATION.

The relations of the foliation or crystalline lamination of metamorphic strata to the cleavage planes, and the planes of stratification, come next to be considered. Two facts may be stated of foliation, which possess, perhaps, the constancy of general laws. One of them is, that this structure, as it is seen in gneiss and mica schist, observes, when the strata are not traversed by cleavage, an approximate parallelism to the original bedding. Apparent exceptions to this rule occur in several localities near Philadelphia, and elsewhere in the United States, and have often been noticed in Europe, by Mr D. SHARPE, and other good observers; but all of them can be reconciled to the general fact, and reduced, it is conceived, to one comprehensive law, namely, that the planes of foliation, or the laminæ, formed by the crystalline constituents of the foliated rocks, are *parallel to the planes or waves of heat* which have been transmitted through the strata. Wherever large tracts of the gneissic rocks retain a nearly horizontal undisturbed position, the foliation is almost invariably coincident with the stratification, and in this case, the wave of heat producing the crystalline structure can only have flowed upwards through the crust, invading stratum after stratum, in parallel horizontal planes. Again, when injections of granite occur in uplifted gneissic strata, the crystalline lamination is generally seen to be parallel to the plane of outflowing temperature.

* In a communication submitted to the American Association for the Advancement of Science in 1849, I attempted to show this analogy of the ribbon structure of glaciers to the slaty cleavage of rocks, in the following remarks:—"The ice of glaciers consists of thin alternate parallel bands or plates of blue crystal ice, and white porous ice, each not more than one-third or one-half of an inch in thickness. These pervade the whole mass of every glacier, and are clearly exposed on the sides of the transverse fissures. Near the sides of the glacier, they are almost absolutely parallel with its mountain walls, but they sweep away towards its medial line, and form, like all the other planes which divide the glacier, a series of innumerable loop-like curves. This looped or festooned form is obviously caused in part by the downward tendency of the movement or flow of the semiplastic ice, and in part by the influence of the terminal moraine to induce that parallelism to itself, which the rocky sides of the glacier produce in the ice near them. The most general fact noticeable in relation to these structural planes, is the approximate parallelism to the rocky walls and terminal moraine confining the icy mass; or in other words, to the surfaces of higher temperature, which inclose the glaciers. However the direction of the ribbon lines may alter by irregularities in the onward flow of the glacier, their position near the region of the nevé is strictly parallel with the surface of the warmer mountain sides."

The other general rule is, that *the foliation is parallel or approximately so to the cleavage*, wherever these two structures occur in the same mass of rocks. This fact, recorded by Darwin, of the gneissic rocks and clay slates of South America, has been noticed likewise by Mr D. Sharpe, Mr David Forbes, Mr Sorby, and other geologists in Great Britain, and by the author, in many localities in Southern Pennsylvania, and in other districts of the Atlantic Slope. An interesting instance of such parallelism of the foliation to the cleavage, tending to show convincingly, that both phenomena are the consequences of one species of force, or only different degrees of development of the same molecular or crystallizing agency, is presented in the great synclinal trough of the lower Appalachian limestone, north of Philadelphia. On the north side of this trough, the Primal and Auroral rocks dip southward over a wide outcrop at a very regular angle of about 45°. On the south side they have been lifted into, and even a little beyond, the perpendicular position, so that the synclinal axis plane of the belt dips at an angle of 65° or 70° to the south. Neither formation shows cleavage structure on the northern side of the valley, the limestone being there of an earthy texture, and in thick massive beds, but on the south or upturned side, this limestone is altered into a mottled blue and white crystalline marble, and is pervaded with cleavage planes, dipping at angles of 70° and 80° southward. Many parts of the rock are like a foliated calcareous gneiss, thin laminæ of mica and talc dividing the slate-like plates of the marble. It is especially worthy of notice that the foliation of these mica and talc, composing some of the thin partings between the original beds of the limestone, is itself very generally parallel to the cleavage in the adjoining calcareous rock. Indeed, wherever the cleavage is excessive, the mass becomes, by introduction of fully developed talc and mica between its laminæ, a true foliated stratum. An especial interest annexes to cases of this kind, from their showing, that in the two contrasted conditions of the absence and presence of metamorphism in the two opposite outcrops of the same synclinal fold, both effects, cleavage and foliation, have originated at the same time, and from one and the same cause, and are, in truth, but different stages of the same crystalline condition, superinduced in the mass by high temperature, at the period of its elevation. The above general fact of the prevailing parallelism of the foliation to the cleavage, is but a corollary of the more general relationship already expressed of the parallelism of the resulting planes of crystallization to the waves of heat, which have produced the metamorphism.

Examination of the Prevailing Theories of Elevation.

Perhaps the most current notion respecting the force which has displaced and elevated the originally horizontal strata of the globe, is that which represents the granitic and volcanic rocks as forcibly injected in a melted state into fissures, and

violently thrust in solid wedge-shaped masses upwards through the incumbent crust. That this is the prevailing idea, is apparent from the manner in which nearly all geological sections, even the most modern ones, designed to represent the relations of the Plutonic to the sedimentary rocks, are to this day constructed. Where igneous rocks constitute the whole, or a large portion of the central axis of a mountain chain, or even that of a simple anticlinal ridge, they are usually represented in cross sections, in the form of a broad wedge, and the stratified rocks are drawn as leaning upon the sloping flanks of the wedge or prism. This is not, I think, the true relation in Nature of the igneous to the sedimentary masses, as I propose to show from the following considerations.

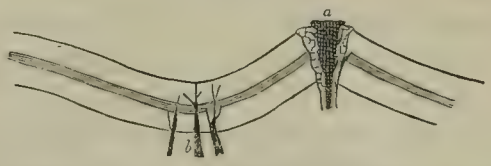
Hypothesis of Wedge-like Intrusion of Melted Matter.

The notion of an upward wedging, or intrusion of molten mineral matter into or through the superincumbent strata, in the manner of a wedge, implies a function in the soft material which belongs to the mechanical action of a solid, and is incompatible with the dynamic properties of fluids. Until a fissure from below first penetrates or traverses the invaded overlying strata, it is not possible to conceive, that the liquid matter could introduce itself in the mode of a wedge. Some force must first crack the crust, and then the molten matter, flowing into the fissure may act as a narrow wedge or key, to keep the walls of the chink distended, but such plates of solidified refrigerated volcanic matter, known as veins and dykes, must necessarily be narrow, and have the shape rather of walls with parallel surfaces, than great wedges broad at the base.* They will also abound chiefly in the districts of subsidence, or in the concave waves, not in those of elevation, or in the convex, where the wedge-like form tapering upwards, is usually represented. Where a rupturing of the strata has taken place in a tract of elevation, or at an anticlinal, the fissure or fissures will be found to gape upwards, and the melted volcanic matter which has flowed to the surface, will be seen widening outwards and tapering as it descends, the very opposite of the form usually assigned to such outbursts, in the igneous axes of uplifted chains. So common is this upward enlargement of the Plutonic masses in certain regions, that it constitutes, I conceive, one element of the fan-shape or inward dip of the boundary walls of the rocks, so frequently encountered in the Alps and other much disturbed mountain systems. A true conception of the formation of a mineral vein or dyke will represent it as the *consequence*, not the cause, of the fissure which it fills, the real process being, not a protrusion of the fluid matter through the crust,

* It is in consequence of this natural expansion of surface-cracks outwards in anticlinals, that the miner so frequently finds his mineral lodes contracting or dying out as he descends. Several striking instances of this thinning of veins downwards could be cited, from the mines of the United States, situated in anticlinal flexures.

breaking it in its passage as a solid wedge might, but an actual injection or pumping of it into the newly opened vacuous cavity, from the pressure or tension below.

Fig. 6.



Dykes expanding upwards in Anticlinals and downwards in Synclinals.

Intrusion of the Igneous Rocks in Solid Wedges.

The other notion, frequently connected with the above idea, of a forcible propulsion of igneous matter through the crust, is that of the violent thrusting upward of volcanic or granitic matter already solidified, in broad wedge-like masses through the strata. This conception I hold to be at variance both with sound mechanical laws, and with the physical facts. For the solid igneous mass to have acted in the manner of a wedge, it is absolutely necessary that it should have moved freely upward through the opening in the strata, which it is supposed to have wedged apart and to have uplifted, and even corrugated, by lateral compression. But it is impossible to imagine such a slipping of the assumed granitic wedge past the edges of the strata confining it, since we can imagine no force acting downward upon these latter, to prevent their moving upward along with the wedge of granite, nor any localization of the force below, to prevent it operating on both alike. We have furthermore no evidence of that discontinuity between the igneous rock and the ruptured strata, which the notion of a sliding wedge obviously presupposes; but, on the contrary, every proof from general theory and from observed facts, that the two descriptions of rock are intimately bound together in closest crystalline contact, keyed together by veins, branching from the mass of the one into the fissures of the other, and even fused together by an actual incorporation of substance. Any upward movement, therefore, of Plutonic masses, bearing sedimentary rocks upon their flanks, cannot have been in the manner of a mechanical wedge; and those results—corrugation for example—of the adjacent strata habitually attributed by many geologists to an imagined wedge-like lateral thrust, must be accounted for upon some other sounder mechanical theory.

A modified form of this conception of an igneous wedge lifting and displacing the strata, assumes no sliding or wedge-like protrusion of the solid granitic matter past the edges of the rupture in the bedded rocks, but recognizing the inseparable cohesion of the two, regards the stratified masses flanking the anticlinal mountain, as merely borne upward by the uprising of the central igneous nucleus. I deem this notion to be a much truer picture of the procedure of nature; for it so far accords with what we notice in anticlinal districts having igneous crests or centres,

that it represents the stratified rocks leaning against the walls of the great granitic central dykes, at steeper and steeper angles, the higher we ascend towards the summits. It is inexact, however, in picturing the granitic nucleus of the anticlinal mountain, as a wedge or broad prism tapering upward, for reasons already shown. Undoubtedly such a mountain, if we can imagine it denuded or truncated to lower and lower levels, would disclose a progressively increasing quantity of intrusive igneous rock, but this would be in the multiplication of the lateral granitic injections, and it is only in this erroneous sense, that the igneous nucleus can be regarded as a prism. Its cross section is branching rather than wedge-like.

The Upward Movement of an Igneous Dyke would tend to Stretch and not to Corrugate the Flexible Strata.

The view here admitted of the elevation of the igneous nucleus of a mountain, along with the strata which mantle it, while it is perfectly compatible with the hypothesis, to be hereafter advanced, of the origin of anticlinals generally, is wholly inconsistent with the somewhat current notion of the mode of origin of undulations and plications in the stratified rocks, by pressure from the tangential horizontal thrust of such uprising igneous axes; so far from its producing a lateral corrugating pressure upon the strata adjoining, and resting against it, a central granitic or other igneous dyke lifted vertically by one or many successive movements, paroxysmal or gradual, would rather *stretch* or distend the strata as it carried them upward than compress them.

Theory of Upward Tension against Lines or Points of the Crust.

Another common theory of crust movement and elevation of anticlinal belts supposes, vaguely, an upward tension or stretching of the crust of the earth along one or several lines, or at one or several focal points, without attempting to account for the linear or focal force, or to assign a cause for the restricted limits within which it is assumed to act. This conception, though confessedly indistinct, is frequently appealed to in explanation of the lifting of mountains, the corrugation of strata, and even the formation of regular groups of parallel anticlinal waves. I propose to consider its weak points.

Any theory henceforth admissible into physical geology, must explain the now clearly established general fact of the regular wave structure of the earth's disturbed zones. But this wave structure cannot be interpreted on the mere supposition of simply an upward pressure exerted either along one or many lines. The peculiar configuration of the crust waves, shown in this paper to be characteristic of them in all undulated regions, requires an hypothesis which will furnish both an undulating and a horizontal tangential motion; moreover, the ordinary doctrine, if it assumes the pressure from beneath to be exerted along a single line at

a time, fails altogether to show us how this pressure could have shifted to new and parallel lines, or how it could take up new positions, and exhibit that relation of relative distances constantly widening, which is seen in all undulated belts. Besides, how could a simple upward pressure along a line in the crust, form a defined or limited anticlinal flexure? Whether the pressure were exerted by a liquid or a solid subterranean mass, it would produce rather a wide general moderate elevation, than a narrow, sharp, anticlinal wave.

If, again, this vague theory be modified to admit the action of a series of linear simultaneous pressures, coincident with the observed anticlinal flexures of an undulated district, it is not possible to understand why, being contiguous, they should not all conspire to lift the outer mass or crust into one general bulge or broad distended dome, rather than into a series of alternately synclinal and anticlinal waves. In addition to these difficulties, this notion of self-awakened lines of pressure, contains no clear hypothesis of the *origin* of the linear forces.

Hypothesis of Corrugation from Sinking of Tracts of the Earth's Surface.

Another theory of the cause of flexures in the Crust conceives them to have been produced from a sinking of the ground by removal of matter by volcanoes, or by the contraction of argillaceous rocks by heat and pressure. Sir C. Lyell, who appears to advocate this view, supposes that pliable beds may, in consequence of unequal degrees of subsidence, become folded to any amount, and have all the appearance of having been compressed by a lateral thrust; and the creeps in coal-mines are adduced as affording an excellent illustration of this fact.* With every respect for this eminent geologist's ingenious views, I must confess that this conception seems to me quite as much beset with difficulties as the somewhat kindred theory of elevation and simple upward protrusion. Apart from the objections that it supplies no cause for the peculiar shape of the crust waves, nor any explanation of their parallelism, and their remarkable laws of gradation, it appears to me quite inadequate to account for lateral corrugation at all, or for more than a very insignificant amount of it. A downward pressure or tension over a single area, produced by release of support arising from vacuities beneath the surface, ought not to engender, on any known mechanical principle, a series of flexures, either within or around the area, but should result in a mere subsidence or flattening of the portion from whence the support has been withdrawn. If the centering of a very flat dome, too weak to sustain itself, be removed, the dome either suddenly collapses with a fracture, or it indents itself, and sinks where it is weakest and most yielding, till it meets the supporting floor. Before the wide nearly level dome of a segment of the earth's crust can corrugate either itself

* See Lyell's Elementary Geology, 5th Ed., p. 50.

or the adjoining strata, some alternate upward and downward force must undulate them, or they must contain alternate weak and strong belts, and even then these must be somewhat undulated; none of which conditions the hypothesis of subsidence is prepared to supply.

Hypothesis of a simple Horizontal Compression.

A somewhat favourite and familiar mode of accounting for the undulation and plication of strata, is that which assumes them to have been corrugated by a purely horizontal or tangential pressure, without elevation and without pulsation; and this imagined mode of folding has been ingeniously illustrated by Sir James Hall, Sir H. De la Beche, and other geologists, by their placing flexible layers of clay, or cloth, or other substances, horizontally under a weight in a trough, and forcing one or both ends towards the centre, so as to contract the length of the strata, and thereby produce a series of miniature plications. It has been alleged that this folding of the clay or cloth is an exact imitation of the flexures of strata seen in nature; but I must deny the assumed analogy. The plications thus produced are merely irregular contortions; they exhibit no definite form of curvature, no constancy in the direction of their gentler and steeper slopes, and no law of regular gradation. Their anticlinal and synclinal axis planes, if they can be said to have any, lean some one way and some another; and the flexures, when the crowding is great, have a tendency to the horse-shoe form, and not to that of waves.

This hypothesis of corrugation, while erroneous in thus failing to present a true representation of the waves of the crust, is also defective in its mechanical principles, for it assigns no cause for the origination of the wave structure. A purely lateral or horizontal force should, as already intimated, simply bulge out to a feeble extent the whole compressed arch, but ought not of itself to wave it; some independent agency, producing alternate upward and downward flexure is indispensable to give even the most powerful tangential pressure the ability to plicate the flexible mass. This hypothesis is furthermore imperfect in not suggesting any cause in nature for the assumed horizontal pressure. It has been already shown, when discussing the hypothesis of simple elevation, and of simple subsidence of areas of the earth's crust, that neither of those movements, unaccompanied by an actual pulsation of the strata, would be competent to corrugate the crust at all; the pure elevation of an igneous axis having the tendency to stretch rather than compress the adjoining strata; and the simple sinking of an area, by retreat of support beneath, having only the effect to irregularly warp the surface, but in nowise to undulate it.

Views of Geologists concerning Cleavage and Foliation.

Professor Sedgwick, as early as 1822, discovered and subsequently publicly taught the true nature of slaty cleavage, distinguishing it from joints, and showing it to be a tendency to separation in perfectly parallel planes, which are irrespective of the bedding. He ascertained that the slaty cleavage is usually confined to the finer-grained rocks, alternating coarser beds possessing it very imperfectly, and laid it down as a rule, that the *strike* of the cleavage is nearly coincident with the strike of the beds He referred it to crystalline or polar forces acting simultaneously and somewhat uniformly in giving directions. Subsequently, Professor Sedgwick in 1835,* after many additional observations on the modifications of slaty cleavage, showed that the rule admitted of many limitations, which the geologist is compelled to notice in working out the structure of complicated districts. In a recent publication, his "Synopsis of the Classification of the British Palæozoic Rocks," he shows conclusively, that the cleavage structure is "the compound effect of all the crystalline forces acting on the mass, and that it cannot be due to a mechanical action." In this work, he also mentions the important fact of the existence frequently of a second cleavage plane, generally inclined at a great angle to the primary.

Sir J. Herschel has suggested that the rocks possessing cleavage may have been so heated as to allow a commencement of crystallization, or heated to a point at which the particles may have begun to move among themselves, or on their own axes; surmising that some general law has determined the positions on which the particles have rested on cooling, and that this position has had some relation to the direction in which the heat escapes.†

Professor Phillips‡ has shown, that in some slaty rocks, fossil shells and trilobites have been much distorted by cleavage; and he imputes this to a creeping movement of the particles of the rocks along the cleavage planes. This displacement, uniform over the same tract of country, he states to be as much as a quarter or even half an inch. Hard shells are not thus affected, but only the thin ones. Professor Phillips, in 1843, stated that the cleavage planes of the slate rocks of North Wales are cleavages parallel to the main direction of the great anticlinal axes.

Mr Daniel Sharpe conceives that the present distorted form of the shells in certain slates, has been produced by a compression in a direction perpendicular to the planes of cleavage, and an expansion in the direction of the cleavage dip.§

He conceives that the planes of cleavage range vertically along certain lines or belts, and dip towards those lines on each side of them; those nearest the central vertical belts at high angles, the angles gradually diminishing as

* Geol. Trans., 2d Series, iii., p. 461.
† Lyell's Manual, p. 610.
‡ Report British Association, Cork, 1843.
§ Quarterly Jour. Geol., viii., p. 87.

the distance from the vertically dipping cleavage increases. This is his explanation of the fan-like arrangement of dip noticed in some countries. "This regularly descending series of planes being found on each side of parallel lines of vertical cleavage, the two series either meet in the centre in a sort of anticlinal axis, or coalesce into an arch. The planes between two lines of vertical cleavage appear to form a complete whole, and the area bounded by the vertical cleavage, may be considered as belonging to one system of cleavage, and may be called an area of elevation of the cleavage." He thinks the cleavage planes are really parts of great curves, which, if completed, would represent a series of semicylinders turned over a common axis.

Mr SHARPE thinks "that there is reason to believe that all slaty rocks have undergone a compression of their mass in a direction perpendicular to the planes of cleavage," connecting with this view his supposition that the cleavage areas are great anticlinal waves. He supposes that the compression of the slaty mass, and its expansion in the direction of the cleavage dip, have been due to the stretching of the strata in the direction of the curve representing the cleavage dips.

Mr CHARLES DARWIN,* reviewing his observations on cleavage in South America, says,—"The cleavage laminæ range over wide areas with remarkable uniformity, being parallel in strike to the main axes of elevation, and generally to the outlines of the coast." He recognizes the fact that the cleavage planes frequently dip at a high angle inwards, and he cites an instance of cleavage dip, in the mount at Monte Video, where "hornblendic slate has an east and west vertical cleavage, with the laminæ on the north and south sides near the summit dipping inwards, as if the upper part had expanded or bulged outwards." Mr DARWIN first proposed the term foliation for the laminæ in gneiss and other crystalline rocks, or the alternating layers or plates of different mineralogical composition. He pointed out the parallelism of the planes of foliation of the mica schists and gneiss with the planes of cleavage of the clay-slate in Tierra del Fuego and Chili, as seen by him in 1835. DARWIN conceives that foliation may be the extreme result of the process of which cleavage is the first effect, or that the crystalline form may have been most energetic in the direction of cleavage. He further suggests, "that the planes of cleavage and foliation are intimately connected with the planes of different tension to which the area was long subjected, after the main fissures or axes of upheavement had been formed, but before the final cessation of all molecular movement," "and that this difference in the tension might affect the crystalline and concretionary processes."

Mr SORBY, adopting the mechanical theory of cleavage, maintains that it varies directly as the mechanical changes, and inversely as the chemical (molecular) changes, which the strata have undergone. He thinks he has shown that the cleavage of certain limestones, microscopically examined by him, varies di-

* See Geological Observations on South America.

rectly as the amount of mechanical compression to which they have been subjected, and that this compression was such as would necessarily change the structure of uncleaved into cleaved rock. He alleges "that cleaved limestones possess no crystalline polarity," and that in place of crystallization producing slaty cleavage, it has a contrary tendency, and, when perfect and complete, obliterates it altogether. Mr Sorby conceives that the absolute condensation of the slate rocks amounts, upon an average, to about one-half of their original volume.* This condensation he ascribes to the forcing together of the particles, and the filling up of their interstices by pressure perpendicular to the cleavage, and partly by elongation in the direction of the cleavage dip.

Mr David Forbes,† writing upon foliation in rocks, leans to the conclusion that foliation is a distinct phenomenon from cleavage, and that the causes producing them were also distinct. He refers the foliation to chemical action, the cleavage to mechanical pressure. He admits that the planes of foliation and those of cleavage are often parallel to one another.‡ But the parallelism of the foliation to the cleavage he ascribes to a previously induced cleavage structure facilitating crystalline lamination in its own planes.

He supposes foliation to have resulted from a chemical action combined with a simultaneous arranging molecular force, developed at heats below the semifusion of the mass; also that the arrangement of foliation is often due to the proximity of igneous rocks, and tends to follow the direction of any lines in the rocks where the cleavage stratification, *or striæ of fusion*, follow preferably those lines offering least resistance.

Examination of the Prevailing Theories of Cleavage and Foliation.

From the theory of the origin of cleavage by mechanical compression exerted perpendicularly to the cleavage planes, as adopted by Mr Sharpe, Mr Sorby, Mr David Forbes, and other geologists, I am constrained to dissent, and upon the following grounds:—

1. It has been already shown, in the general description of the phenomena of cleavage, that this tendency of fissuration is stronger and weaker in alternate closely contiguous planes, and is not diffused equally, even in the one direction, through the mass. Now it is impossible to conceive how a purely mechanical compression could have occasioned a regular alternation of greater and less condensation of particles, all equally free to move and adjust themselves into positions of statical equilibrium, and all equally subjected to the same amount of force. The well-known law of a *quaquaversal* tension of fluids is manifestly applicable to partially soft and flexible rocky matter, if we are to impute to this an

* Lyell, p. 612. † See his Paper, Quarterly Journal, Geological Society, 1855.

‡ See his Paper for a good figure of deflection of cleavage and foliation in the margin of a vein of quartz.

actual rotation of its parts, such as the mechanical theory assumes; and I cannot see why one uniform condition of aggregation should not be the result.

2. In the second place, it assigns no reason for the presence of cleavage planes in fine-grained argillaceous and calcareous rocks, and their absence in silicious ones, both fine-grained and coarse, even when the two classes alternate with each other in intimate parallel contact, where they must have been exposed to precisely the same pressure, both in direction and in amount. In other words, there is no relation discoverable between the known susceptibility of different materials to cleavage, and their susceptibility to compression. But on the other hand, some of the most compressible are the least subject to this peculiar structure. The different susceptibilities of different kinds of mineral matter to molecular polarity is, I conceive, the true explanation of this marked contrast in rocks.

3. Another quite conclusive objection, I conceive, to the pressure theory of cleavage is, that it fails to show how the cleavage-traversed strata can have received the pressure in one constant direction, and under an equalized intensity, through all the contortions and bendings which we know they must have possessed before cleavage was imparted to them. It is obvious that no mechanical pressure, come from what quarter it might, could transmit itself uniformly through convex and concave curves, through bodies of rock placed edgewise and flatwise towards it; but, on the contrary, dynamic considerations must convince us that the resultants of such a pressure would be as various in their directions within the mass, as the ever-changing planes of the corrugated stratification. Not only would the posture of the strata at any point next the quarter of the primary pressure influence the form and direction of the resultant planes of pressure at that point, but the differences in pliability of the different layers compressed would greatly modify them. In other words, while the dip of the cleavage planes within even wide limits, is usually remarkably constant, whatever the contortions of the strata, any pressure transmitted through these contortions must be as various, in different portions of the flexures, as the innumerable resultants produced by the ever-varying resistances and the pressure combined.

4. A like difficulty opposes itself to the pressure theory, in the constancy of the direction of the elongation or stretching of the mass in the line of its cleavage dip. This extension, well expressed by Professor PHILLIPS as a "creeping movement of the particles," seen not only in the fibrous grain of cleavage slates, but in the distortion of imbedded fossils, and of the whole substance of the rock indeed,—ascribed to mere compression by the authors above cited, but attributable, I think, to an actual molecular movement of the mass, in obedience to crystallizing polar forces,—is so equally graduated in amount, and so wonderfully constant in direction (never deviating much from the line of dip of the cleavage plane), that it could never have acquired this constancy from a merely lateral mechanical force, liable to infinite modification, in both of these

respects, by the continually varying resistances consequent on the contortions of the beds.

5. A further objection lies against the pressure theory, in the contradiction it offers between the direction which it assumes the compression to have come from, and the direction in which we can demonstrate the strata to have been actually pressed and moved. In every district of plicated and undulated strata, it can be shown, from the shape of the waves, from the declension in their curvature and height, from their mutual recession, from abatement in all the metamorphic signs of igneous action, and, finally, from the direction of the great planes of fracture in the crust, that the movement and pressure were upward and forward from the quarter of chief crust disturbance. Now it is nearly at right angles to this established direction of the forces, that the hypothesis I am reviewing assumes a pressure to have been applied to produce the cleavage. The planes of fissuration dipping inward towards the igneous side of the belt, any cleavage-producing pressure to be perpendicular to these planes, as the theory alleges it was, must have come either from a point or line elevated at least 45° above the earth's surface, or else from a point or region far below the earth's crust on the opposite side, or in the quarter where the cleavage is absent, or is invariably the least distinct, and where the flexures of the strata, and all other evidences of crust movement, are vanishing. This is, I conceive, a dynamic dilemma in which the compression theory finds itself,—either to make the force emanate from a quarter external to the crust entirely, or from just that quarter where we have the fullest evidence of the absence of any force at all. Thus, if the theory is applied to explain the south-dipping cleavage of the northern flank of the Alps, it implies either that the pressure came, not from within the crust below the crest of the chain, but from some point in the air high over the summits of the mountains, or else from some deep-seated subterranean region far to the north of the Alps, under the undisturbed plains of Northern Switzerland or Germany. In the case of the Appalachians, it requires that the pressure should have come, not from under the convulsed and ruptured region of the Atlantic slope, but from some high aerial point above this, or else from a spot diametric to it, deep under the plains of the Western States, where neither cleavage, metamorphism of any kind, nor undulations of the strata exist, to indicate the former presence there of any compressing force at all. (*See Sections of the Appalachians and Alps.* Figs. 1 and 3.)

6. Besides this general difficulty, I have a special one to offer connected with the laws of cleavage dip. This applies not only to the theoretical generalization of Mr Daniel Sharpe respecting the relations of the cleavage planes to each other in different parts of a zone of slaty cleavage, but to the observations upon which his generalization has been built. His sections of the cleavage in North Wales and elsewhere, represent it as perpendicular or steepest in the belts of maximum igneous action, and flattest in the regions most remote from these, where he places

the anticlinal axes of his cleavage curves. Now, just the reverse of this steepening of the cleavage planes towards the regions of chief metamorphism, will be found to be the real law of gradation in the Appalachians, the Alps, and the district of the Ardennes and Southern Belgium. Obedient to a law already explained, the cleavage dip, following the dip of the axis planes of the flexures, is not most but *least* inclined in the districts most convulsed, and grows progressively steeper, as we advance across the undulations to the districts of minimum disturbance. In the Alps the plications lie flattest next the high central crests of the chain, and there the cleavage dip is often at a very low angle; but receding towards the plain of Switzerland, where the theoretical view requires that it should be flatter, it is really steeper, and even approaches to perpendicularity; and precisely analogous is the gradation when we cross the Appalachians from south-east to north-west. Generalizing the dips of the cleavage planes on both sides of a double belt of flexures like that of the Alps, and excluding the central crests, where the jointage of the igneous rocks, and the cleavage structure impressed by them is more vertical, the real curve of dip for the whole zone will be found to be a *synclinal* one, and not the two halves of two anticlinals, the generating axes of which are far outside the chain, one in the plain of Switzerland, the other in the plain of Northern Italy.

I am much gratified to find, that my objections to the mechanical theory of cleavage find support in the able writings of Professor SEDGWICK, who, in a note in his "Synopsis," states several cogent reasons for rejecting the hypothesis. While some of my own objections are but an expansion of those presented by this eminent geologist; others are independent of his, growing out of my own observations. This accordance gives me additional confidence in the soundness of the generalizations upon which they rest.

THEORETICAL VIEWS.

Theory of the Flexure and Elevation of Undulated Strata.

The wave-like structure of the Appalachians and other undulated zones, has been attributed by the author and his brother, W. B. ROGERS, in their communications to the American Association in 1842, and to the British Association in the same year, to an actual undulation of the supposed flexible crust of the earth, exerted in parallel lines, and propagated in the manner of a horizontal pulsation from the liquid interior of the globe. We suppose the strata of such a region to have been subjected to excessive upward tension, arising from the expansion of molten matter and gaseous vapours, the tension relieved by linear fissures, through which much elastic vapour escaped, the sudden release of pressure adjacent to the lines of fracture, producing violent pulsations on the surface of the liquid below. This oscillating movement in the fluid mass below would communicate a series of

temporary flexures, to the overlying crust, and these flexures would be rendered permanent (or keyed into the forms they present) by the intrusion of molten matter. If, during this oscillation, we conceive the whole heaving tract to have been shoved (or floated) bodily forward in the direction of the advancing waves, the union of this tangential, with the vertical wave-like movement, will explain the peculiar steepening of the front side of each flexure, while a repetition of similar operations would occasion the folding under, or inversion, visible in the more compressed districts. We think that no purely upward or vertical force, exerted either simultaneously or successively along parallel lines, could produce a series of symmetrical flexures, and that a tangential pressure unaccompanied by a vertical force, would result only in an imperceptible bulging of the whole region, or an irregular plication dependent on local inequalities, in the amount of the resistance. The alternate upward and downward movement necessary to enable a tangential force to bend the strata into a series of regular parallel subsiding flexures has been, we conceive, of the nature of a pulsation, such as would arise from a succession of actual waves rolling in a given direction, beneath the earth's crust. It is difficult to account for the phenomena, by any hypothesis of a gradual prolonged pressure exerted either vertically or horizontally. The formation of the grand, yet simple flexures so frequently met with, cannot be explained by a *repetition* of feeble tangential movements, since these could not successively accord, either in their direction or in their amount, nor can it again, by a repetition of merely vertical pressures, for it is impossible to suppose that these could, without some undulating action, shift their positions through a series of symmetrically disposed parallel lines. We find it equally impossible to understand how, if feeble and often repeated, these vertical pressures should always return to the same lines to produce the conspicuous flexures we behold. The oscillations of the crust to which the undulations of the strata are attributed have been, we conceive, of the nature of the Earthquakes of the present day. Earthquakes consist, as we think we have demonstrated, of a true pulsation of the flexible crust of the globe, propelled in parallel low waves of great length and amplitude with prodigious velocity, from lines of fracture, either conspicuous volcanic axes, or half concealed deep-seated fissures, in the outer envelope of the planet.

Theory of Cleavage Structure.

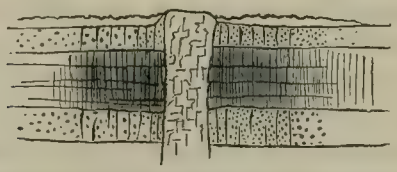
Concerning the cause of slaty cleavage, I have adopted the explanation originally proposed by Professor Sedgwick, that it is due to crystalline or polar forces acting simultaneously and somewhat uniformly in given directions on large masses having a homogeneous composition. And following up the further suggestion in extension of this idea ingeniously proposed by Sir John Herschel, that this molecular force was of the nature of an incipient crystallization, and has been

developed in the particles, by their being heated to a point at which they could begin to move among themselves, or upon their own axis, I have endeavoured to show, that whether the cleavage-traversed strata have been much disturbed or not, the cleavage planes invariably approximate to parallelism with those great planes in the crust, which appear to have been the planes of maximum temperature. It has been already stated in the present paper, that the cleavage dip is parallel to the average dip of the anticlinal and synclinal axis planes, or those bisecting the flexures. Now, it is easy to prove, that these axis planes, and the inverted parts of the flexures, are just those portions where the greatest wrenching, fissuring, and opening of the strata must have occurred, and where the highly-heated, pent up, volcanic steam and gases, and liquid mineral matter, must have found their chief channels upwards to the surface.

Without attempting at present to apply this doctrine in detail, I will content myself with reviving a suggestion I formerly put forth, that every plicated belt of strata may be looked upon as having, from the causes here adverted to, become traversed at the time of their folding and metamorphism, by a series of alternate hotter and cooler parallel planes or zones of temperature, arranged in oblique dip, coincident approximately with the axis planes of the flexures. These planes or surfaces of high temperature, we may suppose to have acted to polarize the particles in corresponding planes, by transmitting through the half-softened mass, a succession of parallel waves of heat, stimulating the molecular crystallizing forces, which are ever resident in mineral matter, and which only await there the quickening influence of such a temperature, to develop in the mass special lines and surfaces of maximum and minimum cohesion.

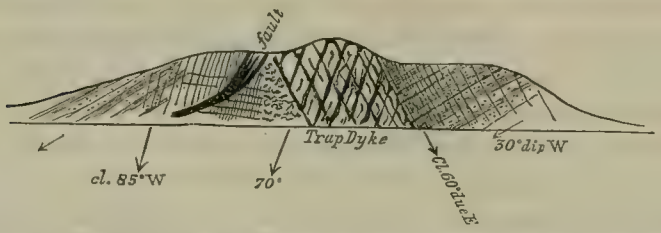
This conception, that the surfaces or planes of crystalline lamination, including cleavage, which is but a lower grade of the same species of molecular metamorphism, are approximately parallel to the surfaces of the waves of temperature, which have moved through the strata, is not a mere hypothetical speculation, but an induction at which I have arrived, from a comparison of many observations of my own, with phenomena well recorded by the ablest geologists. Nearly all observers who have noted the influence of igneous dykes and veins upon the strata adjoining them, both in mines and external exposures, have seen a more or less distinct lamination or cleavage adjoining the walls of the once heated mineral matter, and have been struck by its very general parallelism, to the surface or the axis of the vein. Cases occur in strata of all ages, and are frequently brought to light in coal-fields, when nearly vertical dykes cutting low dipping or horizontal shales, susceptible of the cleavage metamorphism, have occasioned in the latter a true cleavage perpendicular to the stratification, or parallel, more strictly speaking, with the once hot surface of the intrusive rock.

Fig. 7.



Cleavage in Red Shale caused by a Trap Dyke parallel to its walls, near New Hope, Pennsylvania.*

Fig. 8.



Cleavage superinduced by a Trap Dyke in Red Argillaceous Sandstone of Jurassic age, west of Gettysburg, Pennsylvania.

Other instances are often presented of masses of superincumbent trap rock, baking and altering argillaceous and other strata, in which a like law of parallelism of the cleavage to the heat-imparting surface of the molten matter is shown in the horizontality of the cleavage planes, whatever be the dip of the strata. Numerous examples can be cited, where one igneous dyke cutting another, or traversing a mass of earlier Plutonic rock, produces in the latter a crystalline grain, amounting to a sort of cleavage, adjoining the bounding surfaces of the newer injection, and in planes invariably parallel, or nearly so, to the walls of the fissure. A similar fact of the occurrence of a cleavage parallel to the walls of highly heated fissures, may be seen in the faults and great dislocations which traverse some parts of the anthracite coal basins of Pennsylvania. Here the greatly indurated argillaceous shales, and even sometimes the coal itself, display a cleavage-structure invariably parallel to the general plane of the fracture. Such fissures would be the natural channels through which heated volcanic steam would ascend from the interior, and the action of this upon the strata most susceptible of cleavage would be precisely analogous to that of a molten dyke, in transmitting a wave of heat perpendicular to its surface, partially softening and half polarizing the matter as it passed.

* There is a similar instance cited by Professor Phillips, I think in his Geology of Yorkshire

General Resumé.

1. *Wave-like form of all belts of uplifted strata.*

a. It is a general fact that strata dip in curved and not in straight planes.

b. Wherever wide areas of the crust have been elevated or depressed from the level at which their strata were deposited, these strata will be found, except where their dip is disordered by crust dislocations, to constitute, in their varying angles of dip, one or more wide regular curves.

2. *Parallelism of crust undulations.*

a. It is another general fact, that these undulations of the strata are in the form of long parallel waves, resembling much those great continuous billows called in dynamics waves of translation, and by seamen rollers.

3. *Relations of flexures.*

a. Parallelism of the waves to the general trend of the part of the mountain system to which they belong, and especially to its chief igneous axis.

b. Parallelism of flexures extends not only to adjacent individual waves, but to contiguous groups, and is as true of curvilinear as of straight.

c. The waves of the strata are generally of two or three grades of magnitude, as respects their length, height, and amplitude, and while those of the same grade are parallel, the different grades are not necessarily so.

4. *Laws of form and gradation of waves.*

There are three characteristic forms of crust waves; symmetrical flexures equally steep on the two slopes; normal flexures, curving more rapidly on one side than on the other; and folded flexures, or those with a doubling under of their more incurved slopes, and among which the steepest slopes are generally directed to the same quarter.

The geometric planes bisecting the anticlinal and synclinal bends of the strata, here called axis planes, are nearly perpendicular in the symmetrical waves, but inclined in the other two classes, dipping at the lowest angle in the folded flexures. In many belts the plication is such as to amount to parallelism of all the inverted to the uninverted sides of the waves.

Some waves are straight, some curvilinear and crescent shaped, and many of them extremely regular, changing their trend 40° or even 50°. The curvilinear ones convex *from* the disturbed sides of the zones, are generally more regular than those which are convex *towards* them.

5. *Gradations in flexures.*

a. In all undulated zones, the succession, starting from the most disturbed side, is invariably from the folded waves to the unequally sloping or normal ones, and from these to the equally sloping or symmetrical.

b. The waves grow progressively wider apart, or increase their amplitude, as they pass from the folded to the equally sloping form.

c. The waves progressively flatten down as they recede from the folded side of the belt.

d. The axis planes of the flexures of any great undulated zone all incline towards the same quarter, that of maximum disturbance, the angle of inclination being less the nearer the wave or plication is to that side.

6. *Fractures or faults.*

In undulated districts, the dislocations are of two kinds: (1.) Numerous short ones, transverse to the strike of the axes, and shifting the strata to but a trivial extent; (2.) longitudinal ones, fewer in number, of great length, and producing often great displacement.

The longitudinal faults very generally dip towards the same quarter as the axis planes; indeed they are either ruptures in the axis planes of the flexures, or in the steep or inverted sides of the waves.

This slanting of the plane of dislocation parallel with the leaning of the wave, causes the newer or upper formations, on the inverted side, to dip under the older or lower on the uninverted side of the flexure or the fault, for almost always the uninverted side has been shoved forward and upward across the inverted.

Some undulated belts are single, or have all the axis planes of the flexures dipping to one quarter, as the Appalachians and the zone of Southern Belgium. Others are double, or consist of two such zones, both dipping inwards towards one central line of chief igneous disturbance, and these latter present in this inward general leaning a fan-like structure, as in the Alps.

7. *Phenomena of slaty cleavage.*

a. The cleavage dip is independent of the dip of the strata; and still more remarkably, the cleavage planes of a district are generally parallel to the axis planes of its flexures.

b. Immediately within the anticlinal and synclinal axes, the cleavage planes depart from their parallelism to the axis planes to dip inwards towards them in a kind of fan-like arrangement.

c. The cleavage is only present where the rocks consist of certain materials, abounding most where they are most argillaceous and of finest texture, disappearing and reappearing with changes in the composition of the strata, even where this closely alternates.

d. In such groups of alternating cleavable and non-cleavable beds, the cleavage planes curve from the normal dip they possess to approach to a parallelism with the planes of separation of the strata as they near their surfaces.

e. The cleavage susceptibility is alternately greater and less in closely adjacent parallel planes.

The ribbon structure of glaciers is probably analogous to the cleavage structure of argillaceous rocks.

8. *Foliation.*

In districts of crystalline, metamorphic, or gneissic strata, not much disturbed or corrugated, the foliation generally coincides with the stratification. In regions much corrugated the foliation, on the contrary, is often at a steep angle to the stratification, and shows a tendency to dip, as cleavage does, parallel to the axis planes of the flexures. Generally the direction of the foliation appears to conform to that which the waves of heat metamorphosing the rocks would take in slowly flowing through them.

9. *Theories of elevation.*

a. A common hypothesis of the cause of the elevation of strata is that of a wedge-like intrusion of melted matter. But this implies a function in semifluid or fluid matter incompatible with the dynamic properties of liquids. Some force must have first cracked the strata before the molten rock could insert itself. Veins and dykes tapering upward do not belong to lines of anticlinal elevation, where geologists so frequently indicate them, but to synclinals or concave curves.

b. The kindred idea of the intrusion of igneous rocks in solid wedges separating and lifting the crust is also at variance with sound mechanical laws. To exert this lifting and thrusting force, the assumed wedges must have moved freely through the fissures they fill; but we see no proofs of discontinuity between the igneous and stratified rocks, but evidences of the closest cohesion.

c. A modified view of the wedging up of the flexible strata, conceives them to have been simply carried up by the lifting of the igneous nucleus. Such movements have no doubt occurred, and have served to steepen the strata leaning against the igneous rocks, but they cannot have corrugated the strata, which would be rather stretched than compressed by the elevation.

d. The hypothesis of a simple upward pressure at points or lines in the crust, which does not include an explanation of the wave structure of disturbed districts, cannot be a true theory; it must show how the pressures have shifted to new and parallel lines, and lines constantly receding, and also show why, if the linear pressures were simultaneous, they should not have produced a wide general arching, rather than a series of contiguous sharp waves.

e. The hypothesis of the origin of flexures from a sinking of the ground by re-

moval of volcanic matter beneath it, supplies no explanation of the origin of zones of regular undulations. The sinking of any weak segment of the earth's crust might produce a trivial general warping, but not a belt of waves.

f. The hypothesis of a simple lateral or horizontal compression, illustrated by the folding of layers of any flexible material squeezed edgewise, and kept down by a weight, is open to the objection that this mode of folding offers no true analogy to the great symmetrical parallel flexures met with in nature. Flexures thus artificially produced, show neither the forms nor the gradations characteristic of the crust waves. A purely tangential force would cause the district within its influence to bulge slightly upward, but not to corrugate into regular undulations, and it fails to find an origin for a pressure in the direction assumed.

10. *Theories of cleavage and foliation.*

a. The prevailing notions of geologists respecting the origin of cleavage and foliation are, on the one hand, that they have been produced by different intensities of molecular crystallizing polarities, excited by heat operating in a definite direction, on the other hand, that they have been caused by mechanical compression of the strata applied perpendicularly to the cleavage and foliation planes.

b. One main objection to the pressure hypothesis is, that it does not account for the existence of planes of alternately stronger and weaker cohesion.

c. Another difficulty is, that it fails to explain the dependence of cleavage upon the texture of the rock, especially its chemical nature, and particularly where cleavable and non-cleavable strata alternate in close contact.

d. A third important objection exists in the dynamic difficulty, that the cleavage is nearly parallel and constant in its dip, despite the inequalities which a lateral pressure should undergo in its transmission through all the contortions and various postures prevailing in the strata.

e. A fourth difficulty, analogous to the last, is presented by the constancy in the amount and direction of the elongation or creep of the cleavage rocks in the direction of their cleavage dip; a constancy not compatible with the ever-varying tension which the flexures and bendings of the strata would occasion.

f. An additional objection presents itself, in the direction of the pressure implied by the theory, which, assuming the force to have been perpendicular to the cleavage planes, implies it to have come either from a source above the earth's surface, on the side towards which the cleavage planes are dipping, or from a source far beneath the crust, in that quarter where invariably the cleavage and all other symptoms of metamorphism are least abundant, or entirely wanting.

g. Still another important objection arises, in the contradiction exhibited between the law of gradation in the steepening of the cleavage dip demanded by the theory, or one form of it, and the actual law of the gradation of this dip

witnessed in all districts of regular crust plications. The theory represents the cleavage dip as growing progressively steeper the nearer it is to the lines of greatest igneous action, the facts in nature show that the cleavage, the foliation, and the axis planes of the flexures, with which these are approximately parallel, grow progressively steeper the farther they recede from those lines of maximum energy.

Concluding theoretical views.

The wave-like structure of undulated belts of the earth's crust is attributed to an actual pulsation in the fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The forms of the waves, the close plication of the strata, and the permanent bracing of the flexures, are ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the rents occasioned by the bendings.

This oscillation of the crust, producing an actual floating forward of the rocky part, has been, it is conceived, of the nature of that pulsation which all great earthquakes produce at the present day.

11. *Cleavage.*

The cleavage planes having been shown to be parallel to the axis planes of the flexures, and locally to the planes of the great faults, and these being obviously the belts of maximum temperature in a plicated district, it is suggested that both cleavage and foliation are due to the parallel transmission of planes or waves of heat, awakening the molecular forces, and determining their direction.

XXXI.—*On New Forms of Marine Diatomaceæ, found in the Firth of Clyde and in Loch Fine.* By William Gregory, M.D., F.R.S.E., Professor of Chemistry. *Illustrated by numerous Figures, drawn by* R. K. Greville, LL.D., F.R.S.E.

(Read 19th January 1857.)

In two papers read before this Society, I have very fully described the Diatomaceæ of the Glenshira Sand, which is very remarkable both for the large number of species found in it, which is certainly more than 320, and for the circumstances in which it must have been deposited. There can be no doubt, from the nature of the locality, which I have lately visited, that this bed was formed in the bottom of the Dhu Loch, a shallow fresh-water lake, at that time extending about two miles farther up the valley than it now does, and being at a higher level. In consequence of a rise in the level of the land, or a fall in that of the sea (from which—that is, from Loch Fine, the lower end of the lake is separated by a narrow and low barrier, through which the waters of the lake pass to Loch Fine), the lake has long ago been drained, till its upper end is nearly two miles from the point it must have reached when the bed of sand was formed. The present level of the lake is considerably lower than it was then; the precise difference I had no means of ascertaining, but I believe it is about 30 feet. Now, the most interesting fact about this lake is, that its actual level is that of half-tide, so that at low water the lake is discharged into the sea, while at high water the tide flows upward into the lake. Hence marine plants and animals are found in the Dhu Loch; herring, for example, are often caught in it, and were taken while I was in the neighbourhood. Hence also the present deposit in the lake exhibits a mixture of fresh-water and marine Diatomaceous forms. Now, the older sand, the subject of my paper, deposited at a considerably higher level, also contains both marine and fresh-water Diatoms; and while the individuals of the two classes are both abundant, the marine species are at least twice, perhaps thrice, as numerous as those of fresh water.

The natural, and, I have no doubt, the true explanation of the occurrence of so many marine forms in an inland deposit, formed in a fresh-water lake, is this: that at the period when the sand was formed the relative levels of the Dhu Loch and of Loch Fine were the same as now, when similar results ensue.

But as the lake was then at a higher level than now, so also must the sea have been at a level as much above its present one. This conclusion is in accordance with those derived from the observations made on raised beaches on the

banks of the Firth of Clyde, the level of which must always have regulated that of Loch Fine since the present form of the coast has existed.

There was, however, a circumstance which at first tended to throw some doubt on this conclusion, according to which the marine forms of the Glenshira sand must have come from Loch Fine. For although the known and described marine Diatoms found in the sand occur on our coasts, yet I was struck with the fact that out of upwards of fifty new or undescribed forms, there seemed to be no trace in deposits from the Firth of Clyde, examined by more than one naturalist during the progress of my investigation. The fact of these forms being undescribed was *prima facie* evidence that they had not yet occurred on the British coasts.

Yet it was evident that the formation of the Glenshira sand was, geologically speaking, very recent; so recent, indeed, that we could not suppose any number of species to have since become extinct. I came, accordingly, to the conclusion, that these undescribed forms must still exist in the waters of Loch Fine, or, what is the same thing, of the Firth of Clyde. I was therefore desirous to examine with care deposits from these waters, and this, during the past six months, I have been enabled fully to do.

The materials which I have examined are the following:—

1. A small quantity of dirt or sand washed from some nests of *Lima hians*, dredged in Lamlash Bay on the 19th of July last, in 4 fathoms, by Professor Allman. This material, though, when cleaned, very scanty, proved the richest of all.

2. Four dredgings, made by myself, with the kind assistance of the Duke of Argyll, in Loch Fine, at different points within two or three miles of Inveraray. These were all different, and three of them were interesting. They were taken at depths of from 14 to 18 fathoms, early in October last.

3. Three dredgings made at the same time by the Rev. Dr Barclay, in Loch Fine, off Strachur, at depths of 15, 20, and 60 fathoms, also in October last.

4. Three materials forwarded to me in October by the Rev. Mr Miles of Glasgow, who was for some time on the Holy Island, in Lamlash Bay.

One of these was washed from the nests of *Lima hians*, as I had reported the richness of the former. These last were from 7 fathoms in Lamlash Bay, This material, dredged, I think, in June, was not so rich in Diatoms as Professor Allman's, but yet contained many interesting forms.

The second was a coarse red sand, dredged off Invercloy, Arran, which was rather poor.

The third was a mass of *Corallina officinalis*, taken with the hand, in rocky pools, at Corregills, Arran, when the tide was low. The Corallina proved to have been a good Diatom trap, and yielded a material, not remarkable for the number

of species, but rich in individuals, and these nearly all of interesting, rare, or new species.

I had thus eleven different materials, no two of which were exactly alike, although in all certain prevalent forms occurred. In each, on the other hand, some forms, few or many, were peculiar, and their presence gave a distinct character. A careful study of the whole has yielded interesting results; and these it is the object of the present paper to state as briefly as may be consistent with accuracy.

The first observation is, that these waters contain a very large proportion of all the known and described marine forms belonging to Britain, including a good many which have hitherto been very rare; so scarce, indeed, in some instances, that few observers have seen them. I may specify the following as being by no means rare, several, indeed, being abundant in these materials:—

Coscinodiscus concinuus.
Eupodiscus crassus.
... Ralfsii.
... sculptus.
Campylodiscus Ralfsii.
... Horologium.
Navicula Hennedyi.
... granulata, Bréb.
... Lyra, Ehr.
Pleurosigma rigidum.
... obscurum.
Pleurosigma delicatulum.
... transversale.
Surirella lata.
Hunantidium (?) Williamsoni.
Amphiprora elegans.
Podosira Montagnei.
Orthosira marina.
Grammatophora macilenta.
Biddulphia Baileyi.
... turgida.

The second observation which I made was, that, as I had anticipated, nearly the whole of the new forms figured by me from the Glenshira sand are found living, and generally abundant, in these waters. The following list contains the names of such of the marine species, figured in my former papers, as I have found in the new materials:—

Cocconeis distans.
... costata.
Eupodiscus Ralfsii; also var. β, sparsus.
Campylodiscus simulans.
Surirella fastuosa, very large.
Amphiprora recta.
... lepidoptera.
Navicula rhombica.
... maxima.
... angulosa, and var. β.
... humerosa.
... latissima.
... clavata.
... splendida.
... incurvata.
... didyma, var. γ, costate
Navicula didyma δ.
... crassa.
Pinnularia Pandura.
... longa.
... inflexa.
Amphora Arcus.
... crassa.
... elegans.
... plicata.
... obtusa.
... Grevilliana.
... rectangularis.
... lineata.
Synedra undulata.
Tryblionella constricta.
... apiculata.

I think we can hardly doubt that all the new Glenshira marine forms will ultimately be found in the neighbouring waters.

Before going farther, I have to remark, that two of the forms in the first list above given, namely, *Campylodiscus Horologium* and *Himantidium Williamsoni*, which had only been found by Professor WILLIAMSON, who detected them both in a dredging made by Mr BARLEE on the coast of Skye, in which they were very scarce indeed, have occurred abundantly, the former in one of the Loch Fine dredgings, and sparingly in some of the others, the latter in another of them, and, though less abundantly, yet frequent in nearly all the Clyde materials. We shall see that *Himantidium Williamsoni*, which Professor SMITH had referred doubtfully to that genus, not having been able to see more than the front view of it, is really no Himantidium; the side view, which is very abundant in one of my dredgings, having characters quite incompatible with the genus Himantidium. On this account, I shall refer to it among the new forms which I have to mention. I have found it a matter of very great difficulty, if not impossible, to refer it to any of the genera in SMITH's Synopsis. I may here add, that *Synedra undulata*, which I had recognised in the Glenshira sand, but which had never occurred entire in that deposit, is frequent in the first material from Lamlash Bay (Professor ALLMAN's), where it occurs quite entire in more than half of those I have seen, and, as I had concluded, from the imperfect specimens I had seen, attains a length of from 0·015 to about 0·02, which, for a Diatom, is gigantic. I had previously noticed a fragment of it in a recent gathering made by Professor SMITH, and he had himself subsequently found it frequent in Cork harbour. The first observer, however, was Professor BAILEY, of West Point, New York, who had found it still larger on the American coast, which I was not aware of till long after my observations on the Glenshira sand were made.

The third observation I shall here record is, that in these dredgings I found, in sufficient abundance, several very curious forms which had occurred in the Glenshira sand; but the description and figuring of which I had postponed, because either they were so scarce that I could not obtain good specimens, or, being only found in a fragmentary, detached, or imperfect state, I was quite at a loss to determine their true nature and position. I think I may say that in every such case I have been enabled, by the study of the new materials, to understand the nature and structure of these obscure or doubtful forms, and to establish them as new and distinct species. I have also been enabled to understand better several of the forms which were figured in my former papers, and to correct some errors which had crept into these.

I need not here give a list of the forms just alluded to, as they will be included in that of the new forms to be described. In that list, I shall mark them with a G, to indicate that they were first noticed in the Glenshira sand.

Lastly, in the new materials I have found a large number of entirely new and undescribed species, which I shall now proceed to enumerate. I may here mention, that although a good many fresh-water forms do occur in these dredgings,

as must, indeed, be the case, since the Clyde and all its tributaries bring down such forms, yet the new forms in question appear to be all of marine origin. They are, in general, much too abundant to have been derived from any other quarter, whereas the fresh-water forms among them are much scattered. It is proper also to state, that although all these forms are, to the best of my belief, new to Britain, yet a few of them have been described by Ehrenberg in some of his numerous works, and also by De Brebisson. The great majority, however, have not anywhere been figured; not, at least, in any works accessible to me.

As the new forms belong to a very few genera, it will be convenient to arrange them in groups. Those I shall adopt are as follow:—

I. Naviculoid Forms.
II. Cocconeides.
III. Filamentous Forms.
IV. Discs, including Campylodisci.
V. Amphiproræ.
VI. Amphoræ. { A. Simple. B. Complex.
VII. Miscellaneous.

GROUP I.

Naviculoid Forms.

These, as is usual in all gatherings, are numerous. Including two or three varieties of species already known, those which I have recognised as new are the following:—

1. Navicula minor, n. sp.
2. ... Cluthensis, n. sp.
3. ... inconspicua, n. sp.
4. ... brevis, n. sp.
5. ... Claviculus, n. sp.
6. ... Musca, n. sp.
7. ... rectangulata, n. sp.
8. ... nebulosa, n. sp.
9. ... Barclayana, n. sp.
10. Navicula spectabilis, n. sp.
11. ... prætexta, Ehr.
12. ... Bombus, Ehr.
13. ... Lyra, Ehr.
14. ... Lyra, Ehr. var. β, abrupta.
15. ... Smithii, var. β, fusca.
16. ... Smithii, var. γ, nitescens.
17. ... Smithii, var. δ, suborbicularis.
18. ... maxima, Greg.

1. *Navicula minor*, n. sp. Pl. IX., fig. 1. Form rectangular in the middle, acuminate at the ends, which are acute. Length from 0·0012″ to 0·0025″; breadth 0·0004″ to 0·0008″. Striæ fine, inclined near the ends, not reaching the median line, 36 to 40 in 0·001″. The whole form has a delicate aspect.

This little form, represented in fig. 1, occurs in two or three of the Loch Fine dredgings, in one of which it is sufficiently frequent.

2. *Navicula Cluthensis*,* n. sp. Pl. IX., fig. 2. Form oval, rather broad. Median line broader at the centre, narrower at the apices. Central nodule definite, large; terminal nodules smaller. Length 0·0013″ to 0·0016″; breadth about 0·001″. Striæ conspicuous, clear, and sharp, inclined slightly in the middle, strongly near the ends; about 20 in 0·001″.

Fig. 2 represents this form, which occurs in Professor Allman's dredging from Lamlash Bay, and though not abundant, is yet frequent enough for all practical purposes. It is very uniform in its characters, and though the description above given may not appear very characteristic, yet I know of no form with which this one can be confounded. Its aspect is so peculiar that it is instantly recognised.

3. *Navicula* (?) *inconspicua*, n. sp. Pl. IX., fig. 3. Form linear, rather narrow, with rounded ends. Median line strong, complex, interrupted in the middle. Nodule definite. Aspect of valve hyaline. Striation so fine that it has not yet been resolved; and at all events it cannot be visible under a power of 400. Length 0·002″ to 0·0032; breadth about 0·00035.″

This little form occurs both in Lamlash Bay and in Loch Fine. I do not feel quite sure that it is a Navicula, as it may possibly belong to a filamentous species; it may be, for example, a Diadesmis; or it may prove to be a Schizonema. This can only be ascertained by observations on examples in the living state.

4. *Navicula brevis*, n. sp. Pl. IX., fig. 4. Form nearly elliptical in the middle; broad, short; contracted to short, produced, obtuse extremities. Length about 0·0023″; greatest breadth 0·0013″. Striæ fine, about 35 in 0·001″; very slightly inclined, not reaching the median line, and at the centre leaving a large, round, blank spot,within which the two halves of the median line end in small rounded expansions. Central nodule indefinite; terminal nodules definite.

This form is easily recognised by its short, squat shape, and is distinguished from *N. semen*, which it resembles in form, by its much finer striation. It occurs in Lamlash Bay, and is not very scarce in Professor Allman's dredging from that locality.

5. *Navicula Clavículus*, n. sp. Pl. IX., figs. 5, 5 *b*, and 5 *c*. Form of valve linear, narrow, with one central and two terminal expansions, separated only by two constrictions. The terminal expansions are much elongated, oval at the extremities, and rather broader than the central expansion. Central nodule definite. Length 0·0015″ to 0·002″; greatest breadth 0·0002″ to 0·0003″. On the S.V., figs. 5 and 5 *b*, the central expansion is unstriated. Striæ on the rest of the valve transverse, sharp, not quite reaching the median line; about 32 in 0·001″. The F.V., fig. 5 *c*, is rectangular, with slightly expanded and bevelled angles, and exhibits the same absence of striæ from the middle part.† The striæ are seen to a

* From Clutha, the Clyde.

† Figs. 5 *b* and 5 *c* are magnified 800 diameters, to bring out the details. Fig. 5 is magnified only 400 times, and is below the average size.

certain extent on the F.V., but most towards the extremities, indicating that the S.V. is more convex near the ends than in the middle. I have named this form from its resemblance to a small two-headed club.

It occurs only in one of the Loch Fine dredgings, in which, though far from frequent, I have been able to examine many more specimens than are required to ascertain the characters of the dead form. I observe that it often occurs in pairs, as well as solitary, so that it may perhaps belong to a filamentous genus, such as Diadesmis. But as I cannot be sure of this, without seeing the living or growing form, I refer it, for the present, to Navicula.

6. *Navicula Musca*, n. sp. Pl. IX., fig. 6. Form of valve deeply constricted in the middle, broadest at a point near the middle on each side of it, and almost triangular thence to the acute apices. Length 0·002″; greatest breadth 0·0011″; breadth at middle 0·00075″. Striation confined to a marginal band, which is rather broad, and nearly of uniform width, except at the apices. Median line sharp; central nodule definite. Striæ coarse, 18 in 0·001″; distant, moniliform. Aspect of the valve transparent.

The form of this very pretty species is allied to that of *N. didyma* and the other panduriform Naviculæ, which are so frequent in marine gatherings. Even its form, however, is peculiar, and it is at once distinguished from all the others by its marginal striation. It so much resembles in shape the body of a bee or wasp, that I should have named it Apis or Vespa, had not these names been already appropriated to other species by Ehrenberg. I have chosen, therefore, the specific name Musca, as the form is also that of various large flies. It occurs in the same Loch Fine gathering, as Nos. 1, 3, and 5; a gathering which, though very scanty and very stony, has proved singularly rich in undescribed forms, especially of Amphoræ, as we shall see farther on. This dredging was a very coarse sand, which, after boiling with acid, I was on the point of rejecting as useless, when I observed a very trifling cloud of finer matter. This, though full of mica, supplied a remarkable proportion of new species; so much so, that I believe it contained as many of these as of known species; and of the undescribed forms found in it, a majority have occurred in it alone. I mention these facts, in order to show that every dredging, however unpromising, in such localities as Loch Fine and the Clyde, ought to be closely examined. This one was most unpromising; yet it turned out not only rich in new species, but very different from the other dredgings made in the same waters.

7. *Navicula rectangulata*, n. sp. Pl. IX., fig. 7. Form of S.V. rectangular, the extremities being rounded; rather narrow. Length from 0·003″ to 0·004″; breadth about 0·0006″ to 0·0008″. Striation highly radiate, there being three centres of radiation on each side—one in the middle, and one at each end. Striæ soft, not very fine, subdistant; about 22 in 0·001″, not quite reaching the median

line, and leaving a very small, round, blank space in the centre. Central nodule indefinite. The F.V. has not yet been recognised.

This form is remarkable for the shape of the S.V., which is that usually found in the F.V. It occurs rather sparingly in Professor Allman's dredging from Lamlash Bay, which, of all the dredgings, is the richest in species, whether known or undescribed.

8. *Navicula nebulosa*, n. sp. Pl. IX., fig. 8. Form oval, broad; generally with a slight tendency to angularity in the middle, and also a tendency to acumination at the apices. Length from 0·0025″ to 0·0035″; breadth 0·0013″ to 0·0016″. Median line sharp, ending in two elongated expansions at the centre, which do not meet. Nodlue indefinite. On each side of, and close to, the median line, is a narrow rectangular band of striation, interrupted at the nodule. At the margin is a somewhat broader, but still narrow, striated band, almost exactly of uniform width throughout. Striæ 34 to 36 in 0·001″. Aspect of valve hazy and indistinct. Striated portions pale blue under the half-inch objective. This form is allied to *N. Hennedyi*, figured in my second paper on the Glenshira sand (see *Trans. Mic. Soc.*, vol. iv., pl. v., fig. 3.) But I have found it necessary to separate it from that species, in consequence of its very different aspect. *N. nebulosa* is a smaller form than *N. Hennedyi*, the one here figured being an unusually large one. It is also much more finely striated; and, above all, it has invariably that peculiar indistinctness of aspect from which I have named it; whereas *N. Hennedyi*, even when of a smaller size, as we sometimes find it, is always remarkable for the sharpness of its markings. The tendency to angularity generally seen in *N. nebulosa* is never found in *N. Hennedyi*. Lastly, the striation of the former is so much finer, that the striated parts, seen under a low power, have a very pale bluish tinge never seen in the latter. When the two forms are seen in the same field of view, as often happens in Professor Allman's dredging from Lamlash Bay, and even when *N. Hennedyi* is the smaller, though it is generally much larger, the difference between them is very striking. I might have considered *N. nebulosa* as a variety of *N. Hennedyi*, but that I have found both forms exceedingly uniform in their characters, and have not been able to observe any tendency to transition from one to the other. *N. nebulosa* is frequent in the Lamlash Bay dredging just mentioned, in which *N. Hennedyi* also occurs; but elsewhere I have hardly ever seen the present species.

9. *Navicula Barclayana*, n. sp. Pl. IX., fig. 9. Form an elongated oval, somewhat suddenly contracted to acute extremities, terminated by small round apiculi. Median line narrow, ending at the middle in two small expansions. Nodule indefinite. Length 0·004″ to 0·0045″; breadth 0·001″ to 0·0012″. Striæ about 38 in 0·001″, somewhat inclined, sharp, minutely moniliform, confined to a marginal band, which is rather narrow, and of uniform width except near the apices, where it becomes narrower.

This is a fine conspicuous form, and occurs not unfrequently in the same Loch

Fine dredging, with *N. Claviculus*, *N. Musca*, &c. I have also seen it, though much more sparingly, in some of the other dredgings, both from Loch Fine and Lamlash Bay.

10. *Navicula spectabilis*, n. sp. Pl. IX., fig. 10. Form elliptic-lanceolate, very broad; ends subacute or acute. Length from 0·003″ to 0·005″; breadth from 0·0023″ to 0·0032″. Median line sharp, having close to it on each side a narrow striated band, interrupted at the middle. Central nodule large, indefinite. There is a marginal band of striation, which is very broad in the middle, where it projects inwards to an obtuse point, and very narrow at the apices. Striæ coarse, moniliform, about 22 in 0·001″. The blank spaces between the marginal and central bands are very broad; and this part of the valve is so thick and strong, that in fractured specimens we never find it broken across, but we often see the entire blanks, united by the central nodule, which is elongated laterally, separated from all the striated parts, forming a singular object.

This form is allied to *N. Lyra*, Ehr., and also to *N. Hennedyi*. I consider it, however, quite distinct from either. It occurs frequently in Lamlash Bay; but I have not yet seen it elsewhere. The form and width of the marginal band distinguish it from *N. Hennedyi*, while the broad blank spaces distinguish it from *N. Lyra*, in which, as we shall see, these spaces are linear. Besides this, *N. Lyra* very often occurs with produced ends, and never has the peculiar form of *N. spectabilis*. The latter never occurs with produced ends. Moreover, under a low power, *N. spectabilis* has a bright brown colour, in the striated parts, not observed in *N. Lyra*. Lastly, I find this form remarkably uniform and constant in its characters. It is very conspicuous, and generally larger than *N. Lyra*.

11. *Navicula prætexta*=*Pinnularia prætexta*, Ehr. Pl. IX., fig. 11. Form a pure and broad oval. Length from 0·004″ to 0·005″; breadth 0·0025″ to 0·003″. Median line sharp, the central extremities ending in large, rounded expansions, which are bent to the same side. Central nodule indefinite, extending transversely. On each side of, and close to the median line, a narrow linear band of very coarse and coarsely moniliform striæ. At the margin of the valve is a rather broad band of striæ, exactly like those of the central bands. This marginal band is of uniform width till near the apices, where it gradually becomes narrow. Striæ 8 to 10 in 0·001″. The broad intermediate space between the marginal and central bands is not blank, as in *N. Hennedyi*, but is irregularly dotted or stippled with round granules, precisely the same as those of the striæ. Towards the centre, and near the ends, the median striated bands pass gradually into the sparsely dotted space. Between these points the median bands end more abruptly. The scattered granules are consequently most thickly set round the nodule and near the apices. The granules are so large, that there are not more than five in each of the longest of the marginal striæ.

This conspicuous and beautiful species has been figured by Ehrenberg, as

occurring in the Clay Marl of Ægina, a bed belonging either to the Chalk formation or to the oldest Eocene strata. It seems to be very scarce there, for Ehrenberg has figured an imperfect specimen. I found it first rather sparingly in Professor Allman's Lamlash Bay dredging; and, since then, still more sparingly in Mr Miles's, from the same locality, as well as in several of my Loch Fine dredgings. It is obviously a member of the same group as *N. Hennedyi*, *N. nebulosa*, and others, with marginal and central striated bands. It is distinguished by its size, by the remarkable coarseness of its striation, and by the peculiarity that granules, such as form the striæ, are scattered over the unstriated space, without regularity. I have been informed that a form of *N. Hennedyi* occurs, with a similar character, but this I have not seen. I presume it will be easily known by its much finer striation, and its smaller size. Though this species is hitherto scarce, I have been able to examine a large number of examples, and also to supply various correspondents with specimens.

I avail myself of this opportunity to point out, that we have here an excellent example of the occurrence, in the recent state, in our seas, of a species hitherto known only as a fossil one. But as the Clay Marl of Ægina is the oldest deposit in which Diatoms have been detected with certainty, we have evidence that a species which is among the oldest of known Diatoms still exists. Nor is this by any means an unusual occurrence. In Ehrenberg's plate of the microscopic forms of this Eocene clay marl (Eocene at least, if not Cretaceous), he figures many other forms, all of marine origin; and all, or nearly all, of which are still living species. Indeed, I have seen upwards of three-fourths of these Diatoms in the dredgings described in this paper. Among these are *Actinocyclus undulatus*, *Coscinodiscus radiatus*, *Pyxidicula cruciata*, *Navicula prætexta*, *N. Bombus*, and many other frequent forms. I feel assured that every form of Diatom found in that Clay Marl, still lives in the present seas. And if this be the case with the oldest Diatomaceous deposit, it is no less likely to hold good of such as are of later date. In the great bed of Richmond, Virginia, which is marine, and said to be of the Miocene period, perhaps the most frequent form is *Orthosira marina*, Sm. (*olim Melosira sulcata*, Kütz.); a form which I find, as already mentioned, very abundant in Lamlash Bay. In the same deposit occurs *Coscinodiscus centralis*, Ehr., a splendid disc, to be described farther on, as occurring in the Clyde; and I might multiply similar examples almost *ad infinitum*.

Here the question naturally presents itself, Are there any extinct species of Diatoms? Strange as it may seem, when compared with what is found to occur in organisms of other and higher classes, I believe that this question ought to be answered in the negative.

In the earlier works of Ehrenberg, we frequently meet with species, and even with large groups of species, or almost genera, which are stated to be "fossil only," and which were believed to be extinct. Such forms are *Campylodiscus*

clypeus, found in the polishing slates of Bohemia, and the whole series of *dentate Eunotiæ*, found so abundantly in the Lapland Bergmehls.

But the progress of observation has shown that these forms are still in existence. *C. clypeus* has recently been found in British waters; and in America, and elsewhere, the *dentate Eunotiæ*, such as *E. Diadema*, *E. heptodon*, *E. octodon*, *E. Serra*, and others, have been found recent. I have myself often found, during the last two years, *E. triodon*, a form long regarded as extinct, in many of our streams, although scattered. But last summer I detected it as the predominant form in a gathering made by Professor Balfour, in a small stream on a hill in Arran, not far from Lamlash.

I conclude, therefore, that our knowledge of the existing species of Diatoms is yet far too limited to allow us to say that any fossil species no longer exists. In this very paper, I make known the actual existence of several species, hitherto supposed to be exclusively fossil, and every day adds to the number of existing forms, while it diminishes that of those conjectured to be extinct, few of which are now left. Surely, when one or two localities yield so many undescribed forms as I have here the honour to lay before the Society, we are not entitled to conclude that any form is extinct, because hitherto it has only been met with in the fossil state. In the present state of our knowledge, it is far more probable, that we shall ultimately find, as I have done in the case of *N. prætexta*, that the supposed extinct species are all still in existence.

But, it may be asked, How is it that you suppose no species of Diatoms to have become extinct, when, in almost every other class, the extinct species far outnumber the existing ones? In answer, I would observe, first, that we have no undoubted evidence of the existence of Diatoms earlier than the Clay Marl above named, which is either Eocene, or a member of the latest Chalk deposits. Now, if it be Eocene, then we know that that formation contains, even among fishes, a certain proportion of existing species. This proves that the condition of the Eocene period did not differ nearly so much from the present conditions as those of earlier deposits must have done; those, for example of the Carboniferous series, of the Old Red Sandstone, or of the Silurian strata.

Secondly, the size of Diatoms is so very minute, and their structure so exceedingly simple, that they must be little, if at all, affected, even by very considerable climatic variations. Of this, indeed, we have ample evidence, so far at least as concerns existing differences of climate. If we consult the plates of Ehrenberg's Microgeology, we shall see that the existing species of Diatoms found in the most distant and different parts of the world, in the Arctic and Antarctic Seas, in the tropical zone, and in our own temperate regions, are, for the most part, absolutely identical. There are, no doubt, local differences; but these, as is shown in this paper, may be very great in almost contiguous localities. On the other hand, having examined the diatoms in a large number of American and other exotic

soils, I have always found a very great majority of common British species. As an example, I may specify two soils particularly rich in Diatoms; one from the Sandwich Islands, the other from Lebanon. The former was quite like an ordinary fresh-water gathering, the latter resembled a poorer material. In both, I have great doubts whether all the numerous species are not identical with our own. Some few of the species, indeed, are not to be found in SMITH's Synopsis; but most of these have been described by others, or by myself, as British forms, since that work appeared.

So far, therefore, as the greatest actual differences of climate are concerned, Diatoms are apparently not affected; as in the cases just mentioned, it is impossible to distinguish the exotic specimens from British ones.

If, therefore, Diatoms did not exist earlier than the Eocene period, it is quite conceivable that none of them may have become extinct.

I have already stated that the Clay Marl of Ægina is supposed by some to belong to the formation next below the Eocene, that is, to the latest Cretaceous beds; but that there is no satisfactory evidence of Diatoms in any earlier formation. If we admit the Xanthidia to be Diatoms, these forms are known to occur in chalk flints. But the Xanthidia are not usually regarded as Diatoms, and I have not seen, either in flint or in chalk, any admitted or recognised Diatoms.

EHRENBERG figures many microscopic forms from the Chalk and older strata, some even from the Silurian Greensand. But these older forms, at least so far as are shown in the Microgeology, are not Diatoms, but either the siliceous Polycystineæ, or the calcareous Polythalamia, or, finally, sponge spicules.

I admit that Diatoms may have existed in the Chalk or earlier, and that, by a slow chemical change, they may have been destroyed, so that their form is lost, the siliceous material alone remaining, whether alone or in combination. We may even suppose that flint has been formed in part from the shells of Diatoms which lived along with the Foraminifera or Polythalamia of that period. But these are mere conjectures, and till Diatoms are found in the older strata, it must remain doubtful whether they existed previous to the Eocene period.

The Chalk or Marl of Meudon, near Paris, and that of Caltanisetta, in Sicily, exhibit a mixture of microscopic forms, calcareous and siliceous, including Diatoms. Here Diatomaceous shells, in contact with excess of calcareous matter, have remained unaltered; and if the Chalk of the true Cretaceous period had originally contained Diatoms, it seems probable that they would have been found as little altered as those of the newer beds just alluded to.

On the whole, then, it is probable that the continued existence of all, or nearly all, the known fossil species of Diatoms is the result, first, of their comparatively late introduction, and secondly, of their small susceptibility to climatic changes, arising from their minute size and very simple structure.

12. *Navicula Bombus*, Ehr. Pl. IX., fig. 12. Form much constricted in the

middle, the two halves broad and rounded, with subacute extremities. Median line broad; central nodule square, definite. Length about 0·0045″; greatest breadth 0·0018″. Striation coarse, strongly moniliform, not reaching the median line, but leaving a narrow blank space on each side of it. Striæ about 18 in 0·001, much inclined near the apices.

This form, which I had found frequent in the Glenshira sand, is also frequent in the new dredgings. It is regarded by many as a variety of *N. didyma*, and by others as a variety of *N. Crabro*. I am disposed to consider it a distinct species, on account of its peculiar and very constant form, and also because it has a decided light-brown colour in balsam, under a low power, which *N. didyma* has not. It is much larger than *N. didyma*. As to *N. Crabro*, the moniliform structure in it is always obscure, and the form is also different. I cannot perceive that *N. Bombus* passes into either of these species by intermediate forms. But whatever be the ultimate decision on this point, I give it here as the form called by Ehrenberg *N. Bombus;* which, be it species or variety, is at all events conspicuous, and very constant in its characters.

In my last paper on the Glenshira sand, I have figured several Naviculæ and Pinnulariæ of the panduriform group, and I have pointed out that this remarkable group requires thorough investigation. In order to contribute towards this end, I have figured the present form, as well as *N. Musca*, a new member of the same group; and I shall describe, farther on, another, namely a remarkable form of *Pinnularia Pandura*, Bréb.

13. *Navicula Lyra*, Ehr. Pl. IX., fig. 13 and 13 *b*. Form oblong-elliptic, broad; often contracted to short produced extremities. Length from 0·002″ to 0·0045″; breadth 0·0007″ to 0·0018″. Median line fine, interrupted by a large indefinite nodule, extending transversely. On each side of, and in contact with, the median line, is a linear, somewhat broad, striated band; and this is separated from the very broad, marginal, striated band by a narrow linear blank space. These linear blank spaces are, in each half, united by their base to the extremities of the nodular blank. They bend outwards from this point, then inwards, and finally again outwards at their extremities, thus forming, in the entire valve, two lyrate shapes united by their bases. Hence the name. The lyrate character is often much more decided than in the specimens figured. The extremities of these lyrate blanks generally reach the margin of the valve near its apices, but sometimes fall short of this, as in the figure. Striæ about 22 or 24 in 0·001″, somewhat inclined near the apices.

This species, which occurs in the Glenshira sand, and is scattered through all the dredgings here mentioned, has been described, though not as I have described it above, in the 2d volume of Professor Smith's Synopsis. Professor Smith seems to have seen only a variety, to be presently mentioned, which does not possess the lyrate character, and has therefore omitted that character. He refers to a figure

in vol. i. (fig. 152 *a**) given as *N. elliptica*, which is not lyrate; but the name given by EHRENBERG proves that he regarded the lyrate character as a principal one. I have therefore figured it, in order to show that it occurs in Britain as EHRENBERG described it.

14. *Navicula Lyra*, Ehr., var. *β*, *abrupta*. Pl. IX., figs. 14 and 14 *b*. Form usually oval, more or less elongated; sometimes linear in the middle, broad, with parallel sides, and obtusely acuminate at the extremities. I have hardly ever seen it with contracted and produced ends, as is so often observed in *N. Lyra*. Size and striation as in *N. Lyra*, but the blank spaces, which are linear, as in that species, instead of being recurved at the ends, or lyrate, bend inwards at the ends, so as to form two narrow ellipses meeting in the central nodule. These linear blanks in this variety stop abruptly at some distance from the terminal margin of the valve, which, in *N. Lyra*, they often, though not always, reach.

This form, which is frequent in the Glenshira sand, as well as in the dredgings, is that already referred to as having been figured in vol. i. of the Synopsis (fig. 152 *a**), as *N. elliptica*, and since referred to as *N. Lyra*, in vol. ii.

I figure it here, both that it may be compared with the *N. Lyra* of EHRENBERG, and that it may be contrasted with *N. spectabilis* (fig. 10), which is supposed by some to be a form of *N. Lyra*. The form of the latter, and the fact that the blank spaces in it are not linear, but broad, and reach the margin, all which characters are very constant; to which may be added the rich brown colour of *N. spectabilis* in balsam, under a low power, seem to me to be sufficient to distinguish it from *N. Lyra*. The reader is requested to compare fig. 10 with figs. 13, 13 *b*, 14, and 14 *b*. The latter forms are colourless in balsam.

15. *Navicula Smithii*, var. *β*, *fusca*. Pl. IX., fig. 15. Form an elongated oval, broad, with rounded ends. Length from 0·003″ to 0·0063″, and even more; greatest breadth from 0·0014″ to 0·0028″. Median line narrow at the terminal nodules, which are a little within the apices; broad, and formed of three parts, all ending in expansions, on each side of the central nodule. Nodule large, broad, indefinite. Striation very coarse, and coarsely moniliform, not reaching the median line, but leaving on each side of it a narrow blank line, terminating in the angles of the nodular blank. The whole spaces, taken together, form two very acute long triangles, base to base Striæ about 10 in 0·001″. At about one-third of the distance from the median blank lines to the margin, the striæ are traversed by a strong, dark line, which is often, as in the figure, nearly rhombic, but is generally curved, though very slightly. This line is caused by a ridge or elevation of the valve, and is very conspicuous. Valve thick, and highly convex, of a strong brown colour, in balsam, under the $\frac{1}{2}$ or $\frac{2}{3}$ of an inch objectives.

This form, which is very conspicuous, occurs, like the two preceding, both in the Glenshira sand and in the dredgings; and in that of Professor ALLMAN from Lamlash Bay and one from Loch Fine, it is frequent. I give it as a variety of *N.*

Smithii (olim *N. elliptica* Sm.), because I have always understood the typical *N. Smithii* to be a form which is very frequent in the Glenshira sand, and occurs also in the new materials. It is of a short, broad, inelegant, oval shape, flat, colourless, and much less coarsely striated. Neither does it exhibit the longitudinal ridge so distinctly. It may be, that the present form, *N. fusca*, is the typical one, and the other a variety of it; but in my experience I have only seen *N. fusca* in the gatherings above named, while I observe *N. Smithii* in every marine gathering.

16. *Navicula Smithii*, var. γ, *nitescens*. Pl. I., fig. 16. Form lanceolate, tending to rhombic, with obtuse ends. Median line straight, nodule definite. Length from 0·002″ to 0·0035″; breadth from 0·0009″ to 0·0014″. Striæ about 16 in 0·001″, considerably inclined, obscurely moniliform, and of a shining aspect. They are traversed by a ridge, which is about half-way from the margin to the median line, and has an outline more rhombic than that of the valve.

This form occurs both in Lamlash Bay and in Loch Fine, and is not at all rare in some of the dredgings. It is conspicuous, from its elegant form and shining aspect. It is quite colourless under a low power. I have given it as a variety of *N. Smithii*, from a desire to avoid unnecessary multiplication of species. But I am inclined to regard it as distinct from that species, from its peculiar form, its smaller size, the character of the nodule and median line, and its bright white aspect; all of which characters are very constant.

17. *Navicula Smithii*, var. δ, *suborbicularis*. Pl. IX., fig. 17. Form a short, broad oval, or suborbicular. Length 0·002″ to 0·0026″; breadth 0·0013″ to 0·0018″. Median line bounded by white lines, curving inwards both to the apices and to the indefinite nodule. Striation conspicuous, much inclined. Striæ 16 or 18 in 0·001″, moniliform. There is a ridge, as in the two preceding forms, traversing the striæ, and when the striæ near the margin are in focus, those between the ridge and the median line are very faint.

This form occurs in Lamlash Bay, and is also tolerably frequent in one Loch Fine gathering, in which the preceding form is not found. Its small size, nearly round form, and peculiar median line, with the slightly-marked ridge, compared to that in the two preceding forms, seem to point it out as distinct; especially as it is very constant in its characters. But, for the reasons already stated, I give it as a variety. It is at least a form to be noticed, and to be considered with the others with a view to a more accurate determination of species than has yet been possible, but which, in the progress of observation, we may hope to attain.

18. *Navicula maxima*, Greg. Pl. IX., figs. 18, and 18 *b*. Form of S.V. linear, rather narrow, with obtuse ends. Length from 0·002″ to 0·008″; breadth of S.V. from 00·0025″ to 0·00011″; of F.V. in the larger specimens, 0·0009″ in the middle, 0·00115″ at the ends. Striæ fine, but distinct, about 52 in 0·091″, parallel, not quite reaching the median line, from which, at the centre, they retire, leaving a pretty large round space. F.V. rectangular but narrowest at the middle, and

slightly expanded at the ends, the angles being bevelled. From the opposite ends the margin inclines very slightly, but visibly, to the middle. Nodules very conspicuous on the F.V., in which the striation also extends, on each side, to rather more than $\frac{1}{4}$th of the width of the frustule, which arises from the convexity of the S.V.

I first described this species in my first paper on the Glenshira sand, in which the figure was not characteristic. I figured it again in the second paper; giving, however, a shorter, broader, and constricted form as the type, and the present one as a variety. I have since found it frequent in all the dredgings, but especially abundant in one from Loch Fine, and am now satisfied that the linear form is typical and the broad constricted form a variety. I give the peculiar and characteristic F.V. for the first time. The S.V. in fig. 18 *b*, is that of a broad individual of the linear type. It is generally narrower, and often even no more than half this width. The shortest specimens are often still narrower. The broad, incurved form, at first regarded as the type, is very scarce in the dredgings, compared to the linear form.

This form has been supposed to be identical with *N. firma*, Kütz, var. β; but its marine habitat at once negatives this supposition; and, besides, its aspect and colour are quite different. *N. firma* is brown, while *N. maxima* is of a pale straw-colour. The striation in *N. firma* is coarser and more conspicuous; and, lastly, *N. firma* is broader, has acute extremities, and yields several marked varieties, such as Ehrenberg's *N. dilatata* and *N. Amphigomphus*; while the only observable variety of *N. maxima* is the shorter, broader, incurved one, represented in fig. 2 of my second paper on the Glenshira sand.

19. *Pinnularia* (?) *subtilis*, n. sp. Pl. IX., fig. 19. Form linear rhombic, very narrow, with elongated apices. Length about 0·0035″; greatest breadth about 0·00025″. Nodule definite. Costæ about 28 or 30 in 0·001″; transverse, slightly inclined towards the apices.

This form occurs in Lamlash Bay. I do not feel quite sure about its genus. It may be a Navicula. The whole form is delicate and translucent, and it is far from conspicuous.

20. *Pinnularia rostellata*, n. sp. Pl. IX., fig. 20. Form linear, broad, with acuminate ends, terminating in short, acute apiculi. Length from 0·002″ to 0·0027″; breadth about 0·0007″. Central nodule definite. Costæ strong, subdistant, inclined near the ends, reaching the median line, about 14 in 0·001″.

This pretty form occurs both in Lamlash Bay and in Loch Fine. It is not frequent, but I have been able to examine a considerable number of specimens, which are quite constant in their character.

21. *Pinnularia Allmaniana*. Pl. IX., fig. 21. Form elliptic-lanceolate, broad, extremities subacute. Valve highly convex on one side, concave on the other. Length from 0·0016″ to 0·0026″; breadth from 0·001″ to 0·0014″. Costæ appa-

rently marginal, strong, about 20 in 0·001″, giving the appearance of a narrow marginal band of very strong costæ. Within this band, however, the valve, on close inspection, is found to be marked with similar but much fainter costæ nearly to the median line. The valve appears to be thicker near the margin than in the middle, and this perhaps is the reason why the costæ are so strong and conspicuous there.

This form is frequent in Professor Allman's dredging from Lamlash Bay, and it occurs also in Loch Fine. I have named it after Professor Allman, to whom I am indebted for this dredging, the richest of all those here described.

22. *Pinnularia Pandura*=*Navicula Pandura*, Bréb., var. *β*, *elongata*. Pl. IX., fig. 22. Form deeply constricted in the middle, with elongated sub-triangular ends, and obtuse apices. Length 0·0075″ or more; greatest breadth 0·002″, breadth at constriction 0·0014″. Median line sharply defined, broader at the centre than at the ends; nodule square, definite. On each side of the median line, and a little way from it, there is on each side a line or ridge, apparently formed of large granules, but probably only apparently so, from the sudden and sharp elevation of the ends of the costæ. Costæ, from this point to the margin, perfectly entire and glassy, like those of *P. alpina*. Valve thick, costæ 10 or 11 in 0·001″, somewhat inclined near the apices.

This seems to be a variety of De Brebisson's *Navicula Pandura*, which I have represented in the second paper on the Glenshira sand, in figs. 11, 12, and 12*. But as De Brebisson himself describes the costæ as being entire, and represents them distinctly so in his figure of the species, I have changed the generic name to *Pinnularia*. I consider it as quite distinct from *N. Crabro*, Ehr., as described by Professor Smith, in vol. ii., of the Synopsis; for the latter has obscurely moniliform striæ, as is shown in Dr Greville's figure of it from Trinidad, in the Microscopical Journal for January 1857.

The forms represented in figs. 11, 12, and 12* of my second paper on Glenshira (*Micr. Trans.* iv., pl. v.), and that here figured (fig. 22), are abundant in several of the dredgings; but of all the numerous examples I have examined, not one exhibits the slightest trace of moniliform structure; and I have had the satisfaction of having this observation confirmed by Dr Greville, who is acquainted with the form in which that structure exists obscurely. Unless, therefore, we are prepared to abolish the distinction between entire and moniliform striation, on which Professor Smith founds the distinction between his genera *Navicula* and *Pinnularia*, we cannot regard this form as a *Navicula*.

GROUP II.

Cocconeides.

The new forms belonging to this group are not numerous, but they are, in every case, interesting. In addition to one species, already figured, though imperfectly, as occurring in the Glenshira sand, I have detected in the new materials six additional species, all of them beautiful and well-marked forms. These constitute a largeaddition to a genus, which, in Britain at least, has hitherto been a very small one. The species to be described are:—

23. Cocconeis distans, Greg.
24. ... ornata, n. sp.
25. ... dirupta, n. sp. G.
26. ... nitida, n. sp.
27. Cocconeis pseudomarginata, n. sp.
28. ... major, n. sp.
29. ... splendida, n. sp.

23. *Cocconeis distans*, Greg. Pl. IX., fig. 23. Form oval, broad; ends subacute. Length from 0·0014″ to 0·0026″; breadth from 0·001″ to 0·002″. Median line delicate. The valve is marked by distant lines, much inclined near the apices, not reaching the median line. These lines are about 10 in 0·001″, and consist of white hyaline faint bars, on which are set small and distant granules. The number of granules is only 4 or 5 in the longest of these lines, so that the granules are very distant. They are, as nearly as possible, of equal size, and from their distance, give to the valve a spotted rather than a striated aspect. In the figure, the granules appear larger than they really are; but this, as I have since ascertained, depends on the focus, and is an effect of shadow. By careful focussing, the real size of the granules is easily seen. The valve is hyaline; but it is always easy, by focussing, to see the faint bars on which the granules are set, a character which at once distinguishes this form from *C. Scutellum*, to which some are disposed to refer it. Another character, besides that of its having much fewer lines and much fewer granules than the coarsest varieties of *C. Scutellum*, is, that in *C. distans* the granules are of equal size, while in *C. Scutellum* they diminish in size as they approach the median line. In *C. distans*, if there be any difference, it is that the marginal granules are somewhat smaller than the others. I may here also allude to the fact, that while *C. Scutellum* is a most variable form, *C. distans*, so far as I have seen, exhibits only one variety, and that quite different from any variety of *C. Scutellum*. This form, *C. distans*, var. *β*, *præmorsa*, is considerably larger than the type, having a length of 0·003″ to 0·0038″, and being a little narrower in proportion than the type. There is always, at one point of the margin, a notch or solution of continuity, as if a portion had been cut out, and then smoothed over. The only other difference is, that the granules are somewhat smaller, but the faint bars are exactly as in the type.

I figured this species, both in my first and in my third paper on the Glenshira

sand. But in the former, a variety of *C. Scutellum* was figured by mistake: and the latter figure was imperfect, because I had not then seen the faint white bars. This species, along with *C. costata*, also figured in the third paper alluded to, is so frequent in Lamlash Bay, that I have had ample means of studying it, and am quite satisfied of its being a good species. I may say the same of *C. costata*, with the remark, that I cannot ascertain from EHRENBERG's figures, whether his *Raphoneis fasciolata* may not be the same form. EHRENBERG's form seems to be much larger, and the markings much coarser and more conspicuous. I must leave this point undecided till I can compare the two forms.

24. *Cocconeis ornata*, n. sp. Pl. IX., fig. 24. Form a pure and elegant oval. Length about 0·0022″; breadth about 0·0014″. There is a broad marginal band, marked by strong distant costæ, the ends of which are rounded. Within this band the surface appears concave to the median line, which is delicate, with a large, definite, central nodule. The middle part is marked by fainter costæ, corresponding to those on the marginal band, and, like them, so much inclined near the apices, as to be nearly vertical. There is a narrow blank line between the marginal and central costæ, and the latter do not reach the median line, leaving a long lanceolate blank space in the middle. The whole valve has a rich ornate aspect.

This beautiful species occurs in Lamlash Bay; and although scarce as yet, I have been able to examine a sufficient number of specimens to ascertain its characters. I have also observed a few in Loch Fine.

25. *Cocconeis dirupta*, n. sp. Pl. IX., fig. 25. Form a broad, short oval, sometimes all but orbicular. Length from 0·001″ to 0·0024″; breadth from 0·0007″ to 0·0021″. Valve thick, and under the half-inch objective of a strong brown colour. Median line irregular, like a slit or tear down the middle of the external surface. The whole valve is marked, except the slit, with coarse, wavy, longitudinal striæ; but, when carefully focussed, fine transverse striæ are seen over the whole surface to the median line. Under the half-inch, there is an appearance of a long stauros, which, under a higher power, disappears as such, and can only be seen as a transverse gleam of light from below. The striated surface seems to be an outer one, torn asunder in the middle, and from this I have named it. Vertical striæ about 26, transverse striæ about 60 in 0·001″.

I had observed this form in the Glenshira sand, where, however, it was very scarce, and hardly ever entire, so that I postponed its investigation. It occurs very abundantly in Mr MILES's Corallina gathering, and less frequently in several of the dredgings. There is but one known form which is in any degree allied to it. This is *C. diaphana*, Sm., which I find to occur along with it. After many comparisons, I am disposed to conclude, although these two forms are not the same thing, since *C. dirupta* is by no means diaphanous, while its striæ are conspicuous and its colour brown, the striæ of *C. diaphana* being very obscure, and the valve colourless, that *C. diaphana* may perhaps be an imperfect form of

C. dirupta, possibly the surface which lies under the one here figured, or possibly also the lower valve, which in *Cocconeis* is often different from the upper. It is, however, at least equally probable that these two forms belong to different species. In the Corallina gathering, *C. dirupta* is infinitely more frequent than *C. diaphana*.

26. *Cocconeis nitida*, n. sp. Pl. IX., fig. 26. Form a very broad oval, suddenly contracted, above and below, to very short, subacute, produced apices. Length from 0·001″ to 0·0038″; breadth from 0·0008″ to 0·0035″. Valve very thick, aspect glassy. It is marked by lines of very large nitid granules, these lines forming, longitudinally, concentric striæ, the two inner ones of which bend slightly outwards from the median line, leaving a narrow lanceolate blank space; the others becoming more and more curved as they approach the margin. In large specimens, there are five such lines on each side, or from 3 to 4 in 0·001″. But the granules also form transverse lines, much inclined near the apices, of which there are, in large specimens, 28 or 30 in the length of the valve, or from 6 to 8 in 0·001″. The margin is marked by a series of finer striæ. Median line obscure. In the middle of the valve, the transverse lines contain on each side five granules, corresponding to the five vertical lines. The granules are generally of equal size or nearly so, except a few near the apices, which are smaller. The whole form is very conspicuous, from its glassy aspect, and the size and brilliancy of the granules.

This striking form occurs not unfrequently in Lamlash Bay, and sparingly in the Loch Fine dredgings. It is very uniform in its characters.

27. *Cocconeis pseudomarginata*, n. sp. Pl. IX., fig. 27. Form a very broad elliptic, or elliptic-lanceolate. Length 0·0016″ to 0·0033″. Breadth 0·0011″ to 0·003″. Valve thin and transparent. Within the margin is a line or shade parallel to it; and within this, half-way from the margin to the centre, is a very strong line, forming a broad lanceolate figure. At first sight it seems as if the latter were the inner boundary of a marginal striated band; but on close inspection the striæ, which are very fine, are seen to extend from the margin almost to the median line, where they leave a very narrow rhombic blank space, extending, in the median line, only to the inner of the two marginal lines. The third, or interior line, counting from the margin, is a very strong and raised ridge, the ends of which are almost in contact with the second line. Median line delicate, central nodule definite; terminal nodules placed within the ends of the third line. Striæ very delicate, but sharp, about 62 in 0·001″, transverse in the middle, nearly vertical at the ends. The first or outer margin is formed of two lines very close to each other.

This remarkable form occurs in Professor Allman's dredging from Lamlash Bay, where it is rather scarce, and also in that of Mr Miles from the same locality. It requires a very good glass to resolve the markings perfectly.

28. *Cocconeis major*, n. sp. Pl. IX., fig. 28. Form a very broad oval. Length to 0·0015″ to 0·0038″; breadth from 0·001″ to 0·00315″. Median line distinct;

central nodule indefinite. Terminal nodules considerably within the margin, small. The two parts of the median line terminate in the middle in small rounded expansions, but do not meet. Striæ delicate, but sharp, transverse in the middle, and gradually more and more curved towards the apices, where they become nearly vertical. They are not so close together as in some forms in which they are equally delicate, and there are about 54 in 0·001″. Valve thin, flat, hyaline.

This remarkable form occurs in Professor Allman's Lamlash Bay dredging, where, however, it is rather scarce; also in that of Mr Miles.

29. *Cocconeis splendida*, n. sp. Pl. IX., fig. 29. Form, a pure broad oval. Length about 0·0044″; breadth about 0·0039″. Valve strong, and very richly marked with striæ, which are highly inclined and curved near the apices. These striæ are coarse, about 14 in 0·001″, and are formed of granules, which gradually diminish in size towards the median line. The four or five outer granules of each of the striæ are set as closely together as possible, while the rest are separate. This gives the appearance of a broad marginal band. There is a small, nearly square blank at the centre, which is no doubt the indefinite central nodule. The two halves of the median line are strong, somewhat bent at the terminal ends, where they form elongated expansions, lying just within the dense marginal band. The central ends terminate in small expansions, which lie at the upper and under edges of the central blank.

This beautiful form occurs in Lamlash Bay, but it is hitherto scarce. No doubt it will some day be found more abundantly. It is, with the two preceding forms, remarkable for the size it attains, being the largest Cocconeis yet described, while *C. major* and *C. pseudomarginata* are but little below it in this respect, and even *C. nitida* is unusually large for this genus.

GROUP III.

Filamentous Forms.

Of this class of forms, the number is considerable. It is worthy of remark, that most of them belong, so far as I am able to judge, to the genus Denticula, which hitherto has yielded only fresh-water species. But there are six of which the genus is doubtful; partly because the F.V. alone is as yet known, which is the case in four of them; partly because, if not Denticulæ, they cannot well be referred to any of the genera in Smith's Synopsis. One remarkable species, the genus of which is still uncertain, I have been compelled to remove from Himantidium, in which genus Professor Smith had provisionally arranged it. The forms of this group are 14; viz.:—

30. Denticula (?) interrupta, n. sp.
31. ... (?) capitata, n. sp.
32. ... (?) ornata, n. sp.
33. ... (?) lævis, n. sp.
34. Denticula nana, n. sp.
35. ... minor, n. sp.
36. ... distans, n. sp.
37. ... staurophora, n. sp.

38. Denticula fulva, n. sp.
39. ... marina, n. sp.
40. Diadesmis (?) Williamsoni = Himantidium Williamsoni, Sm.
41. Meridion (?) marinum, n. sp. (or Gomphonema lineare ?).
42. Pyxidicula (Dictyopyxis) cruciata, Ehr.
43. Orthosira angulata, n. sp.

30. *Denticula* (?) *interrupta*. Pl. X., fig. 30. Form of F.V. nearly rectangular, the middle part very slightly convex, and the ends a little expanded. There is an apparent interruption of the margin at the middle point on each side, and on each side of this opening is a round punctum. Length about 0·0015″; breadth about 0·0004″. It occurs in chains of 2, and the two dots of each margin form a square with those of the margin of the adjacent frustule. The S.V. is not yet known, and the species is only provisionally referred to Denticula.

This species occurs in Lamlash Bay, but is scarce.

31. *Denticula* (?) *capitata*, n. sp. Pl. X., fig. 31. Form of F.V. generally rectangular, but with the middle part considerably convex, and the apices expanded and rounded. Length about 0·0018″; breadth about 0·0005″. Occurs, like the last species, in chains of two. S.V. as yet unknown; and it is only doubtfully and provisionally referred to Denticula.

This species occurs only in one of the Loch Fine dredgings, and it is as scarce as the preceding.

32. *Denticula* (?) *ornata*, n. sp. Pl. X., fig. 32. Form of F.V. rectangular, but somewhat expanded in the middle, and also, after a slight contraction, at the apices, which are finally truncate. The margin, at the expansions, is beautifully moulded, having a moulding or notch on each side of the middle point.—Occurs in chains of two. Length 0·0015″ to 0·0017″; breadth, in the middle, 0·0005″. S.V. not yet known, so that the species is only provisionally referred to Denticula.

This very pretty species occurs both in Lamlash Bay and in Loch Fine, and is much less scarce than the two preceding ones. Notwithstanding this, however, I have not been able to find the S.V.

33. *Denticula* (?) *lævis*, n. sp. Pl. X., Figs. 33, 33 *b*, and 33 *c*. Form of F.V. linear, rectangular, with a small sharp prominence in the middle of each margin, and the apices slightly, but sharply, expanded. Occurs in chains of two or three, and also solitary. Length from 0·0016″ to 0·0027″; breadth 0·0006″. It is striated on each side to one-third of the width, indicating that the S.V. is convex. Striædelicate, but distinct under a high power, about 48 in 0·001″. The general aspect is smooth, and the striation is only seen on very careful adjustment. There are two terminal nodules visible on the F.V. at each end, which are joined by lines bounding the striæ. The middle prominence of the margin of the F.V. seems to indicate a central nodule on the S.V., which view has not yet been observed.

This species is by no means rare in Lamlash Bay, and it occurs also in Loch Fine; but I have hitherto been unable to detect the S.V. I have referred it provisionally to Denticula, but with many doubts. It is probable that if we had the

S.V. we might find it to be a Diadesmis, that is, a catenated species, having naviculoid frustules. But I do not venture to name it on conjecture, and I only refer it, with the three preceding forms, to Denticula provisionally, in order that some name may be used in speaking of them. Indeed it is probable that the three preceding species may also prove to belong to Diadesmis. My present object is, not to determine their genus, for which I do not possess the necessary data, but only to point them out as well-marked species, for the researches of other naturalists.

34. *Denticula nana*, n. sp. Pl. X., fig. 34. Form of the F.V., which occurs in chains of two, three, four, and occasionally more, rectangular, expanding a little in the middle, and also at the apices, which are truncate. Length from 0·0005″ to 0·001″; breadth, in the shorter examples, 0·0003″ to 0·0004″, and less in the longer ones. Margin of F.V. faintly denticulate, from the ends of the striæ. S.V. obtusely rhombic, broad, with a raphe in the median line. Striæ rather fine, inclined.

This little form is tolerably frequent, both in Lamlash Bay and in Loch Fine. I think it is properly referred to *Denticula*, although it has some resemblance to some of the forms figured by foreign authors under the name of *Zygoceros*.

35. *Denticula minor*, n. sp. Pl. X., figs. 35, 35 *b*, 35 *c*, and 35 *d*. Form of F.V., which occurs in chains of from two to seven or eight, on the whole rectangular; sometimes exactly so, more frequently with an angular expansion at the apices, which become capitate and subtruncate, while the margin is convex in the middle. Length from 0·0005″ to 0·002″; breadth from 0·0002″ to 0·0006″. Margins of F.V. strongly denticulate. S.V. rhombic or rhombic-lanceolate, very narrow, with srong marginal costæ. Costæ 18 or 20 in 0·001″.

This form, which is very frequent in the Lamlash Bay dredgings, and also in one of those from Loch Fine, varies much both in size and shape, the F.V. being sometimes as short as the shortest *D. nana*, and very broad in proportion, bulging in the middle, and capitate, sometimes longer, rectangular, and broad; and most frequently longer, much narrower, and capitate. The S.V. is so narrow, that the frustule seldom lies on that side, so as to present it to the eye. It appears to belong distinctly to Denticula.

36. *Denticula distans*, n. sp. Pl. X., figs. 36 and 36 *b*. Form of F.V., which occurs in chains of from two to five or six, and also solitary, rectangular, rather broad: often convex on the sides, and with the ends a little expanded. Margin strongly denticulate. Length 0·0017″ to 0·0026″; breadth 0·0006″ to 0·0008″. S.V. rhombic or rhombic-lanceolate, broad; marked with very strong, distant, sharp, and marginal costæ. Terminal nodules large and conspicuous. No central nodule. Costæ about 10 in 0·001″. Valve thick and glassy.

This fine species is tolerably frequent, both in Lamlash Bay and in Loch Fine. There is a considerable resemblance between this and the preceding species, so

that *D. minor* almost looks like a miniature of *D. distans;* but on a close comparison, they are found to be totally distinct. *D. distans* often occurs shorter than the average length of *D. minor*, but it never loses its own characters, the strong, distant, glassy costæ, and the broad S.V. But the two forms are evidently allied species, and both seem to be true *Denticulæ*.

37. *Denticula staurophora*, n. sp. Pl. X., figs. 37, 37 *b*, and 37 *c*. Form of F.V., which occurs in chains of two, three, and sometimes more, rectangular, with coarse marginal striæ, which in the middle on each side, are interrupted by a blank space, bounded by diverging lines. Length from 0·001″ to 0·0038″; breadth 0·0005″ to 0·0008″, the shorter examples being the broadest. S.V. lanceolate, rather narrow, marked with coarse moniliform striæ, except in the middle, where there is a broad stauros, on each side of which is a line, curved and concave towards the extremities. Striæ 14 to 16 in 0·001″.

This striking form is not unfrequent either in Lamlash Bay or in Loch Fine. I have referred it to Denticula, but perhaps it ought to be referred to *Diadesmis*, or, if the stauros be considered an objection, to a new genus allied to *Diadesmis* as *Stauroneis* is to Navicula. But this point must be left for farther investigation.

38. *Denticula fulva*, n. sp. Pl. X., figs. 38 and 38 *b*. Form of F.V., which occurs in chains of two, three, and sometimes four, linear, rectangular, and slightly expanded at the apices, the margin marked with the ends of somewhat coarse striæ. Length from 0·0018″ to 0·004″; breadth 0·0005″. S.V. linear, narrow, broadest in the middle, and gradually contracting to long, narrow extremities, which are ultimately subcapitate and rounded. Striæ moniliform, somewhat coarse, leaving a raphe in the middle, and the two terminal knobs unstriated. No central nodule in the S.V.; but the two nodules seen at each end of the F.V. appear to form the unstriated knobs at each end of the S.V.

This well-marked species occurs with the three last, and is even more frequent than they are. The genus cannot be considered as determined with certainty.

39. *Denticula marina*, n. sp. Pl. X., figs. 39 and 39 *b*. Form of F.V. linear, rectangular, with the angles very slightly expanded, and the margin strongly denticulate. It occurs in chains of from 2 to 18 or 20, so that the filament seems to be tenacious. Length from 0·002″, to 0·008″ or 0·009″; breadth from 0·0003″ to 0·0005″. S.V. linear, expanded at the middle, and obtusely acuminate at the ends. Striæ very coarse, and very coarsely moniliform, about 10 in 0·001″. On each side of the median line each of the striæ is formed of only two granules, which are distant, and half of a third, which seems to be on the margin. The two or three central striæ on each side, having only one and a half granules, the inner granule of each being absent, there is a blank space round the centre; and there is a smaller blank at each apex. The F.V. here figured is of about the usual

length, the S.V. is one of the longest. The whole form has a pale, whitish aspect on the S.V., and the single valves are hyaline.

This very fine and conspicuous form is frequent in Lamlash Bay, and even abundant in one of the Loch Fine dredgings. It occurs scattered in all the dredgings without exception. It seems to be a true *Denticula*.

40. *Diadesmis* (?) *Williamsoni*, n. sp. Pl. X., figs. 40 and 40 *b*. Form of F.V. rectangular, narrow, with three angular expansions, one in the middle, and one at each end; so that there are two long, narrow elliptical spaces between the adjacent frustules, which occur, as in the last species, in chains of from two to twenty, and upwards. Margin of F.V. strongly denticulate. Length from 0·0016″ to 0·008″; breadth from 0·0004″ to 0·0005″. S.V. linear, narrow, straight, very slightly incurved in the middle, and acuminate at the ends. Striæ coarse, coarsely moniliform, but closely set, giving to the valve a black aspect; about 16 or 18 in 0·001″. Central and terminal nodules large, white, conspicuous.

This remarkable and conspicuous form occurs with the last, and is even more abundant, especially in two of the Loch Fine dredgings. It is found in all the materials. The F.V. has been described and figured by Professor Smith, as having occurred sparingly in a dredging made by Mr G. Barlee, off the coast of Skye, and detected by Professor Williamson. It was referred by Professor Smith, but doubtfully, from the absence of the S.V., to *Himantidium*. The S.V. is particularly frequent in one of my Loch Fine dredgings, and certainly cannot belong to *Himantidium*. The nearest genus is *Diadesmis;* but I do not feel at all sure that it is the true one. The conspicuous nodules agree with it; but the aspect of the species is unlike that of any known *Diadesmis*. I therefore give it as such with a doubt, and am satisfied with having, in the meantime, ascertained that it is not a *Himantidium*. I would indicate one curious character, that the striæ, on the F.V. seem to pass across the intervals between the adjacent frustules. This seems also to have been observed by Professor Smith, as it is represented in his figures of the F.V., which are accurate. I have also observed, that the shortest examples, which are sometimes so short as to be nearly square on the F.V. are not only broader on both views than the larger ones, but also devoid of the central expansion or undulation, seen on the F.V. In this state, they approach in form to the rectangular F.V. of *D. distans*, but are at once recognised by the closeness of the striæ, as well as by their moniliform character. In one dredging I find many long filaments, especially of the shorter frustules; while in another, treated precisely in the same way, the detached frustules, generally exhibiting the S.V., and much longer, are much more common than the chains.

41. *Meridion* (?) *marinum* (or *Gomphonema lineare* (?)), n. sp. Pl. X., figs. 41 and 41 *b*. Form of F.V., which occurs in chains of two, three, or four, both in entire apposition, or attached only by an angle at the broader end, cuneate and truncate at both ends, and narrow. It is marked with coarse marginal denticu-

lations. S.V. linear, narrow, broadest at a point above the middle, from which it becomes narrower both ways; the shorter half being rather broader than the other, and rounded at the apex. The longer and narrower half is also rounded, and very slightly expanded at the end. Striæ coarse, not reaching the median line, but leaving a somewhat broad raphe in the middle. Striæ about 16 in 0·001″; Length about 0·0015″; breadth of F.V., at larger end, 0·0004″, at smaller end. 0·0003″. Breadth of S.V. 0·0002″ at the broadest point.

This form resembles both a *Meridion* and a *Gomphonema*. The absence of a central nodule prevents me from referring it to the latter genus; and with reference to the former, the mode of attachment, as well as the form of the frustules, agree pretty well with it. But I have not seen more than four attached; so that it is still doubtful whether it forms a spiral filament, as the slightly cuneate frustules must tend to do. It occurs, by no means sparingly, both in Lamlash Bay and in Loch Fine. Future observations on the living form will decide the question of its generic position, but in the meantime it is a well-marked species.

42. *Pyxidicula cruciata*, Ehr. Pl. X., fig. 42. Form of V. cup-shaped, hemispherical, with hexagonal cells over the whole surface, and in one direction, a crest composed of square or irregular cells running round the hemisphere in a plane at right angles to that of the junction of the two valves. Cells large, and of uniform size. Diameter 0·0019″.

The form here figured is only a detached valve, the entire frustule not having yet occurred in these dredgings. But it agrees precisely with Ehrenberg's figure of the valve. The specific name is given on account of the arrangement of some of the cells, as seen in a view obtained by looking down on the frustule, at right angles to the junction, in the form of a broad rectangular cross. This cannot be seen in the view here figured. The species is very scarce as yet, having occurred very sparingly in Lamlash Bay. But it is interesting, as being one of the forms which Ehrenberg has figured from the Ægina Clay Marl already mentioned at p. 482, and from the deposit of Richmond, Virginia. I have placed it in this group, because I have reason to think that Pyxidicula is a catenate form, though we cannot expect to see this in fossil deposits. I would here refer to the beautiful new form, detected by Professor Walker Arnott, and figured in the Appendix to this paper, which, in the frustules, is so closely allied to the present species, that it may prove to be actually *P. appendiculate* of Ehrenberg, a form which occurs along with *P. cruciata*. Professor W. Arnott's form is decidedly catenate.

43. *Orthosira angulata*, n. sp. Pl. X., figs. 43 and 43 *b*. Form of F.V., which occurs solitary, and in chains of from two to six, rectangular in the middle, acuminate and truncate at the ends. On the margin, which is often slightly incurved, are seen denticulations arising from the cells of the disc or S.V. Length from 0·0005″ to 0·0015″; breadth of F.V. from 0·0003″ to 0·00045″. Diameter of disc,

0·0005″ to 0·0015″. Disc cellulate; cells largest in the centre, becoming regularly smaller towards the margin, arranged in quincunx, but, from the diminishing size of the cells, in curve lines. It often happens that the cells in the two inner thirds of the disc are conspicuously seen, inclosed within an equilateral triangle, formed by three slightly curved lines of cells, beyond which the cells and lines of cells are so small and hyaline, as to become obscure. Such discs are convex, and seen on the convex side. Others, especially the largest, are much flatter, and, when properly focussed, exhibit the whole cellular structure plainly.

This species is very frequent, even abundant, in Professor ALLMAN'S dredging from Lamlash Bay; and it is found more or less frequently in all the others. Possibly the disc is identical with the *Coscinodiscus minor* of EHRENBERG or KÜTZING; that of Professor SMITH being a fresh-water form. But the present species is unquestionably an *Orthosira*, notwithstanding the resemblance of the disc to *Coscinodiscus*.

GROUP IV.

DISCS, INCLUDING CAMPYLODISCI.

The new forms belonging to this group are not, in the materials examined, very numerous, but they are very interesting, and most of them are very fine species.

44.	Melosira or Coscinodiscus (?) n. sp.		50.	Eupodiscus subtilis, Ralfs, n. sp.	
45.	Coscinodiscus nitidus, n. sp., G.		51.	Campylodiscus centralis, n. sp.	
46.	...	punctulatus, n. sp., G.	52.	...	Ralfsii (?) Sm.
47.	...	concavus, Ehr., G.	53.	...	angularis, n. sp.
48.	...	umbonatus, n. sp.	54.	...	eximius, n. sp.
49.	...	centralis, Ehr., G.	55.	...	limbatus, Bréb.

44. *Melosira* or *Coscinodiscus* (?) (?), n. sp. Pl. X., fig. 44. Diameter of disc 0·003″. It is marked by fine, sharp, radiate lines, which are very numerous. These lines are strongest near the margin, which has the form of a broad, thick, raised rim, within which the valve seems to sink and then to rise, the middle part being apparently somewhat convex.

This disc occurs not very unfrequently in one of the Loch Fine dredgings, and more sparingly in one from Lamlash Bay. As no view of it is to be seen except the disc, or S.V., I have not been able to determine its genus, although the disc looks more like that of a filamentous form than a Coscinodiscus.

45. *Coscinodiscus nitidus*, n. sp. Pl. X., fig. 45. Diameter of disc 0·0012″ to 0·0025″. Margin entire, transversely striated. Striæ of margin about 16 in 0·001″, traceable to some small distance beyond the marginal band towards the centre. Surface of disc marked with distant and irregularly radiate lines of rather large, round, distant cells or granules. The rays are distinctly marked to-

wards the margin, but somewhat confused towards the centre. Puncta or granules larger towards the centre than at the margin. Aspect of valve glassy, puncta nitescent, very much as in *Cocconeis nitida* (fig. 26).

This pretty disc was figured without a name, from an imperfect specimen, in my last paper on the Glenshira Sand (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 50). Having found it tolerably frequent in Lamlash Bay, I now figure a perfect example, which, provisionally, I refer to Coscinodiscus.

46. *Coscinodiscus punctulatus*, n. sp. Pl. X., fig. 46. Diameter of disc 0·003″ to 0·0036″. It is marked by very fine and obscure lines, which, near the margin, are traceable as rays, but which soon become fainter, and apparently wavy at the same time, as they proceed towards the centre. Over the whole surface are scattered, sparsely, small puncta, which, in a certain focus, appear as points of light.

This disc was also figured, but not named, in my last paper on the Glenshira Sand (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 48); but the specimen here figured is a better one, and shows more of the very obscure structure. It is impossible, in the present state of our knowledge of it, to refer it with certainty to any genus; but it may be a Coscinodiscus. It occurs in Lamlash Bay and in Loch Fine, but is not very frequent. It may possibly prove to be the end view, or the dissepiment, of a Melosira or an Orthosira.

47. *Coscinodiscus concavus*, Ehr. Pl. X., fig. 47. Diameter of disc 0·0025″ to 0·0043″. Margin entire, transversely striated. Striæ about 10 in 0·001″. Surface concave, covered with large, equal, hexagonal cells, arranged as in a honeycomb.

This beautiful disc occurs rather sparingly both in Lamlash Bay and in Loch Fine. It agrees exactly with one of Ehrenberg's figures of *C. concavus*. But in my last paper on the Glenshira Sand, I have figured another disc (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 52), which differs from the present form in having a punctum in the centre of each cell, and in the margin being formed of very large cells, divided by strong bars, which appear to project from the plane of the valve, and which also extend beyond the outer margin. This disc I at first suspected to belong to some of the Polycystineæ; but I afterwards found it figured by Ehrenberg as *C. concavus*. I confess that I cannot believe these two discs to be of one species; but that represented in fig. 47 seems to be a true Coscinodiscus; and as it is identical with one of Ehrenberg's examples, we may consider it as the true *C. concavus*, leaving the other for farther investigation. (I have recently found, in some of the Clyde dredgings the other disc, just alluded to as having been figured in my paper on the Glenshira Sand, and as being also named by Ehrenberg, *C. concavus*.)

48. *Coscinodiscus umbonatus*, n. sp. Pl. X., fig. 48. Diameter of disc about 0·0045″. Surface densely cellulate, having a broad, nearly flat, marginal zone,

the central portion being almost or quite hemispherical. It is so convex, that, when the marginal zone is in focus, the middle part appears as if full of air. Cells in lines, generally radiate, rather small, irregular in outline, and unequal in size to some extent. As the rays diverge from each other towards the margin, the space is often filled up by a bifurcation of the rays, which gives an aspect of irregularity to the markings.

This fine disc occurs in one of the Lamlash Bay dredgings, in which, however, it is very scarce indeed. We must hope that it will be found in greater abundance. The broad marginal zone or brim, and the very convex middle part, give to it a great resemblance in shape to a "wide awake" hat; but I have named it from its resemblance to a shield with a large boss in the centre.

49. *Coscinodiscus centralis*, Ehr. Pl. XI., fig. 49. Diameter of disc 0·004″ to 0·009″, or even 0·01″. Surface regularly cellulate. Cells rather small, hexagonal, equal, except at the centre, where there are three large oblong cells, meeting in a point; and between these, a little farther from the centre, three more cells, a little smaller. The valve is remarkably transparent, and from this, and the small size of the cells, it is apt to be overlooked, although, when accurately focussed, the cells are very distinct. It has frequently a yellow or straw colour, in balsam, under the $\frac{1}{2}$-inch objective.

This very beautiful disc is by no means rare in the Glenshira Sand; but when I described that deposit, I was not well acquainted with the discs figured by Ehrenberg, and supposed it to be a form of *C. radiatus*, or else *C. concinnus*. It agrees exactly with Ehrenberg's figure of *C. centralis*. It occurs not unfrequently, both in Lamlash Bay and in Loch Fine, and it reaches occasionally a diameter exceeding that above mentioned.

50. *Eupodiscus subtilis*, n. sp. (Ralfs.) Pl. XI., fig. 50. Diameter of disc 0·0033″. Surface apparently convex, very hyaline, and very densely marked with fine lines, and indications of minute cells, of which the lines are probably composed. In the centre is a rather large circular spot, and the usual pseudo-nodule of the genus is placed close to the margin.

This disc was first noticed by Mr Ralfs in the early part of this winter (1856–57). He distributed specimens as probably *Coscinodiscus concinnus;* but when Dr Greville and myself came to examine it with object-glasses of high power and of superior quality, it was soon recognised as an Eupodiscus. This, so far as I know, was first done by Dr Greville. I had frequently observed a disc like it, with very delicate and obscure markings, in some of my dredgings; but it was Dr Greville also who first ascertained that these discs, which I had taken for granted were *Coscinodiscus concinnus*, were really identical with the disc of Mr Ralfs. As the form occurs in these dredgings, therefore, though less abundantly than in Mr Ralfs' gathering, I figure it here as an Eupodiscus.

51. *Campylodiscus centralis*, n. sp. Pl. XI., fig. 51. Form orbicular, or nearly so.

Surface marked with strong canaliculi, which are broad near the margin, narrower towards the centre, near which they terminate in a portion of the middle of the median line; in length about two-fifths of the diameter. At about one-third of the radius, or a little more, from the margin, is a strong shade, probably a ridge or elevation, of a nearly square outline, placed diagonally to the median line, which passes through two of its angles. Half-way from this ridge to the centre is a second ridge of a circular form, inclosing the elongated centre, from which the canaliculi arise. The radiating canaliculi are fainter within the square ridge, stronger outside of it; they diverge as they approach the margin, near which their ends are joined by semicircular loops, forming a scalloped inner margin, beyond which is an entire outer margin. Diameter about 0·0021″. Canaliculi about 40 in a disc of that size.

This species occurs very sparingly in one of the Loch Fine dredgings, and none of those I have seen are larger than the one figured. It is probable, however, that it may occur of greater dimensions, as the next species does.

52. *Campylodiscus Ralfsii*, Sm. (?). Pl. XI., fig. 52. Diameter 0·003″ to 0·0045″. Form orbicular in many instances, but with all the modifications of the genus. From two points in the median line, near its extremities, arise two lines, diverging in the middle, so as to leave a long, narrow, vacant space, the width of which varies from 0·0003″ to 0·0005″, according to the size of the valve. To these lines the canaliculi reach. These canaliculi are narrow, very short near the ends of the median line, longest in the middle, where they reach a length of from 0·0013″ to 0·0017″. Near the margin each expands into a small round head, and beyond the line of these heads, the margin is entire. Canaliculi in a disc such as is here figured, 64, curved as they approach the ends, having their concavity turned from the centre. The valve is much undulated.

This fine and conspicuous form first occurred to me in the Glenshira Sand, where it is scarce. I afterwards found it, still scarce, in Professor Allman's Lamlash Bay dredging; and still more recently, I found it frequent in three of the Loch Fine dredgings. I have referred it to *C. Ralfsii*, Sm., although it is so much larger than the form figured by him, and although there are other differences. Thus in *C. Ralfsii*, Sm., the canaliculi reach the median line, and the row of heads or expansions lie some distance from the margin. But these differences cannot be regarded as specific. The smaller variety figured by Smith occurs also in some of the dredgings and in the Glenshira Sand. But the larger form seems to be the typical one, and for that reason, chiefly, I have here figured it.

53. *Campylodiscus angularis*, n. sp. Pl. XI., fig. 53. Valve orbicular, with the usual modifications. Diameter of disc from 0·0025″ to 0·0039″, the smaller sizes being the most frequent. The canaliculi from 160 to 180 in large examples, such as the one here figured. They are very short at the ends of the median line, and inclose a broad blank space, which, in the median line, occupies the whole

diameter, but in the middle, in a line at right angles to this, extends to less than half the diameter. Hence the canaliculi form a broad marginal band, except near the ends of the median line, where it is narrow; and the vacant space is elliptical, suddenly contracted at the ends to narrow processes, which traverse the band of canaliculi at its narrowest part to the very margin. At the roots of these processes, the canaliculi suddenly recede from the median line. They are much inclined at the extremities; and the longest are from 0·0008″ to 0·015″ in length. The true median line is visible, and is very delicate; but there are no other markings visible on the vacant middle space.

The surface of the valve, both above and below, that is, near both ends of the median line, is suddenly bent back, so as to form an angle with the rest of the valve. On the surface thus bent, short lines appear, apparently between the canaliculi, which lines terminate abruptly. The greater part of the valve, and especially that part on which are the canaliculi, is convex, which causes the canaliculi to appear curved.

This species is frequent in one of the Loch Fine dredgings, and occurs more sparingly in two others. I have named it from the angular bending back of the valve.

54. *Campylodiscus eximius*, n. sp. Pl. XI., figs. 54 and 54*b*. Form nearly orbicular, and sometimes nearly square, with the usual modifications. I have seen one specimen spiral, like *C. spiralis*. Diameter of disc from 0·004″ to 0·007″ or even 0·008″. Canaliculi strong, very numerous, about 150 in average specimens; confined to a marginal band, the width of which is generally about 0·0007″ or 0·0008′. The middle space is pale and hyaline, and at first appears blank; but on close inspection is seen to be covered with pretty large, very transparent, round granules, which are not arranged in any order, but indiscriminately scattered, like the points in shagreen. The median line is marked by a raphe, the ends of which, as of the middle space, do not, as in the preceding species, traverse the band of canaliculi. There is, at each end, a small point of the middle space which indents the marginal band, that being a very little narrower there than it is elsewhere.

This very fine and conspicuous form is very frequent in one Loch Fine dredging, and less so in two others. At first I thought it might be identical with *C. Hodgsonii*, Sm., of which one example is figured as large as this form. But in his description, Professor Smith describes the middle space as marked by strong radiate lines, formed of large granules; and although I have not seen any of *C. Hodgsonii* of this large size, yet I find the small ones to agree exactly with this description. Now in *C. eximius*, not only is the valve in the middle not marked with strong radiate lines, but under a low power it appears at first sight blank; and the very transparent granules are in general quite irregularly disposed, and only in some instances show faint traces of a linear arrangement close to the marginal band. As far as I have been able to judge, I am satisfied that the small

C. Hodgsonii is not of the same species as *C. eximius*. And with regard to the large *C. Hodgsonii* of the Synopsis, not having seen it, I am unable to decide. But if it be, as the figure indicates, as strongly marked as the small one, it cannot be *C. eximius*. If, on the other hand, it should prove to be like *C. eximius*, then it is probable that it will be found to differ specifically from *C. Hogdsonii*.

55. *Campylodiscus limbatus*, Bréb. Pl. XI., fig. 55. Form orbicular. Diameter from 0·005″ to 0·008″. Canaliculi marginal, short, broad, transversely sulcate, so as to appear, on close inspection, almost moniliform. Within this marginal band is another fainter band (the outer one being strong and black), which looks almost like the reflection in a mirror of the first, except that the bars in it are more decidedly moniliform, or formed of more distinct granules, which, near the median line, extend from both ends towards the centre, in a broad band, which becomes gradually narrower and fainter as it proceeds, and is lost on both sides before reaching the centre. In De Brebisson's figure these projecting bands reach the centre, but I have not been able to trace them so far, and have only done so with difficulty, so far as I have traced them.

This very fine and striking form is not unfrequent in two of the Loch Fine dredgings. About four years ago, I observed a fragment of a large Campylodiscus in a marine gathering from Oban, which was not known to Professor Smith; but I did not describe it, having waited for an entire specimen, which I did not meet with till this last October, when it proved to have been a fragment of *C. limbatus*, which De Brebisson had shortly before figured as occurring at Cherbourg.

GROUP V.

Amphiproræ.

Of the genus *Amphiprora*, the new species in these dredgings, though not very numerous, are very interesting. One form is very doubtfully referred to this genus.

56. Amphiprora pusilla, n. sp.	60. Amphiprora obtusa, n. sp.
57. ... plicata, n. sp.	61. ... maxima, n. sp., G.
58. ... elegans, Sm.	62. ... (?) complexa, n. sp., G.
59. ... lepidoptera, n. sp., G.	

56. *Amphiprora pusilla*, n. sp. Pl. XII., figs. 56 and 56 *b*. Form of the F.V. nearly rectangular, a little incurved in the middle, and expanded at the extremities. Above each valve lies a plate, in shape like a narrow arc of a very large circle, the convex edge outwards, the middle of it slightly overlapping the central constriction, while the ends coincide with the inner terminal angles of the valve. Length about 0·0027″; breadth, including plates, at the middle, 0·0008″. S.V. lanceolate,

narrow, and, but for the absence of apiculi, a miniature of that of *Apr. lepidoptera*. (See farther on, fig. 59 *b*.) Both views, and also the plates, are marked with very fine transverse and parallel striæ, which, however, on the F.V. do not extend to the rectangular space between the valves. Striæ about 60 in 0·001″.

This pretty little species has occurred only in one of the Loch Fine dredgings, where it is rather scarce. It is remarkable for the occurrence of the lateral plates, which we shall find in several other species, and which perhaps ought to constitute a new genus. But the resemblance to Amphiprora is so great in other points, that I do not think it advisable to separate these species from that genus.

57. *Amphiprora plicata*, n. sp. Pl. XII., fig. 57. F.V. deeply constricted, with rounded extremities, which are very broad. Inner margin of valves straight. On each valve lies a plate extending from the inner margin of the valve to the nodule; its outer or convex margin being a little incurved in the middle, and bent forward at the ends, to join the inner angles of the valve. The rectangular space joining the two valves is marked with faint vertical lines or folds; and the valves and lateral plates, as well as the rectangular space, are all marked by fine transverse striæ, about 50 in 0·001″. Length 0·0037″; breadth from nodule to nodule 0·00075″, at broadest part 0·0016″. Outer margin double. S.V. not yet certainly known.

This species occurs with the preceding, and is equally scarce. It approaches nearest to *Apr. alata*, but differs from it in the folds of the middle space, and in the presence of the lateral plates.

58. *Amphiprora elegans*, Sm. Pl. XII., figs. 58 and 58 *b*. Form of S.V. linear-lanceolate, ends obtuse. Valve traversed by two longitudinal lines, and marked by fine but distinct transverse striæ. Length from 0·0085″ to 0·012″; breadth of S.V. about 0·0011″. F.V. rectangular, with two longitudinal lines. Striæ 44 in 0·001″, parallel.

I have figured the S.V. of this fine form, first noticed by Mr Bleakley, because, although described in vol. ii. of the Synopsis, no figure of it has yet appeared. It occurs both in the Glenshira Sand, and in more than one of the dredgings; but more frequently in one of those from Loch Fine than in any of the others. The valves are, in these deposits, generally detached, so that I have not met with the entire F.V. I am indebted to Mr Roper for an entire specimen, fig. 58 *b*.

59. *Amphiprora lepidoptera*, n. sp. Pl. XII., figs. 59, 59 *b*, and 59 *c*. Form of FV. linear, constricted in the middle, expanded at the apices, which are flatly rounded. Length from 0·0055″ to 0·008″; breadth of F.V. in the middle 0·0009″, near the ends 0·0015″. SV. lanceolate, with acute ends, terminating in small apiculi. Central nodule strongly marked; median lines somewhat curved, meeting in the nodule. Striation fine, parallel, about 48 in 0·001″; on the F.V. the striæ are absent from the middle space. The form of the F.V. with its long slender alæ, is very elegant.

(Since this paper was read, I have ascertained that the peculiar plates above mentioned in *Apr. pusilla*, and in *Apr. plicata*, occur also in this species. I had overlooked them, from their being very narrow. They rise from the inner margin of the valves, as seen on the F.V., from two points a short distance from the extremities. Their convex margin extends in the middle just beyond the constriction. They are so narrow as to be readily overlooked, but are quite distinct, and appear to be thicker at the outer or convex margin than at the inner or plane one. There is even some appearance of a second plate in each valve, rising from the same line, as that just mentioned, but apparently extending in a plane at right angles to the surface of the valve, so that, in the F.V., it is seen foreshortened. and appears as a dark line. But I am not yet satisfied about this second plate.

The plates first named constitute a very peculiar feature, both in this species, and in the two previously described. We shall see the same structure still more developed in another species, a very remarkable one, namely, *Apr. maxima*.)

The form represented in fig. 59 *c*, I am satisfied belongs to this species; but I do not quite understand its relations. It is of an elegant rhombic-lanceolate form, with two curve-lines on each side, which at the middle bend inwards, to join a strong stauros, interrupting the median line, and at the ends coalesce with the margin. The whole valve is marked with fine parallel striæ, except the stauros. This form, as will be seen, differs considerably from the usual S.V., (fig. 59 *b*), which latter often occurs with the longitudinal lines in it much more curved than in the example figured. Can the lateral lines in fig. 59 *c* be the outer margins of the two plates, above described, or perhaps of those suspected to lie in a different plane?

This species first occurred to me in the Glenshira Sand, and I figured the S.V. as *Apr. vitrea*, β? in my first plate of that deposit (*Mic. Jour.*, vol. iii., pl. iv., fig. 14, the larger of the two figures, which shows the curved lines above alluded to, with a third line to the side); and the F.V. in my third plate (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 39). But the former had not the striæ, and the latter was from a very inferior specimen, and, besides, did not show the peculiar plates. I have therefore figured both views here from good specimens, which are frequent in Mr Miles's Corallina gathering from Corriegills, near Lamlash Bay. It is at once distinguished from *Apr. didyma* by its much more elegant form, and by being twice as long. The S.V., also, is quite different. I am informed by Dr Greville that he finds this form abundantly, even predominating, in a dredging or gathering from nearly the same locality, which he made early last summer.

60. *Amphiprora obtusa*, n. sp. Pl. XII., fig. 60. Form of F.V. linear, broad, with rounded ends, slightly incurved in the middle. The termination of the middle space projects a little beyond the general curve of the apices. The inner margin of the striated part of the valve is gently and gracefully curved, forming two concave lines towards the middle, which meet in a point of the inner margin

of the valve, in a narrow, elongated nodule, which is in a line with the terminal nodules, also long and narrow. But close to the outer margin, at the constriction, is another large round nodule. The narrow portions, which lie between the curve-lines just mentioned and the inner margin of the valve, appear, like the middle space, to be unstriated; but perhaps this is because they lie in a different plane from the outer compartments. And even in the last, the striæ are too fine to be resolved under a power of 400.

This remarkable form occurs in one of the Loch Fine dredgings, where, however, it is very scarce. I have not yet recognised with certainty the S.V., but there are some forms which may prove to belong to it.

61. *Amphiprora maxima*, n. sp. Plate XII., figs. 61 and 61 *b*. Form of F.V. rectangular, very broad, but deeply constricted in the middle, and rounded on the ends. Length 0·0068″; greatest breadth 0·0028″ to 0·003″, breadth of frustule at constriction 0·0017″. Central nodules large, situated close to the constriction in the outer margin. Terminal nodules on the inner margin of valves, conspicuous. Over each valve lies a strong, broad plate, arcuate outwards, straight on its inner margin, which coincides with that of the valve. Outer margin of plate prominent in the middle, and on each side of this prominence slightly incurved; thick, overlapping the valve at the constriction. Breadth from the margin of one plate at this point to that of the other 0·0023″. Valves and plates transversely striated. Striæ distinct, parallel, about 36 in 0·001″, thicker and apparently coarser at the margin of the plate. S.V. lanceolate, ends acute, with a trace of a constriction just before the apices. Greatest breadth 0·00125″. Two strong lines, as in *Apr. lepidoptera*, proceed from the apices, on one side, and curve inwards to join a large nodule at one-third of the width. Round this nodule the striæ are curved, as if pushed outwards. Between the nodule and the nearest margin the striæ are very short, and they leave a large blank space, extending to the margin. Valve thick and very convex on the S.V. Query, Are the curve-lines on the S.V., the outer margin of the plates, seen on the F.V? From the convexity of the S.V. its striation is more conspicuous than that of the F.V.

This, which is one of the finest and most interesting forms described in this paper, first attracted my attention in the Glenshira Sand. But I could only find there halves of the F.V; and I postponed the description of it. In Professor Allman's Lamlash Bay dredging I again saw these halves occasionally; but it was not till I examined, in October last, the Loch Fine dredgings, that I found, in one of them, besides a number of the same halves, some entire frustules; and finally, the S.V. which, from its comparative narrowness, is seldom presented; the frustule or detached valve lying on its broadest side. There is something very beautiful in the entire form; and in its structure it is peculiarly interesting, as presenting the peculiar lateral plates, already noticed in *Apr. pusilla*, *Apr. plicata*, and *Apr. lepidoptera*, and that in a high degree of development. The occurrence of

these plates in four species, all of which have the general aspect of *Amphiproræ*, naturally leads to the inquiry, whether this remarkable structure may not be found in all the species of the genus, or whether the forms in which it occurs ought not to form a new genus. I understand from Mr Roper, that he has found this species in a marine gathering from the coast of Wales, or of the south of England.

62. *Amphiprora* (?) *complexa*, n. sp. Pl. XII., figs. 62; 62, *b*; 62, *c*; 62, *d*; and 62, *e*. Form elliptical, broad, with a constriction in the middle, and broadly rounded ends. The frustule is composed of two arcuate and constricted segments, which are broad, thick at the outer margin, thin at the inner margin, and placed opposite each other, with a narrow interval between them. Over the middle of these two lateral segments is placed a complex mass, formed of five or six segments, converging inwards and on the ends, like the segments of an orange or melon. The thick backs of these central segments, marked with transverse striæ, are alone seen in the entire frustule, and those of the outer segments approach near to the outer margins of the lateral or flat lying segments, leaving only part of the surface of the latter exposed. A convex line joins the convergent ends of the central segments

When the frustule, as often happens, falls asunder, a number of segments are found lying near each other. Some of these have no constriction, and no nodule; these I take to be the segments of the central mass. Those with nodules at the middle of their outer margin, at the constriction, seem to be the lateral segments.

Length of frustule 0·0035″ to 0·004″; breadth 0·0028″. Segments arcuate, broad; some with a nodule and constriction on the thick convex or outer margin, others without. Surface of segments finely striated; striæ about 45 in 0·01, delicate, radiating from the thin or inner margin, and curved near the ends of the segment. At the margin, there is a row of conspicuous puncta, about 1 for every 2 striæ. The backs of the central segments, when *in situ*, are striated, but exhibit neither nodule nor puncta.

It is with much doubt that I refer this very curious form to *Amphiprora*; which I should not do, were it not that the lateral plates in four species already described may be regarded as the rudiments of the complex central mass in this species. One of the three alluded to, *Ap. plicata*, has even longitudinal folds in the middle part.

It is, however probable, that the remarkable structure of this species may render necessary the establishment of a new genus, a step which I do not venture to take without farther inquiry.

This form, like the preceding one, first caught my eye in the Glenshira Sand, where I could find, however, only detached segments, and one half frustule. Not being able, from these, to understand the structure, and not, indeed, perceiving the

connection between the segments and the half frustule, which only showed the backs of the segments, I postponed it with other forms, all of which I have now been able to establish as species. I found the present species tolerably frequent in Mr Miles's Corallina gathering, in which detached segments are much commoner than the entire frustules.

We shall see, farther on, that a similar structure prevails in a numerous section of Amphoræ, a few of which were described in my last paper on the Glenshira Sand.

GROUP VI.

Amphoræ.

The new forms of this genus in these materials are very numerous, since, in addition to almost all those (ten in number) which I had described in the Glenshira Sand, they have yielded about 32 additional undescribed species. And as I have again to describe and figure four of the Glenshira species, which are now better known, there are in all, 36 species of Amphoræ to be described and figured. As the whole of the British species figured in the Synopsis of Professor Smith amounted only to eight, it appears that the Glenshira Sand, and the Clyde and Loch Fine dredgings, or the latter alone, without the sand, have yielded a five-fold addition to the British forms of this genus. This may serve to show what stores of undescribed forms are yet to be found in our estuaries; for all these have been obtained from two localities, namely, Lamlash Bay and the upper part of Loch Fine, just below Inveraray.

The remarkable group of complex Amphoræ, to which I have lately directed attention, and of which the first known example was *A. costata*, Sm., though the peculiarity of its structure seems to have been overlooked, has now become so large (one-half of the species here described belonging to it) that it is necessary to subdivide the genus. I shall, therefore describe the Amphoræ in two sub-groups, viz., A. Simple, and B. Complex Amphoræ.

(Since this paper was read, I have ascertained that two of the Amphoræ in the following list of simple species, namely, *A. monilifera*, and *A. spectabilis*, belong to the complex division. The latter is indeed one of the most curious of the complex Amphoræ.)

A. *Simple Amphoræ.*

63. Amphora turgida, n. sp.
64. ... nana, n. sp.
65. ... macilenta, n. sp.
66. ... angusta, n. sp.
67. ... binodis, n. sp.
68. ... ventricosa, n. sp.
69. ... monilifera, n. sp., G
70. ... lineata, n. sp.
71. ... Ergadensis, n. sp.
72. ... lævissima, n. sp.
73. Amphora, pellucida, n. sp.
74. ... lævis, n. sp.
75. ... exigua, n. sp.
76. ... dubia, n. sp.
77. ... truncata, n. sp.
78. ... oblonga, n. sp.
79. ... robusta, n. sp.
80. ... spectabilis, n. sp.
81. ... Proteus, n. sp.

63. *Amphora turgida*, n. sp. Pl. XII., fig. 63. Form nearly orbicular, with short, square, produced apices. Detached valves, nearly semicircular, with the vertical margin straight and the extremities capitate. Nodule conspicuous. Length from 0·001″ to 000·2″; breadth 0·008″ to 0·0015″. Valve marked with somewhat coarse radiate striæ. Striæ 24 in 0·001″.

This little form occurs in Lamlash Bay, but it is more frequent in one of the Loch Fine dredgings, which, though stony and unpromising in aspect, is yet singularly rich in undescribed species, and especially in Amphoræ. The detached valves or halves of this species, which are like small and very convex *Cymbellæ*, are much more frequent than the entire frustule.

64. *Amphora nana*, n. sp. Pl. XII., fig. 64. Form a narrow, linear-elliptic. Inner lines lying very near the ventral margin of valves, and nearly straight. Nodules small; near the ventral margin. Rectangular middle space very narrow. Length 0·001″ to 0·0016″; breadth 0·0004″ to 0·0005″. Striæ about 50 in 0·001″.

This small species occurs with the last, and is tolerably frequent.

65. *Amphora macilenta*, n. sp. Pl. XII., fig. 65. Form elliptic, long and narrow, contracting towards the ends, which are again slightly expanded, and terminate so that it has very short, square, produced ends. Valves very slender, arcuate on dorsal margin, straight on ventral. Middle space narrow, with a strong median line. Length 0·0018″ to 0·0022″; breadth of entire frustule 0·0005″ to 0·00086″. Striæ parallel, rather coarse, about 30 in 0·001″.

This species occurs with the two preceding, and is not at all unfrequent.

66. *Amphora angusta*, n. sp. Pl. XII., fig. 66. Form nearly rectangular, or rather linear-elliptic, narrow, truncate at the ends, so as to form a slender and somewhat elegant barrel shape. The inner curve-lines arise in each half or valve from the outer angle, and meet in a nodule situated half-way across the valve. Both valves are transversely striated across, up to the narrow rectangular space in the middle. Length 0·0015″; breadth of entire frustule 0·0004″. Striæ fine, about 44 in 0·001″.

This species occurs with the preceding ones, chiefly in the Loch Fine dredging above specified, where it is not very rare.

67. *Amphora binodis*, n. sp. Pl. XII., fig. 67. Form linear, with rounded apices, and two expansions half-way between the middle and the ends, between which it is deeply constricted. Length from 0·00175″ to 0·002″; greatest breadth about 0·0005″; breadth of narrow parts 0·0003″. Rectangular middle space narrow. The detached halves or valves, having the ventral margin straight, and two large rounded expansions, while the ends are linear, much resemble the frustules of *Eunotia Camelus*, as described by Professor Smith in vol. ii. of his Synopsis, and figured by Dr Greville in the Annals of Natural History; but the expansions are smaller and more rounded than in *E. Camelus*. The inner lines

run nearly parallel to the outer margin of the valve, but are less curved, and generally obscure. Striæ transverse, obscure, about 30 in 0·001″.

This very curious little form occurs by no means unfrequently in Professor Allman's dredging from Lamlash Bay, and more sparingly in the Loch Fine dredging alluded to under *A. turgida*. The resemblance of the detached valves to *Eunotia Camelus* has been mentioned. The entire frustule also resembles a miniature of *A. angularis*, Greg., figured by me in my first paper on the Glenshira Sand. (See *Micr. Jour.*, vol. iii., pl. iv., fig. 6.) But besides the much smaller size of *A. binodis*, the expansions in *A. angularis* are more angular; indeed, the figure represents them less so than they are; and the striation of *A. angularis* is not only distinct, but very much coarser, so that the whole aspect of the two forms is different.

68. *Amphora ventricosa*, n. sp. Pl. XII., fig. 68 and 68 *b*. Form linear-lanceolate, with obtuse apices, more or less expanded in the middle. Valves long, very slender, arcuate, with acute ends, marked with somewhat coarse transverse striæ. Length from 0·0023″ to 0·0035″; greatest breadth at the expansion in the middle 0·005″ to 0·008″. Inner lines generally obscure, but in some cases well seen. They arise from the inner angle of the valve, pass rapidly across, and, as shown in fig. 68 *b*, sometimes extend beyond the outer margin; then suddenly bend inwards to meet the obscure nodule near the inner margin. Rectangular middle space narrow. Striæ strong, about 22 in 0·001″; conspicuous.

This pretty and interesting species occurs not unfrequently both in Professor Allman's dredging from Lamlash Bay, and in the Loch Fine one so often alluded to above. The detached valves resemble a very long and slender Cymbella, as may be seen in fig. 68, in which the form of the valve is plainly seen on each side of the frustule. By focussing, a transverse bar, or elongated nodule, may be seen in the middle of the valve; but it is obscured by the striæ, when they are in focus.

69. *Amphora monilifera*, n. sp. Pl. XII., fig. 69. Form elliptic, slightly recurved at the apices, which form very short produced extremities. The recurved ends of the valves do not meet, and the space between them is filled up by a transverse curve-line. Valves arcuate, very convex on dorsal margin, with recurved ends. Nodules on the ventral margin. Between the valves the frustule is marked by three to five longitudinal rows of distant round granules, giving to it a dotted aspect. If there be transverse striæ, they are very obscure. Length from 0·0017″ to 0·0026″; breadth from 0·0008″ to 0·0011″.

This pretty and well-marked species is tolerably frequent in the two dredgings mentioned under the last form, but chiefly in that from Lamlash Bay. It occurs also very sparingly in the Glenshira Sand, as may be seen by referring to my last plate (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 53). I had figured, from that deposit, an imperfect specimen, which at first I took for an *Amphora*, but subse-

quently supposed to be possibly not Diatomaceous. The Lamlash Bay dredging soon cleared up this point, and established it as a distinct species of *Amphora*.

(Since the preceding description was written, I have ascertained that this species belongs to the complex group. Like the other complex species, it is formed of a number of Cymbelloid segments, arranged like the segments of a melon, as will be more particularly described farther on, in some of the other species. The convergent longitudinal lines of dots prove to be the dotted backs of the central segments. I have not been able to find a detached segment, the frustule being usually entire, so that I cannot give a figure of the segment. I can see, however, that it has the form of a Cymbella.)

70. *Amphora lineata*, n. sp. Pl. XII., fig. 70. Form elliptic or elliptic-lanceolate, with short, produced apices, which are truncate. The valves are slender, arcuate on the dorsal, straight on the ventral margin. The nodules lie in the middle point of two strong lines, slightly curved, within the ventral margin. Outside of the nodule is another curve-line, dividing the valve longitudinally into two parts, the outer one being the broader. This outer portion is marked by fine longitudinal lines, of which there are generally four in each valve. The rectangular middle space is narrow, and has a sharp line down the middle. This line, with the inner margins of the valves, the slightly curved line with the nodule, the line beyond it, and the four exterior lines, give to the whole a lineate aspect, which is very characteristic. In certain views, the frustule appears lineate uniformly from one side to the other. Length from 0·0022″ to 0·003″; breadth of frustule 0·0007″ to 0·008″. The whole frustule is marked with transverse parallel striæ, which are fine, about 42 in 0·001″, and obscure, except at the margins, where they may generally be distinctly seen. As the longitudinal lines, however, are much more conspicuous, it is they which characterize the species.

This species is not unfrequent in the dredgings so often mentioned, and occurs also in the Glenshira Sand. In my last paper on that deposit, I described and figured it (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 33). But by some mistake of mine, the form there figured was either not a characteristic specimen, or, more probably, a form of *A. salina*. I have therefore described it anew, and given, in fig. 70, an accurate representation of a very frequent size and shape of this species.

71. *Amphora Ergadensis*,* n. sp. Pl. XII., fig. 71. Form elliptic-lanceolate, narrow, with truncate apices, which are very slightly expanded. Valves in apposition, or nearly so, long, slender; the inner lines, on which lie the conspicuous central nodules, forming one gentle curve from one end of each valve to the other. Length about 0·0035″; breadth in the middle 0·00075″. The valves are marked by strong and conspicuous transverse striæ, about 24 in 0·001″.

* From Ergadia, Argyll.

This form, like most of the preceding Amphoræ, occurs both in Lamlash Bay and in Loch Fine; but it is not so frequent as most of the others. It is conspicuous from its length.

72. *Amphora lævissima*, n. sp. Pl. XII., fig. 72. Form elliptic, rather narrow, ends subtruncate. Aspect remarkably hyaline. Length 0·0025″ to 0·003″; breadth 0·0009″. The inner lines in each valve rise from the inner angles, bend suddenly outwards for a short distance, then gently inward to the nodule, leaving a very narrow inner compartment at each end. The middle of each valve is traversed by a strong bar or elongated nodule. The valves are transversely striated, but the striæ are so fine, and the valves so hyaline, that the striation has not yet been perfectly resolved.

This delicate species is not very rare in the Loch Fine dredging noticed under *A. turgida* as rich in Amphoræ. But the detached valves are much more frequent than the entire form. These detached valves have a resemblance in form to the valve of *A. elegans*, figured in my last paper on the Glenshira Sand (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 30). But *A. elegans* is a considerably larger form, and not particularly hyaline; while its striæ are much less fine, and thus easily seen.

73. *Amphora pellucida*, n. sp. Pl. XII., figs. 73 and 73 *b*. Form a broad oval. Length from 0·002″ to 0·003″; breadth from 0·0012″ to 0·0018″. Valves arcuate, very convex on the dorsal, slightly concave on the ventral surface. There is something very peculiar in the aspect of the terminal parts of the inner margins of the valves, as these coincide with the terminal nodules, which, from the delicacy and transparency of every other part of the inner margin of the valve, appear to project like horns, as the nodules not only coincide with the ends of the inner margin, but are narrow and much elongated. The inner lines follow the margin for a little, then bend rapidly outwards, and then as rapidly inwards to the central nodule on the ventral margin. The ventral margin, except at the nodules, is so hyaline as to be seen only on close inspection, as are also the spaces lying between it and the inner curve-lines. The space outside of these latter curve-lines is striated, the striæ rather coarse; but the whole is so hyaline, that the striæ are only seen on very close inspection, when they come out plainly. They are somewhat inclined, thicker and more easily seen at the outer margin, very delicate and nearly invisible towards the inner one, and about 30 in 0·001″. There is a narrow, rectangular space between the two valves. Sometimes the form of the frustule becomes nearly rectangular, but all its other characters continue as before.

The form of the valves, as well as of the entire frustule, in this species, is somewhat similar to that of *A. ovalis*. But, not to dwell on the marine habitat of *A. pellucida*, its very hyaline aspect, and the singular delicacy of the striæ, which are entire, and, towards the nodule, become so fine as to be hardly visible, effectually

distinguish it from that species, in which the striæ are conspicuous and moniliform.

This species is not unfrequent in the Loch Fine dredging mentioned under *A. turgida*. It has some resemblance, in the form of the valves, to *A. incurva*, Greg. figured in my first paper on the Glenshira Sand (See *Mic. Jour.*, vol. iii., pl. iv. fig. 5). But in *A. incurva* the striation is conspicuous, not hyaline, and the form of the valve is more elongated, and less projecting at the extremities. I am not yet acquainted with the entire frustule of *A. incurva*.

74. *Amphora lævis*, n. sp. Pl. XII., figs. 74, 74 *b*, and 74 *c*. Form of frustule rectangular, slightly incurved in the middle, sometimes with the ends rounded, but more commonly with nearly square ends. Length from 0·0017″ to 0·003″; breadth from 0·0007″ to 0·0012″. Aspect very hyaline. In each valve the inner curve line, rising from the inner angle of the valve, and following the margin outwards, bends inwards again in a long graceful curve to the central nodule placed just within the ventral margin. The nodule extends as a strong bar across the middle of the valve. The outer compartments are transversely striated, but the striæ are very fine, about 60 in 0·001″, very hyaline, and hardly to be seen with a power of 400.

I have added, in fig. 74 *d*, the figure of a fine *Amphora*, which probably belongs to this species. It is remarkable for the fact, that the curve line coincides with the outer margin till very near the middle, where it bends inwards to the nodule. There are also four longitudinal lines at considerable and equal distances. In this specimen, I have not seen the striæ as in the others; but believing it to be of the same species, I give the figure under the head *A. lævis*.

This species occurs with the last, and is more frequent, though by no means abundant. It frequently happens, as in one of the figures, that the valves are in apposition; and as this occurs in long examples, these become proportionally narrow.

75. *Amphora exigua*, n. sp. Pl. XIII., fig. 75. Form linear-elliptic, narrow, with somewhat obtuse ends. Length from 0·0015″ to 0·0022″; greatest breadth about 0·00035″. The valves are transversely striated, the striæ being strongest at the margin. Striæ about 28 in 0·001″.

This little form occurs, scattered, both in Lamlash Bay and Loch Fine. In size and form it approaches nearest to *A. lineata* (Pl. XII. fig. 70); but its markings are totally different. It has no striking characters, but I cannot refer it to the same species with any of the small *Amphoræ* I have here described.

76. *Amphora dubia*, n. sp. Pl. XIII., fig. 76. Form of entire frustule oval, flattened, or even a little incurved, on the sides. The valves are concave in the middle of the ventral margin; so that as they are in apposition, there is a longish rhombic opening in the middle, between them. They also diverge a little at the apices. Within each valve is a faint line, nearly parallel to the outer margin.

but lying nearer to the inner margin. On these lines are faint nodules, while at the middle of the inner margin there are distinct nodules. Length 0·0025″; breadth 0·0011″. The whole is marked by transverse striæ, which also traverse the openings between the valves, in which openings a median line is also visible. Striæ fine, subdistant, about 24 in 0·001″. The divergent extremities are joined by a convex curve line.

This very peculiar species occurs, but very sparingly, in Lamlash Bay. It has some analogy with *A. marina*, a form lately figured by Professor Smith, in the *Annals of Natural History* (January 1857, vol. xix., pl. i., fig. 2.) But besides the coarser striation and distinct nodules of *A. dubia*, Professor Smith describes *A. marina* as so much resembling *A. affinis*, that it has been confounded with that species on our coasts. Now *A. dubia* has no resemblance whatever to *A. affinis*, nor indeed to any other known species; so that I have some doubts as to its being really an Amphora. The figures of *A. marina* given by Professor Smith are not satisfactory, for they do not at all resemble *A. affinis*.

77. *Amphora truncata*, n. sp. Pl. XIII., fig. 77. Form of frustule slightly barrel-shaped, broad, with truncate ends. Length about 0·0028″; breadth about 0·0017″. Valves arcuate on dorsal, straight on ventral margin. Inner curve lines arise from the terminal margin, and bend gently inwards to a small nodule, rather nearer the inner than the outer margins. Inner margin of valve marked by a longitudinal line of short transverse striæ. In the rectangular space between the two valves, which is broad, are two similar longitudinal lines of short striæ, near to, and parallel to those just mentioned, and between the two last-named lines or bars are traces of others. Valves transversely striated, but the striæ are more conspicuous on a band at the outer margin, than elsewhere. So that the frustule appears, at least in a certain focus, marked with longitudinal striated bands, formed of short striæ.

This species is not very rare, in either of the two gatherings so often named in connection with Amphoræ. It frequently happens, that the line joining the ends of the valves appears to be interrupted in the middle. But by careful focussing it may be seen.

The appearance of longitudinal striated bands on the middle part of the frustule seems to indicate a tendency in this species to the complex structure. Indeed, among the complex species there is one, *A. quadrata*, which has considerable analogy with the present form.

78. *Amphora oblonga*, n. sp. Pl. XIII., figs. 78, and 78 *b*. Form linear elliptic, rather broad, the ends obtusely acuminate. Length from 0·0034″ to 0·004″: breadth from 0·001″ to 0·0014″. The inner curve lines are strong and much curved, but they keep unusually near the outer margin of the valve, only in the middle projecting rather more than half way across. The central nodules, which are conspicuous, are situated outside of the curve lines, and nearer to the outer

margin. Valves transversely striated. Striæ about 24 in 0·001″, conspicuous. The portion of the valve outside of the curve line seems to be in a plane different from that of the inner portions, and the striæ on the latter are radiate.

This well-marked and conspicuous species, remarkable for the position of the central nodules, is by no means rare in one of the Loch Fine dredgings, in which, except this form, *Campylodiscus angularis*, and *C. Ralfsii*, but few forms are found. It occurs more sparingly in some of the other dredgings.

79. *Amphora robusta*, n. sp. Pl. XIII., figs. 79, 79 *b*, and 79 *c*. Form of frustule a broad oval with subtruncate extremities. Length from 0·0033″ to 0·0048″, occasionally even as much as 0·006. Breadth from 0·0018″ to 0·0024″. Valves arcuate, with the ends more or less obtuse; ventral margin straight or slightly concave. Inner curve lines very sharp and strong, rise from the inner angles, pass outwards, without reaching the outer margin, and bend suddenly inwards to the central nodules, which are within the ventral margin, by one-fifth of the width of the valve. In some specimens, or perhaps in a certain focus, the points where the two curve lines meet in the middle is on the inner side of a straight line, apparently forming the inner margin of the valve, so that a small blank triangular space is included between that straight line and the ends of the two curve lines. This produces a very peculiar aspect. Frustule thick, marked with strong striæ, which on the compartments outside the curve lines are transverse, but on the inner terminal compartments are somewhat radiate. Striæ subdistant, moniliform, about 16 in 0·001″. Figs. 79 *b*, and 79 *c*, represent two valves, in one of which the curve lines are seen, while it is evident that the inner compartments are in a different plane from the outer one. The other shows the entire valve viewed in a different focus, in which the striæ appear all in one plane.

This fine form, conspicuous for its size and the stoutness of its aspect, is not rare in the Loch Fine dredging mentioned under *A. turgida*, and occurs also in Lamlash Bay.

80. *Amphora spectabilis*, n. sp. Pl. XIII., figs. 80, 80 *b*, and 80 *c*. Form nearly rectangular, broad, with rounded angles; occasionally sub-elliptic. Length from 0·003″ to 0·0047″; breadth from 0·002″ to 0·0025″. The inner curve lines bend inwards from the outer margin very nearly to the inner margin of the valve, dividing the valve into a middle outer compartment, and two terminal inner ones. The detached valve has prominent obtuse rostra or beaks, not seen in the entire form. Outer compartment transversely striated, the striation being very coarse; inner compartments marked with radiate striæ, which are much finer. Aspect of the whole form soft and indistinct, so that in general only the marginal ends of the striæ in the outer compartments are easily seen, these striæ being thicker at that end, and the frustule very convex. Even at the margin they are not sharp but softened. Striæ in the outer compartment 14 to 16 in 0·001″: in the inner ones, 26 in 0·001″, and very obscure. As in the last species, and indeed in many Am-

phoræ, the inner compartments seem to be in a different plane from the outer ones. This species exhibits two frequent varieties, both smaller than the typical form, which are figured (figs. 80 *b*, and 80 *c*). One is long and narrow, its length being 0·0032″, its breadth hardly 0·0008″. The other is short and broad; its length being 0·002″, and its breadth 0·001″. Both have the soft, hazy, indistinct aspect of the larger form, and both, when carefully examined, exhibit the same characters, except that the striation is perhaps somewhat finer.

(Since the above was written, I have found that this species not only belongs to the complex group, but is one of the most interesting forms in that group. As will be seen in the descriptions of various complex *Amphoræ*, the complex structure is only seen in one focus, while in another, the frustule exhibits the characters of a simple *Amphora*. This is peculiarly the case in *A. spectabilis*. The simple view, shown in fig. 80, is so thoroughly simple, that it never occurred to me that it could possibly conceal a second and more complex structure. But when I happened to examine, under a rather high power, and with oblique light, a frustule well placed for bringing out this structure, I detected the appearance shown in fig. 80 *d*. In this case, the coarse transverse striæ of the middle part, as seen in fig. 80, were still traceable, at the margin only, on one side, because the frustule did not lie quite flat. In other examples, no such traces are visible on the complex view, although in general the very strong and elongated central nodules shine through. In fig. 80 *d*, it will be seen that the whole frustule is composed of parallel, longitudinal, very slightly convergent bars, with a narrow sulcus between each two bars. These bars are transversely striated, and the striæ, though much finer than even those of the inner and terminal compartments of the valves, are yet quite distinctly seen, even more so than the others, being apparently free from the haziness above alluded to. Striæ of the bars, in this view, about 50 in 0·001″.

In this remarkable form we have the unusual occurrence of three distinct systems of striation; 1*st*, The coarse, soft, and hazy transverse striæ of the outer and middle compartments of each valve, on the simple view, fig. 80; 2*dly*, On the same view, the finer and radiate striæ of the two terminal inner compartments of each valve, which lie in a plane inclined to that of the middle compartments; and, 3*dly*, The still finer and parallel striæ crossing the longitudinal bands, in the complex view, fig. 80 *d*, which lie again in a different plane. The frustule is also remarkable for its convexity, which is probably the cause, or one chief cause, of the indistinctness of the markings on the simple view.)

This beautiful species is tolerably frequent in Professor Allman's dredging from Lamlash Bay, and in one from Loch Fine, less so in the other dredgings. The short variety is even more frequent than the typical form. The species has some resemblance to the preceding one in general form and coarseness of striation. But it is at once recognised by its hazy indistinct aspect, by the finer

striation on the inner compartments, by the beaked form of the detached valves, and finally, by its varieties. (Its complex structure, and its three systems of striæ, are even more decisive characters.)

81. *Amphora Proteus*, n. sp., Pl. XIII., figs. 81, 81 *b*; 81 *c*; 81 *d*; and 81 *e*. Form very variable, obtuse lanceolate, elliptical, barrel-shaped, broad and truncate, long and narrow, &c. It has usually the rectangular space between the valves, but sometimes the valves are in apposition, and then resemble twin frustules of *Cymbella*. Some of these modifications are figured. The size also varies prodigiously. Length from 0·0015″ to 0·006″; breadth from 0·0013 to 0·0024″. The broadest examples are generally short. Valves acute, sometimes with arcuate, at other times with obtuse apices. Inner curve lines and nodules strongly marked, and inner compartments of the valve in a different plane from the outer ones. There are two peculiarities which are found in all specimens, from the smallest to the largest. When the outer compartment is in focus, and its striæ conspicuous, the striæ of the inner compartments appear in narrow lines or bars, separated by white longitudinal lines or *raphes*; and the transverse striæ, which are finely moniliform, are, especially, in a certain focus, traversed by longitudinal wavy lines or striæ, produced by the circumstance that the granules of contiguous transverse striæ are not placed exactly opposite each other. In figs. 81 and 81 *b*, the same specimen, a long one, is shown as it is seen in two different foci, one of which brings out the curve lines and nodules, the other the transverse striæ, which extend across the whole valve. These striæ are about 22 in 0·001″; but in regard to the number of striæ there are very great variations in this species, as I have shown, in former papers, to occur in other species. In some of the smaller specimens the striæ are at least twice as numerous as in some of the larger, and even in the specimens of equal size they differ in this respect. But in all, the striæ exhibit the characters I have mentioned as peculiar and characteristic.

(I must here state, however, that there are some forms which, for the present, I include under *A. Proteus*, respecting which I have great doubts whether they ought not to form a distinct species. These forms have many characters in common with *A. robusta*, but have uniformly a much finer striation, and consequently a very different aspect.)

This species is very frequent in Lamlash Bay, and also in some of the Loch Fine dredgings. At first I was quite at a loss with the multitude of forms agreeing in striation, but when I had observed the characters above mentioned, I was able to trace all these forms into one another by gradual transition. Those here figured are some of them very different; but intermediate forms occur. One of the figures represents two valves in apposition, which I suspect to belong to this species (fig 80, *e*); but I am not quite certain about that one.

I may here direct attention to the fact, that such a form as that shown in fig. 81 *e*, resembles closely a twin frustule of a Cymbella or Cocconema. Yet it is con-

sidered as only a single frustule, while the Cymbella or Cocconema would be called a double one.

I confess I feel disposed to consider both in the same light, as double forms. If that be correct, then what is usually called the valve of an Amphora will be an entire frustule, and what is usually called the entire frustule (as in figs. 63 to 72, for example) will be considered as two frustules in the act of self-division, but still united by the connecting membrane.

One reason why I incline to this view is, that what, on this principle, are to be called single frustules, which are now regarded as halves or single valves, are often much more frequent than the entire, or, as I should say, double frustules. We never see the halves of Naviculæ separated in this way. *Secondly*, Each single valve, or as I should say frustule, has three nodules; but the entire or double frustules have six. *Thirdly*, In Amphoræ, the two halves of the double or entire frustule are almost always separated by a rectangular space, apparently homologous with the connecting membrane, which is seen in other genera when in a state of self-division. When this is absent, as in fig. 81 *e*, or in fig. 76, the form has exactly the appearance of a twin frustule of Cymbella. *Lastly*, If we regard the so-called valves as entire frustules, they become perfectly analogous, in general form and structure, to Cymbellæ or Cocconemata; and we shall find, in the remarkable group of Complex Amphoræ, next to be described, that the segments in many species, indeed in most, have a still greater resemblance to Cymbellæ or Cocconemata.

Let us now consider that group.

B. *Complex Amphoræ.*

Of this sub-group, as I have already stated, there are, in these materials, as many as of the simple group just described. These species are as follows:—

82. Amphora lyrata, n. sp.
83. ... Milesiana, n. sp.
84. ... elongata, n. sp.
85. ... quadrata, n. sp.
86. ... excisa, n. sp.
87. ... nobilis, n. sp., G.
88. ... Arcus, Greg.
89. ... Grevilliana, Greg.
90. ... complexa, n. sp.
91. ... fasciata, n. sp.
92. Amphora sulcata, Breb.
93. ... acuta, n. sp.
94. ... crassa, Greg.
95. ... pusilla, n. sp.
96. ... granulata, n. sp.
97. ... cymbifera, n. sp.
98. ... proboscidea, n. sp.
99. ... costata, Sm.
100. ... bacillaris, n. sp.

The first species of this group which was figured was *A. costata*, Sm. But the remarkable peculiarity of its structure was not specially noticed. I subsequently noticed several species in the Glenshira Sand, in which the complex structure had attracted my attention, such as *A. Arcus*, *A. Grevilliana*, and *A. crassa*, which I described and figured. I also pointed out that the same structure occurs in *A. costata*, Sm. In the new materials, besides acquiring a more accurate knowledge of the three new species just named, all of which I here figure a second

time, I have found a considerable number of new species, exhibiting the same peculiarities of structure.

The general character of all these species is, that the frustule is very convex, and formed, in the first place, of two segments or valves, in shape like Cymbellæ or the halves of simple Amphoræ, placed opposite each other, as in the simple species. The middle space between these is, in the next place, covered, to a greater or less extent, by a complex mass, like that seen in *Amphiprora complexa*, (fig. 61), and exhibiting, when the two lateral segments lie flat, a series of convergent longitudinal bars, which are the backs of other segments, grouped like those of an orange or a melon. The lateral segments exhibit all the characters of the halves of a simple Amphora; they have the inner curve lines and the nodules, in some cases elongated so as to form a cross bar. The segments forming the central mass appear to have neither curve lines, nor nodules, nor bars. The lateral segments are transversely striated, and the bars of the central mass, or backs of the central segments, are frequently also marked by transverse striæ; but in other cases have much coarser markings, approaching more to the nature of puncta or granules. In some species again, while the margins of the lateral segments are very coarsely marked, the backs of the middle segments are much more finely striated. In general, when the frustule lies so, that the two lateral segments are flat or nearly so, we see in one focus the lateral segments with their curve lines and nodules, while in another focus these become invisible, and the convergent bars alone are seen, which in many cases fill up the whole space between the lateral segments, of which only a part, that nearest the outer margin, can then be seen. This is shown in several of the figures.

This structure is so peculiar, that it seems as if it would be desirable to establish a new genus for the reception of these forms. But the form of the segments is so exactly that of the valves (or frustules) of simple Amphoræ, and even the entire complex forms, in a certain focus, are so like the others, that I think it will be sufficient to make a sub-genus for the complex forms.

I have already mentioned that some of these forms occur in the Glenshira Sand; others, as well as these, are found in the dredgings from Lamlash Bay. But by far the larger part of the complex species here described are to be found in that one Loch Fine dredging spoken of under *A. turgida*, as being rich in *Amphoræ*. Of the above list, thirteen species at least, along with *A. costata*, besides fourteen or fifteen of the simple group, occur in this one dredging, and several of these are found in it alone. It will be unnecessary to repeat this in detail for each species; and I shall briefly refer to Loch Fine as the locality of the species here alluded to as occurring in that particular dredging.

82. *Amphora lyrata*, n. sp. Pl. XIII., fig. 82. Form doubly lyrate, with truncate ends, and a notch in the middle, on each side, where the two lyrate halves meet base to base. The lateral segments have each a strong bar or elon-

gated nodule in the middle. Bars between the lateral segments four or five. Length 0·0011″; breadth 0·00075″. The whole form is transversely striated; striæ distinct, about 36 in 0·001″.

This pretty little form is not very rare in the Loch Fine dredging above mentioned, as containing most of the complex *Amphoræ*. I have seen it also in Lamlash Bay.

83. *Amphora Milesiana*, n. sp. Pl. XIII., fig. 83. Form nearly rectangular, with a constriction in the middle, and subtruncate ends. The sides are very slightly convex. There are several longitudinal bars between the lateral segments. Length 0·0023″; breadth 0·001″. The whole is transversely striated; striæ about 28 in 0·001″, conspicuous.

This species occurs with the last, both in Loch Fine and in Lamlash Bay.

84. *Amphora elongata*, n. sp. Pl. XIII., fig. 84. Form elliptic lanceolate; long, narrow, with truncate extremities. Length 0·0044″; breadth 0·0011″; Lateral segments very narrow; curve lines very near the outer margin. There are six bars in the middle, convergent on the ends. Striæ conspicuous, transverse, about 26 in 0·001″.

This species occurs both in Lamlash Bay and in Loch Fine. It is not frequent, nor yet very scarce.

85. *Amphora quadrata*, n. sp. Pl. XIII., fig. 85. Form nearly rectangular; the sides slightly convex, broad, ends truncated, as in *A. truncata*. Length about 0·0027″; breadth about 0·0018″. Lateral segments arcuate on the dorsal, straight on the ventral margin. Between them are about six broad vertical striated bars, which are not, as in the preceding forms, in apposition, but which are somewhat distant, being separated by narrow *raphes*. The structure, therefore, differs in some respects from that of the other complex species. The lateral segments and the vertical bars are transversely striated; striæ about 34 in 0·001″.

This species occurs both in Lamlash Bay and in Loch Fine, and is not very rare.

86. *Amphora excisa*, n. sp. Pl. XIII., fig. 86. Form rectangular. The curve line in each lateral segment keeps near the outer margin, except in the middle, where it bends inward to a nodule; but outside of this nodule is another, larger and more conspicuous, situated on the outer margin, which appears deeply notched at this point. Length 0·0025″ to 0·004″; breadth 0·0015″ to 0·0018″. There are a number of longitudinal bars, convergent at the ends. The whole form is hyaline, and marked with very fine transverse striæ, which are best seen, though with difficulty, at the outer margin. Striæ about 52 in 0·001″.

This fine species occurs in Lamlash Bay, where it is not unfrequent. It is also found more sparingly in Loch Fine.

87. *Amphora nobilis*, n. sp. Pl. XIII., fig. 87. Form barrel-shaped, broad, ends truncate. Length from 0·003″ to 0·0045″; breadth, 0·0013 to 0·0028″. La-

teral segments arcuate, ends acute, inner curve-lines strongly curved, nodule elongated into a transverse bar. The converging bars in the middle are numerous, in apposition, and the whole frustule very hyaline. It is marked by fine transverse striæ, easily seen on the lateral segments, and which may also be traced, by careful manipulation, across the whole frustule. Striæ about 40 in 0·001″.

I first observed this fine and conspicuous form in the Glenshira Sand, but postponed the description of it that I might examine it farther. I have found it more frequently in Lamlash Bay and in Loch Fine.

88. *Amphora Arcus*, Greg. Pl. XIII., fig. 88. Form of entire frustule barrel-shaped, ends truncate. Segments arcuate, sub-apiculate, marked by coarse moniliform striæ, which are also seen over the entire form. Length from 0·0035″ to 0·0045″; breadth about 0·002.″ Striæ 16 or 18 in 0·001″. The longitudinal bars in the middle between the lateral segments are about 16 or 17, closely set, and we can see that they are segments seen edgewise.

This fine form was rather frequent in the Glenshira Sand, but was only then known in the shape of detached segments, two of which I figured in my first paper on that deposit (*Mic. Jour.*, vol. iii., pl. iv., fig. 4), without at that time understanding its real structure. Subsequently I recognised the entire frustule in the Glenshira Sand, and also in the dredgings from Lamlash Bay. In my third paper I figured an imperfect specimen (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 37), and I now give a more perfect one, which shows that it really belongs to the complex group. See fig. 88.

89. *Amphora Grevilliana*, Greg. Pl. XIII., fig. 89. Form of entire frustule nearly oval, broad, slightly truncate at the extremities, which are broad, the line joining the apices of the lateral segments being concave, so that a short process stands out at each side. Length about 0·006″; breadth about 0·002″. In the specimen figured (fig. 89) the form of the lateral segments is perfectly seen, probably because the frustule is viewed from the flat side, as we view an orange cut in half from the cut side. These segments are precisely like that figured in my last plate from the Glenshira Sand (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 36*). They are broad, arcuate, with somewhat acute rostra, and three curve-lines on their surface, which are peculiar to this species. They are marked with strong transverse moniliform striæ. Striæ about 28 in 0·001″. Between the lateral segments are five or six convergent bars, which are the backs of other segments, and which are in apposition, except at the ends, where they diverge a little, from their apices becoming suddenly narrower.

This fine species occurs both in the Glenshira Sand and Lamlash Bay. But the entire frustule given under this name in the plate just referred to (at fig. 36) does not belong to this species, as may be seen at once by comparing it with fig. 89, and by the form of its segments, as seen in that figure. The segment in that plate, fig. 36*, is correct, as above stated.

The form figured in my last plate, at fig. 36, as the entire *A. Grevilliana*, is a new species, to which I now proceed.

90. *Amphora fasciata*, n. sp. Pl. XIII., fig. 90. Form of entire frustule barrel-shaped, that is, gently convex on the sides, broadly truncate on the ends. The form of the lateral segments, as well as that of the inner curve-lines, is well seen at the sides. Length about 0·005″; greatest breadth about 0·0023″. Between the lateral segments are seven or eight converging bars, separated by very narrow raphes; these are the backs of other segments. The detached segments are arcuate on the dorsal margin, nearly straight on the ventral margin, with acute ends. They have only one curve-line, bending forward in the middle to the nodule, which is near the ventral margin. Striæ about 34 in 0·001″, conspicuous.

This fine and conspicuous form is not unfrequent in the Glenshira Sand, and occurs also sparingly in Lamlash Bay and in Loch Fine. Unlike the preceding one, it is almost always entire.

91. *Amphora complexa*, n. sp. Pl. XIII., fig. 91. Form of entire frustule rectangular, with rounded extremities. Length about 0·005″; breadth about 0·0021.″ Lateral segments narrow, with the dorsal margin straight, except near the apices, where it bends inwards. Inner line strongly curved to the nodule, which lies close to the ventral margin. Between the lateral segments are seven longitudinal converging bars, the backs of other segments, which meet on the convex ends, entirely filling them up. These bars are separated by raphes, rather broader than in the preceding species. The whole form is marked by transverse striæ, which are strong and conspicuous; about 30 in 0·001″.

This very fine and conspicuous form, which is evidently nearly related to the two which precede it, *A. Grevilliana* and *A. fasciata*, while yet all these are distinct, as is seen by the figures, occurs, like the others, both in the Glenshira Sand and in Lamlash Bay, and is not very rare. For some time I confounded all these together in the Glenshira Sand, and it was only a careful examination, especially of the detached segments, which showed me that they were really different species.

92. *Amphora sulcata*, Bréb., n. sp. Pl. XIII., figs. 92 and 92 *b*. Form barrel-shaped, rather broad, ends truncate. Length of frustule 0·0041″; breadth 0·002″. Lateral segments broad, arcuate; inner lines strongly curved, nodule near ventral margin. Between the lateral segments—when viewed in a focus which brings out these segments clearly, as in one of the figures—are seen four or five converging bars. These bars, and all the lines of the lateral segments, are marked by short, transverse striæ, the rest of the surface being hyaline. In another focus, the lateral segments and their curve-lines disappear, and the whole width is taken up by seven or eight converging bars, which are now separated by very narrow lines of transverse striæ. The whole form is very hyaline and very convex. It

is transversely striated; but the striæ cannot be traced throughout without altering the focus. Striæ about 38 in 0·001″.

This remarkable form was first described by De Brebisson as occurring near Cherbourg. No form is better adapted to show the structure of a complex Amphora, on account of its transparency, and the breadth of the convergent bars. I have found it in Lamlash Bay, where it is not very frequent; and also sparingly in Loch Fine. No detached segments have yet occurred.

93. *Amphora acuta*, n. sp, Pl. XIV., figs. 93 and 93 *b*. Form of detached segment arcuate, dorsal margin convex; in some examples with a slight inflection just before the extremities; ventral margin straight, or slightly concave, ends acute. The inner lines are very near the ventral margin, and almost exactly parallel to it, but sometimes a little incurved, except in the middle, where the nodule meets them. Nodule elongated into a strong transverse bar. Length of segment 0·0035″ to 0·0055″; breadth of it 0·00075″. The segments are marked by transverse striæ, about 36 in 0·001″, which are distinctly moniliform.

I have not yet seen the entire frustule; but it is no doubt complex, for I found a good many half-frustules, as it were, formed of segments lying one over the other, to the extent of five or six. Sometimes no cross bar is seen, probably because the cross bar is only found on the lateral segments, which may have become detached, and left the middle ones by themselves. One figure shows a group or pack of segments.

This species occurs in Lamlash Bay, but is more frequent in Loch Fine. It is probable that the entire frustule will somewhat resemble *A. nobilis* in form, but not in its hyaline aspect. On comparing the segments of *A. acuta* with those of *A. nobilis*, as seen in the entire frustule, the curve lines in the latter are seen to be very deeply curved, and to be much farther from the inner margin of the segment, whereas the inner lines in the segments of *A. acuta* are straight, or very nearly so, and close to the ventral margin. In *A. acuta*, the striæ, though not coarse, are strongly moniliform, while the striæ of *A. nobilis* are much finer. Yet it is probable that these two species are related.

(I have very recently observed two specimens, apparently of *A. acuta*, in which two segments are placed opposite, and close to each other. I suppose this view to represent the flat side of the frustule, or the half frustule, like a cut orange, as in the figure of *A. Grevilliana*, fig. 89. But in these specimens of *A. acuta*, the two lateral segments are in apposition.)

94. *Amphora crassa*, Greg. Pl. XIV., figs. 94, 94 *b*, 94 *c*, 94 *d*. Form of frustule rectangular, broad, with rounded ends. Length from 0·0025″ to 0·004″; breadth from 0·0008″ to 0·0013″. Lateral segments linear, from 0·0005″ to 0·00075″ in breadth, straight, or very slightly incurved on the dorsal margin, which, at the apices, bends inwards, forming short, rounded beaks. Sometimes, as in one

of the figures, the dorsal margin is convex. Ventral margin of segments undulated. The inner curve lines arise from the point of the beaks, run a little outwards, then inwards to the nodule, placed very near the ventral margin. Markings entire, coarse, subdistant. Striæ about 12 in 0·001″. Between the lateral segments are from five to eight convergent bars, marked with the same subdistant, entire striæ. In one focus, not here figured, nothing is seen but bars from one side to the other, which are thus eight or nine in number.

This very pretty and interesting species occurs in the Glenshira Sand, but the figure given of it in my last plate (*Trans. Mic. Soc.*, vol. v., pl. i., fig. 35), is not, at all events, the usual form. In that figure the markings are too close, and it may possibly represent a different species. Indeed, I have some reason to think that there are two species which resemble each other in several points. But I have not yet ascertained this to be the case. I have found the form now figured, which is the true *A. crassa*, more frequent in Lamlash Bay, where, also, I have observed the detached segments, previously unknown to me, and which, as may be seen, are very peculiar. It occurs also in Loch Fine, though less frequently.

95. *Amphora pusilla*, n. sp. Pl. XIV., figs. 95 and 95 *b*. Form linear, with rounded ends. Length from 0·0014″ to 0·0021″; breadth 0·0004″ to 0·0006″. Lateral segments very narrow, dorsal margin straight, except at the ends, nodule not far from dorsal margin, inner curve line also near it; striation conspicuous. Between the lateral segments are five or six narrow bars, separated by very fine sharp lines, and marked by subdistant granules or very short striæ. Striæ about 24 in 0·001″, very strong at the margins of the frustule.

This little form is not very rare in the Loch Fine dredging, so often referred to; and occurs also, though more sparingly, in Lamlash Bay. In general aspect, it resembles a delicate miniature copy of the preceding species; but the form of the segments, the curve lines, and the striation, are all totally distinct.

96. *Amphora granulata*, n. sp. Pl. XIV., figs. 96; 96 *b*; 96 *c*; 96 *d*; 96 *e*; and 96 *f*. Form of frustule linear, broad, with slightly convex sides, and truncate extremities. Length from 0·0017″ to 0·003″; breadth 0·0008″ to 0·0013″. Lateral segments slightly arcuate on the dorsal margin, which is suddenly narrowed at the ends; ventral margin quite straight, or slightly concave, apices sub-acute. Segments marked with fine transverse striæ, which are parallel, and from 24 to 36 in 0·001″. Fig. 96 *f*, represents a detached segment. When the frustule is so focussed that the lateral segments are distinctly seen, and their striæ plainly resolved, the space between appears nearly blank. But in another focus, the whole frustule is seen to be made up of about twelve longitudinal bars convergent on the ends, the backs of which are marked by subdistant granules, from 14 to 18 in 0·001″. Hence the name. I have figured these two views of each of two frustules, one short and broad; the other longer and narrower. One figure, fig. 95 *e*, represents a somewhat larger form, which also exhibits granulated bars,

and lateral segments approaching in form to those of *A. granulata*. But on careful comparison, it seems very doubtful whether this form do not belong to a different species. As I have not yet had time to ascertain this precisely, I give it here *cum nota*.

A. granulata is tolerably frequent in the Loch Fine dredging I have named as being so rich in species of *Amphora*, especially of complex *Amphoræ*. I would observe that in this form the striation of the lateral segments is finer than that on the middle bars, whereas in the next species the reverse is the case. *A. granulata* sometimes attains a considerably larger size than in the two undoubted specimens here figured.

97. *Amphora cymbifera*, n. sp. Pl. XIV., figs. 97, 97 *b*, 97 *c*. Form of frustule elliptic, rather broad, with very short, produced, and truncate apices. Length 0·0025″ to 0·0045″; breadth in the middle, 0·0012 to 0·0016″. Lateral segments highly arcuate on the dorsal, almost straight on the ventral margin, the former being suddenly contracted at the ends, so as to produce round heads, with very short necks; thus the segments are capitate. Their form is elongated, and the curve regular and graceful. They are marked by somewhat coarse striæ, slightly inclined. Striæ about 22 in 0·001″. The nodules are on the inner margin, or just within it, and the inner lines are parallel to that margin, and close to it. The segments, when detached, as is seen in one of the figures, exactly resemble the frustules of an elegant Cymbella. Between the lateral segments lie from five to seven convergent bars, and, in one focus, the whole frustule is seen to be made up of these bars (fig. 96 *b*), which are marked by fine transverse striæ, considerably finer than those on the lateral segments, which became stronger and coarser near the margin, as may be seen by the figures. The bars, as in *A. sulcata* (fig. 91), appear to be separated by furrows, and in a certain focus these furrows may be seen marked by lines of short transverse striæ. Fig. 96 is the same frustule as that in 96 *b*, focussed so as to show the lateral segments. Fig. 96 *c*, is a detached segment.

This fine form is not unfrequent in the Loch Fine dredging above mentioned. The views of it are so different, according to its position, and the detached segments are so like *Cymbellæ*, that it was some time before I could see my way among these forms, especially mixed as they were with frustules and segments of the preceding, as well as of the next species, which have a similar structure.

98. *Amphora proboscidea*, n. sp. Pl. XIV., figs. 98; 98 *b*; 98 *c*; 98 *d*. Form of frustule nearly rectangular in the middle, contracted near the ends to truncate extremities. Length from 0·003″ to 0·005″; breadth from 0·001″ to 0·0015″. The longer specimens are narrower than those of middling length. Lateral segments arcuate on the dorsal, often slightly convex, or undulated, on the ventral surface, contracted at the ends so as to be capitate, the heads having longer necks than in the preceding species, which are bent forward at a very obtuse angle,

giving to the detached segment a very peculiar character. The segments are marked by strong, coarse striæ, about 20 in 0·001″. In one focus (fig. 97) the segments are well seen in the frustule; in another, fig. 97 *b*, the frustule is seen to consist of 9 or 10 convergent bars, which are coarsely granulate. In fig. 97 the lateral segments are seen to be nearly in apposition, with a narrow space between them, of varying width, from the undulations of the ventral margin. The inner line of each lateral segment is very slightly curved; the nodule lies nearest to the ventral margin. The detached segments, figs. 97 *c*, and 97 *d*, are precisely like Cymbellæ, and for a long time I considered them as such, with those of *A. granulata* and *A. cymbifera*. But the view in fig. 97 showed the real nature of these forms. Want of space alone prevents me from giving figures of the entire frustule corresponding to the segment in fig. 97 *d*. The reader is requested to compare fig. 97 with the corresponding view of *A. Grevilliana*, fig. 89.

This very striking species occurs only in the stony Loch Fine dredging, so often alluded to, where it is rather frequent, both entire and in detached segments.

99. *Amphora costata*, Sm. Pl. XIV., fig. 99. This species is described by Professor Smith in his *Synopsis*, vol. i., where the entire frustule, and also a half frustule, are figured. But little is said of its peculiar structure, and the detached segment is not figured. For this reason, and to show the analogy between this species and the three preceding ones, I have figured a detached segment (fig. 99). Form of segment highly arcuate, very broad in the middle. Ventral margin straight, or slightly concave, but often, as in the example figured, which is a rather small one, with a rounded prominence in the middle close to the nodule. The ends are capitate. Striæ coarse, conspicuous, about 14 or 16 in 0·001″, thicker and stronger near the dorsal margin. Length from 0·002″ to 0·0033″; breadth in the middle 0·0012″ to 0·0016″.

It will be seen that in this species also the segment resembles a Cymbella, although it is a very broad and highly arcuate one. When the segments are united, as in the entire frustule, it is not easy to see their real characters. The backs of these segments, or longitudinal bars, are, as in Professor Smith's accurate figures, marked by very coarse distant granules, which give no indication of the peculiar striation of the detached segments. Hence it was very long before I was able to detect the component parts of the frustule when detached, or to refer the form shown in fig. 99 to *A. costata*. But some specimens, as in the preceding species, when carefully focussed, clearly show their true nature.

100. *Amphora bacillaris*, n. sp. Pl. XIV., figs. 100 and 100 *b*. Form of frustule linear, narrow, with somewhat rounded ends, which are subacute. Length about 0·0017″; breadth about 0·0003″. In one focus it exhibits two lateral portions separated by a middle space, the sides of which are perfectly straight, the ends beautifully rounded. In another, the whole frustule is seen to be composed of very narrow bars, separated by very sharp lines, converging on the ends, and

marked with small granules. Striation transverse, fine; number of striæ not counted, but they are much finer than in *A. pusilla*. The detached segments are not yet known; but, as seen in fig. 98, the segments appear to be very narrow, and linear in form, the dorsal margin being hardly convex. The inner curve-lines and nodules are obscure. These characters, as well as the finer striations, the finer granulation of the bars in fig. 100 *b*, and the peculiar form of the middle space in fig. 100, sufficiently distinguish it from *A. pusilla*, the only form it resembles.

This species occurs in the same Loch Fine dredging with those which immediately precede.

The numerous examples here given of complex Amphoræ, to which, as we have seen, two have been added from the simple group, prove that this group of forms is by no means a small one, since so many have been obtained in one locality. It is worthy of remark, that the same dredging, which has yielded at least 12 or 13 of the forms just described, also contains *A. costata*, Sm., already alluded to as the first Complex Amphora ever figured in this country, though the peculiarities of its structure had not been fully appreciated. In fact, as we have seen *A. Grevilliana*, *A. complexa* and *A. fascata* to form a smaller group of closely allied species, so *A. granulata*, *A. proboscidea*, and *A. cymbifera* also form another such group, to which *A. costata* also belongs. It would almost seem as if the locality were favourable to these complex forms: for on the waters of the Clyde the whole of them occur. We have also in these waters four *Amphiproræ*, with the remarkable additions of plates lying on the valves, namely, *Apr. pusilla*, *Apr. lepidopteræ*, *Apr. plicata*, and *Apr. maxima;* and lastly we have the doubtfully named *Apr. complexa*, which exhibits the same complex structure in its middle portion as we find in so many species of Amphora, that, namely, of segments packed together, and converging on the ends, like those of an orange or melon. But we must also remember that the same locality is equally rich in new forms of simple Amphoræ.

GROUP VII.

Miscellaneous.

In this last group I shall describe a few forms of genera not yet named in this communication, and among them one or two whose real nature is doubtful. These are:—

101. Navicula (?) Libellus, n. sp.
102. Nitzschia (?) panduriformis, n. sp.
103. Nitzschia distans, n. sp., G.
104. ... hyalina, n. sp.
105. Pleurosigma (?) reversum, n. sp.
106. Sceptroneis Caduceus, Ehr.
107. Synedra undulata, Greg.
Toxarium undulatum, Bail.
108. Synedra Hennedyana, n. sp. (?)

and, as an Appendix,

109. Creswellia Turris, n. sp., (Arnott).

101. *Navicula* (?) *Libellus*, n. sp. Pl. XIV., figs. 101, and 101 *b*. Form of F.V. rectangular, broad, with the angles rounded. The middle part is marked by longitudinal lines or folds, like the leaves of a book; and when the two halves of the F.V. separate, each retains a broad band of this lineate part. The breadth of the detached halves on the F.V. is so great, that, when united, they must, it would seem, mutually overlap, otherwise the resulting frustule would be much broader than it is. S.V. rhombic or elliptic-lanceolate, broad, with obtuse ends. Length from 0·003″ to 0·0035″; breadth of F.V. 0·0017″ to 0·002″. The S.V. is marked by very fine transverse striæ; striæ about 60 in 0·001″; median line distinct; nodule definite. When the edge of the S.V. is seen, as in fig. 101 *b*, the valve seems to be a compound one, formed of five or six, closely packed one over the other. I cannot ascertain if this be so or not.

This species is frequent in Lamlash Bay, and it much resembles the form figured by me in my second plate from the Glenshira Sand, under the name of *N. rhombica*, of which also I had figured two of the S.V. in my first plate (*Trans. Mic. Soc.*, vol. iv., pl. v., fig. 1, and *Mic. Jour.*, vol. iii., pl. iv., fig. 16). But I observe several uniform points of difference. *N. Libellus* is more obtuse and broader, and its striation is not only much finer, but the striæ are everywhere of uniform size and at a uniform distance; whereas in *N. rhombica*, they are near the middle of the valve, not only stronger, but so much more distant than in the rest of the valve as to be almost conspicuous. *N. Libellus* is also, on the whole, a larger form than *N. rhombica*.

But it is very doubtful whether either of them be really a Navicula. They have some resemblance, especially on the F.V., to *Schizonema Grevillii*, Sm., which, however, is a much smaller form. Still they may possibly belong to Schizonema, but this cannot be ascertained except in living, or at least quite recent and uninjured specimens. The F.V., with its foliated or complex structure, appears to me, however, to differ from that of a Schizonema.

I may here add, that there occurs in Lamlash Bay a much smaller form of the same shape, but not foliated, at least not distinctly so. This is perhaps the true *S. Grevillii*.

102. *Nitzschia* (?) *panduriformis*, n. sp. Pl. XIV., fig. 102. Form linear, broad, incurved in the middle, acuminate at the ends, which are usually obtuse and rounded, but sometimes acute and sub-apiculate. Length about 0·003″; breadth in the middle 0·001″; at the shoulders 0·0012″. The specimen here figured is longer than usual, and the only one I have seen of this length. Margin punctate. There is a faint indication of a double keel in the middle of the valve. Striation fine, both transverse and oblique; striæ about 48 in 0·001″.

This species occurs in several of the Loch Fine dredgings, and is not rare. The striation is similar to that of *Tryblionella constricta*, Grig. (*Mic. Jour.*, vol. iii., pl. iv., fig. 13); but the present form is much larger, and is distinguished

by the marginal puncta. Still it resembles a Tryblionella about as much as it does a Nitzschia, and I therefore give it with a query as to the genus.

103. *Nitzschia distans*, n. sp. Pl. XIV., figs. 103 and 103 *b*. Form of F.V. nearly rectangular; margin punctate, the puncta being very distant, some in pairs, others single, 5 or 6 in 0·001″. The punctate margin bends slightly inwards to each end, so that the ends would be narrower than the middle but for two small hyaline expansions at each end, which renders the extremities a very little broader than the middle. Length about 0·0058″; breadth about 0·001″. S.V. linear-lanceolate or rhombic-linear, narrow, broadest in the middle, where the breadth is 0·0005″; ends acute, keel central. The whole form is somewhat hyaline.

This species is not rare in the Glenshira Sand, and I was only prevented from figuring it in my last paper on that deposit by want of room for the figure. Since then I have found it frequent both in Lamlash Bay and in the remarkable stony dredging from Loch Fine, so often mentioned, from which the present figure is taken. It is probably striated, but I have not been able to resolve the striation.

I may here state that the form, of which a drawing was made, but not inserted in the plate just alluded to, seems to differ in some points from this, which is the more frequent. In that form, the puncta, though distant, were regular, and, as stated in the description, which was printed without the figure, appeared to be constricted, and to have fine lines proceeding from one constricted punctum to the constriction in the next. As the F.V. of this form had not the terminal expansions, a circumstance which at the time I attributed to accident, I am inclined to believe that that figure really represented a different species. This I have not had time to ascertain. But the present figure represents accurately the form which from the first I had named *Ntz. distans*.

104. *Nitzschia hyalina*, n. sp. Pl. XIV., figs. 104 and 104 *b*. Form of F.V. rectangular, with expansions at the extremities. On each margin is a row of small puncta. Length 0·0034″; breadth 0·0004″. S.V. linear, narow, and towards both ends contracted to long and still narrower terminations. Keel apparently double; but perhaps one is seen through the very hyaline valve. The whole form is so hyaline as to be easily overlooked.

This delicate species is tolerably frequent in the Loch Fine dredging so often mentioned. It is possible that it may be a *Homœocladia*, but I have no means at present of deciding this point. It is certainly not one of the species of *H.* figured in the Synopsis.

105. *Pleurosigma* (*?*) *reversum*, n. sp. Pl. XIV., figs. 105 and 105 *b*. Form linear-lanceolate, narrow, somewhat contracted on each side of the middle portion, and again expanding towards the ends, which are elongated, and have the expansion all on one side, but on opposite sides at the two apices. On the non-expanded side of the elongated ends, the margin is nearly straight, or slightly in-

curved. The whole form has a strange appearance, as if we were to take two long, narrow stockings, cut them across at the widest part, and join them at the cut ends, with the feet pointing opposite ways. From this last character I have named it. Length 0·005″ to 0·006″; greatest breadth 0·0006.″ Median line sigmoid, straight in the middle, and suddenly bent near the ends in opposite directions. Striation so fine that I have not yet succeeded in resolving it, and therefore not easily visible under a power of 400 diameters.

This singular form occurs in the stony Loch Fine gathering so often referred to. I have as yet only seen the two specimens here figured, and two more; but I have not searched for it, these being so remarkable, and so like each other, as to indicate sufficiently, in a general way, the existence of the species. I do not feel quite certain as to its genus; but I think it right to direct the attention of observers to it. It will probably be found more abundantly in some dredging or gathering from a different locality in the Clyde.

106. *Sceptroneis Caduceus*, Ehr. Pl. XIV., fig. 106. I cannot enter into a detailed description of this species, as the fragment here figured is the only specimen of it I have yet seen in these dredgings, or in any British gathering. And I figure it chiefly as evidence that this genus, which is frequent in several American fossil deposits, yet lives in our waters, although we have yet to find it in such abundance as will probably occur near its true habitat. Ehrenberg thus describes the genus (*Bericht der Berlener Akademie*, 1844, p. 264), "Animal e Bacillariis Echmelleis, affixum? Lorica simplex æqualiter bivalvis silicea stiliformis compressa, nonconcatenale, cuneata (viva facile pedicellata). Sutura laterum utrusque valvæ longitudinalis media, umbilicus nullus. Habitus *Meridii* non concatenati aut *Gomphonematis*, umbilico laterali carentis."

The species, *S. Caduceus*, is distinguished by its long slender form, having a central expansion, and another at one end, while the other end is long and narrow, and by its very coarse moniliform striæ. In this fragment we have the large end, which is unusually large, for it is commonly of a narrower and somewhat elliptical shape.

This form, which adds one to the list of British genera, occurs in the same Loch Fine dredging as the preceding one, and so many more.

107. *Synedra undulata*, Greg. *Toxarium undulatum*, Bail. Pl. XIV., figs. 107 and 107 *b*. Form of frustule very long, and very slender. F.V. rectangular, very narrow; S.V. with an elongated central expansion, and two small semi-elliptic terminal ones. Margin undulated. Striæ conspicuous, moniliform, in the expansions passing, towards the middle, into an indiscriminate punctation. Length 0·023″; greatest breadth of S.V. 0·00035″; breadth of the longer and narrower portions hardly 0·0001.″ So that the length of the frustule is about 70 times the width of the broadest part of the S.V., and more than 200 times that of the greater part of the valve.

Professor Smith describes the S.V. as arcuate, as in fig. 107 *b*; but I find it very often quite straight, as in fig. 107. The arcuation seems to be accidental, due only to the great slenderness of the frustule, and therefore common; but it is most probably naturally straight in the S.V. as well as on the F.V. Professor Bailey represents it as straight, although he figures a specimen of the enormous length of 0·0265″. Those which are not straight are bent quite unequally, some very little, others considerably, others only at one end, and others more at one end than the other. I feel pretty sure, therefore, especially as straight examples are frequent, that it is not essentially an arcuate form.

This very remarkable species, the longest known Diatom, except a *Chætoceros*, figured along with it by Bailey, which is as long, was first observed in this country, by me, in the Glenshira Sand, in which, however, I could not find, among some hundred specimens, one entire frustule. I figured three fragments, two of them nearly complete, in my first paper on the Sand (*Mic. Jour.*, vol. iii., pl. iv., fig. 23), and was able to calculate, that if entire, its length would be about one-fiftieth or one fortieth of an inch, or 0·02″ to 0·025″. The length of the specimen here figured lies between these measurements, that of Professor Bailey's figure is a little above the highest of them. After my paper with the incomplete figures was published, I became acquainted with the earlier observations of Professor Bailey, who had found it living on *Sargassum* on the American coast. I found one specimen of it also recent, but still fractured, before my paper was printed, in a gathering made by Professor Smith on the south coast. Subsequently, Professor Smith found it frequent in Cork harbour, though smaller than in America. Last year (1856) I found it frequent in Professor Allman's Lamlash Bay dredging, and sparingly in the other dredgings. As no entire figure of it has yet appeared in this country, I have here given two figures, one arcuate, the other straight.

108. *Synedra Hennedyana*, n. sp (?) Pl. XIV., fig. 108. This form is in all respects similar to the preceding, except that the margin is not undulated. Fig. 108 represents it of the same length as *S. undulata*.

I first noticed this form along with *S. undulata*, in July 1856, in Professor Allman's Lamlash Bay dredging, but I considered it as simply a variety of that species. I was led to do so by observing that in *S. undulata* it often happens that a considerable portion of the margin is devoid of undulations. But several other observers who had seen it, adopted the opinion that it was distinct from *S. undulata*. Mr Roper was, I believe, one of these; and I rather think Professor Walker-Arnott, and Mr Hennedy have come to the same conclusion. Professor Arnott informs me that it occurs in a gathering from the Clyde, I believe near Cumbrae, without a single frustule of *S. undulata*. As this gathering was made by Mr Hennedy, if I am not mistaken, and as he has at all events studied the form in question, I have figured it under his name, with a mark of doubt, as I am not yet quite satisfied that it is really a distinct species. In my material it

is mixed with *S. undulata;* and I know of no distinction beyond that of the absence of undulations on the margin, unless it be that the striæ in *S. Hennedyana* are perhaps a little finer than in *S. undulata.* Even of this I am not sure. But the figures, which are very accurate, will enable the reader to form his own conclusions.

Such are the results obtained, up to this time, by the examination of these 11 gatherings from the Firth of Clyde and Loch Fine, 10 of which are true dredgings, while the 11th is derived from *Corallina officinalis,* to which a good many Diatoms have adhered.

From the remarkable analogy between the Glenshira Sand and these gatherings, we may regard it simply as another dredging, the marine forms in which have been derived from Loch Fine. I have shown that the period at which it was deposited has not caused any material difference of composition, and that we may say, in general, that it does not differ more from the recent dredgings than they do from each other.

Considering, then, all as supplying us with existing forms, we are struck with the unexpectedly large number of undescribed species which this exploration of the waters of the Clyde, though very limited in the area whence the materials were derived, has yielded in a short space of time.

It is worthy of notice, that the great majority of these new forms are not only new as British species, but have not been observed elsewhere, although Ehrenberg and Bailey have both described many rich marine gatherings from different parts of the world.

This proves that the existing stores of marine Diatoms have not yet been by any means fully explored. It is therefore highly desirable that dredgings or soundings from all seas and estuaries, and from every part of them, should be procured and carefully searched. From what has been already recorded, as well as from the results here given, it appears that estuaries and harbours, or other localities near the coast, are likely to be the richest in Diatoms, perhaps from the comparative shallowness of the water. But the conditions of the distribution of these organisms in the sea, and of the accumulation of their indestructible siliceous shells, are not yet known with certainty. Thus, while every one of these Clyde dredgings proved more or less rich in Diatoms, I have found several from the Long Narrows, in the Firth of Forth, kindly given me by Dr Hector, to be very poor in comparison, and indeed not worth the trouble of mounting. And while Bailey has found many interesting forms of this class in soundings from a depth of 1700 fathoms, and even of 2700 fathoms, in the Kamtschatka Sea, a number of Atlantic soundings, from depths varying from 85 to 2000 fathoms, which, by the kindness of Professors W. Thomson and Allen Thomson, I was allowed to see, contain indeed Foraminifera and Polycystineæ, but are almost entirely destitute of Diatomaceæ. Yet Bailey has found Diatoms in Atlantic soundings from

other localities. We have nothing for it, therefore, but to examine every specimen of sea-bottom that we can procure. And the example of the Firth of Clyde is sufficient to prove that much remains to be done.

It should also be stated here, that these Clyde dredgings are not exhausted. Indeed, it is a work both of much time and much labour fully to exhaust any such mixtures as these are.

While these sheets are passing through the press, I am in a position to state, that I have already collected, from the same materials, so considerable a number of additional undescribed forms, that it will be necessary to describe and figure them in a supplementary memoir. Of these forms, a large proportion are discs, many of which are small, or only of a medium size; but there are also Naviculoid forms, Amphoræ, and forms of a few other genera.

I would further direct attention to the fact, that these dredgings differ materially from each other, each being characterized by the prevalence of certain forms, although some forms are common to all. Thus, off Inveraray and Strachur, in Loch Fine, the proportion of large Campylodisci was very much greater in two gatherings than in all the rest, whether there or off Arran; while in Lamlash Bay, the material was remarkable for the great number and variety of Amphoræ, a character found in one only out of the seven dredgings from Loch Fine. This shows that the deposits may vary much in regard to species, and even genera, in localities at no great distance from each other, and points out the advisability of searching every corner.

Lastly, it appears probable that some genera, whether such as have been adopted by Ehrenberg, Kützing, Bailey, and others, or entirely new, will have to be added to Professor Smith's list of British genera. This is especially the case with the numerous new filamentous forms, hardly any of which agree with the genera in the Synopsis. I have not for the present ventured to introduce any entirely new genus, but I have added *Pyxidicula* and *Sceptroneis* of Ehrenberg, and, more doubtfully, *Diadesmis*, also admitted, in a recent paper, by Professor Smith. I refrain from doing more; because I believe that genera established in the present imperfect state of our knowledge of species as well as of genera, are not likely to be permanent. In one case, I have pointed out the possibility of uniting in one genus and in one species three forms, *Campylodiscus simulans*, *Surirella fastuosa*, and *Surirella lata*, at present referred to two genera and three species.

In distinguishing and describing the very numerous new forms figured in this communication, I have been careful to avoid unnecessary multiplication of species. In numerous cases I have united forms apparently distinct which a closer examination showed not to be so. And in every case where I have admitted a new species, it has been because I could not reconcile it with any figures or descriptions which were accessible to me. I have also had the great advantage of frequent consultation with Dr Greville, whose opinion has deservedly

very great weight with all students of the Diatomaceæ. I have further to thank Mr Roper for many useful hints, and for the use of some very accurate drawings of forms observed by him, in many cases identical with those I had myself described.

It is impossible to do full justice to the scrupulous accuracy and to the artistic beauty of the figures which Dr Greville has made of the forms which I have described, and to the signal success with which Mr Tuffen West has engraved them. I can only say that I have seen no figures of this kind equal to them in these respects, and that the chief value of communications like the present is derived from the presence of good figures. Without figures, descriptions are apt to be misunderstood, and inferior figures tend, more than any other cause, to lead observers to multiply species unnecessarily. Those who are in the habit of studying the Diatomaceæ will agree with me, that a large proportion of the figures in some works on the subject are worse than useless, and lead to hopeless confusion.

There is another point on which good figures throw much needful light. In many species, though by no means in all, the shape, as well as the size of the forms, and even the striation, all vary to a great extent. In such cases, it is most important that every author should figure a sufficient number of selected forms, to show the real extent of the species. These variable species ought to be thus treated individually, by which means many existing species would be got rid of and reduced to a smaller number. I have attempted something of the kind in *Navicula varians* (*Trans. Mic. Soc.*, vol. iii., p. 10), and in this paper I have done so partially in *Navicula Lyra*, *Nav. Smithii*, *Amphora Proteus*, and *Amphora lævis*. Such forms as *N. elliptica*, *N. didyma* (which I have in part illustrated in my last paper on the Glenshira Sand), *N. Crabro* and *P. Pandura*, for example, and even *N. Smithii*, besides others in different genera, require much fuller illustration than they have yet received.

Finally, I wish it to be understood, that in describing so many new species, I make no pretensions to deciding authoritatively on disputed or doubtful points. My sole object is to bring under the notice of observers, the *forms* which I meet with. To do this, I must needs give them names, and in this respect I endeavour to be as accurate as I can. I observe that Professor Smith, in his last paper in the *Annals*, objects to the establishment of new species, unless the specimens are frequent. But although I have given, as distinct species, some forms which are rare, I have not done so till after I had examined and compared many specimens of each, except in one or two cases, such as *Coscinodiscus umbonatus*, where the form is so striking and well-marked that even one specimen suffices.

If we confine our attention to one or two slides, then, indeed, rare forms can not be sufficiently studied. But in the researches made in connection with this paper, I have explored at least 1000 slides, most of them twice, many three times,

and even oftener. Thus it happens, that I have compared as many specimens of by far the greater number of the forms here mentioned as rare, as if they had been very frequent, and I had only seen one or two slides.

Compared with many forms, all the Complex Amphoræ would be considered rare, but of these, I have in every instance examined numerous specimens, and have satisfied myself of the constancy of their characters, which is the most important point.

I trust, therefore, that Naturalists will accept this paper as a simple contribution to our knowledge of Diatomaceous Forms. As such I present it, leaving to those who are better qualified for the task than I am, to decide on the conflicting claims of genera and species.

APPENDIX.

For the following description and figure of a very beautiful new form, belonging to a new genus, I am indebted to my friend Dr Greville. The form in question has not occurred to me as yet, but as Professor Walker-Arnott has found it in the Clyde, it has a claim to be inserted in this account of new Clyde forms.

Notice of a New Genus of Diatomaceæ. By R. K. Greville, LL.D., F.R.S.E., &c.

My friend Professor Gregory having permitted me to introduce in this place the description of a new and most interesting diatomaceous form, I gladly avail myself of the privilege. Having been recently discovered in the Clyde, it may, indeed, be considered as possessing some claim to appear in company with the multitude of fine species described by Professor Gregory in the preceding pages. This remarkable Diatom was communicated to me a few weeks ago, by my friend Professor Walker-Arnott, for publication and illustration. It is to be regretted that he did not undertake this office himself; he has, however, very kindly supplied me with notes of his views regarding it, so that he has rendered my labour comparatively light. Professor Walker-Arnott's attention was first directed to the form in question by the Rev. R. Creswell, who obtained it at Teignmouth, from the stomach of *Cynthia rustica* (*Phallusia rustica*, Flem.), along with *Biddulphia Baileyi* and other good things. It is, however, so scarce, that at present it must be reckoned among the rarissima of its tribe. Very fortunately the specimens which have been obtained are in a state to admit of satisfactory description. Like *Biddulphia* and *Isthmia*, it forms chains, the links or frustules of which are oblong, somewhat depressed at the ends, highly cellulate, separating transversely

into two equal valves. The frustules are united by means of a circle of numerous short, terminal processes of equal length, which ultimately separate in the middle; and the detached frustules then appear furnished at each end with a beautiful coronet, or circle of miniature turrets. This mode of connection is peculiar, although perhaps analogous to that in *Biddulphia*. In the subject of these remarks, however, the connecting processes do not appear to be so distinctly a continuation of the substance and structure of the body of the frustule, as are the horns of that genus, and must rather be regarded in the light of appendages.

With regard to the affinities of this beautiful little object, we may certainly assume, that if a solitary frustule had alone been formed it would have been referred by KÜTZING at once to the genus *Pyxidicula*. But little or nothing is known of the real nature of the variety of forms brought together under that name. EHRENBERG's own character for the original genus is as follows:—

" Animal e familia Bacillariorum, liberum, lorica simplici, bivalvi (silicea); solitarium, globosum (=Gallionella divisione spontanea perfecta aut nulla). *Die Infusionsthierchen*, p. 165.

But EHRENBERG subsequently constituted other genera or subgenera to receive the accumulating species; and as the work in which they appear is not generally accessible, I do not hesitate to give the characters verbatim in this place.

" *Dictyopyxis*, nov. gen. *Pyxidiculæ* generis ea bivalves subglobosæ aut turgidæ formæ, quæ valvularum testæ strictura simpliciter cellulosa insignis sunt ab iis, quæ continua et simplici membrana silicea includuntur, aut appendicibus variis instructæ sunt, gravius differunt et facillime distinguuntur. Cellulosas igitur in Dictyopyxidis subgenere colligendas senserim. *P. cruciata, Cylindrus, hellenica et Lens* huic subgeneri nunc inscribendæ sunt."—*Ehrenb. Bericht. der Berl. Akad.*, 1844. P. 262.

" *Stephanopyxis*, nov. subgenus. *Pyxidiculæ* generis bivalves turgidæ aut subglobosæ formæ, quæ valvularum testæ structura cellulosa insignes sunt et denticulorum, aculeorum aut membranæ coronam in media quavis valvula gerunt in hoc *Pyxidiculæ* subgenere colliguntur."—*Ehrenb., l. c.*, 1844. P. 264.

" *Xanthopyxis*, nov. subgen. *Pyxidiculæ* subgenus bivalve turgidum subglobosum. Valvularum testæ silicæ continuæ integerrimæ nec cellulosæ, superficie hispida, setosa aut alata."—*Ehrenb., l. c.*, 1844. P. 264.

KÜTZING, in his *Species Algarum* (1849), reunites the whole, giving twenty-two species, all of which, except two, are fossil. The frustules, according to him, are " non concatenata." Mr CRESWELL's Diatom is, therefore, by a most important character, excluded. Taking a simple frustule, and leaving the processes out of view, it much resembles *Dictyopyxis hellenica* (*Ehrenb. Microgeologie*, tab. xx., fig. 32), and also *Stephanopyxis appendiculata* of the same work, tab. xviii., fig. 4; but that species has only a single tooth at each end, and is provided with a sort of narrow zone or annulus. It resembles still more closely *Stephanopyxis apicu-*

lata (*Ehrenb.*, *l. c.*, tab. xix., fig. 13) which is represented with three terminal teeth; but these teeth can scarcely be the remains of a *corona*, as KÜTZING, in defining the frustule, says, "utroque fine medio apiculis elongatis hispido." Upon the whole, the safe course seems to be to regard the subject of this notice as not only specifically but generally new; and I gladly adopt the suggestion of Professor WALKER-ARNOTT, that it receive the name of *Creswellia* in honour of its discoverer, the Rev. R. CRESWELL, a gentleman well known to Algæologists, and to whom Professor HARVEY has already dedicated a new British *Schizothrise*. The following character will distinguish it at once from all its allies.

Creswellia. Frustules cylindrical, two-valved, cohering by short, filiform processes into a continuous filament. Valves cup-like, cellulate, destitute of any siliceous connecting band. Pl. XIV., fig. 109.

This singularly interesting Diatom, which may be called *Creswellia Turris*, has only been found in the locality already mentioned by Mr CRESWELL, and off the Island of Cumbræ, where it was dredged along with the nests of *Lima hians* by Mr HENNEDY, and a single frustule detected by Dr WALKER-ARNOTT.

The figure represents four frustules, the largest number which has been hitherto observed in connection. It will be perceived that in two of the frustules one of the valves is dark, and more or less opaque. This appearance we are quite unable to account for. It sometimes happens that the whole frustule is dark. Generally they are all beautifully clear. The structure is highly cellulate, the cells hexagonal. The length of the frustule is about ·0028″; the breadth about ·0016″.

EXPLANATION OF PLATE IX.

Fig. 1. Navicula minor, n. sp.
— 2. ... Cluthensis, n. sp.
— 3. ... inconspicua, n. sp.
— 4. ... brevis, n. sp.
— 5. ... Claviculus, n. sp., S.V. × 800. 5 *b*, do., F.V. × 800. 5 *c*, do. S.V. × 400.
— 6. ... Musca, n. sp.
— 7. ... rectangulata, n. sp.
— 8. ... nebulosa, n. sp.
— 9. ... Barclayana, n. sp.
— 10. ... spectabilis, n. sp.
— 11. ... praetexta, Ehr.
— 12. ... Bombus, Ehr.
— 13 & 13 *b*, Lyra, Ehr.
— 14 & 14 *b*, Lyra, var. *β*.
— 15. ... Smithii, var. *β*, fusca.
Fig. 16. Navicula Smithii, var. *γ*, nitescens.
— 17. ... Smithii, var. *δ*, suborbicularis.
— 18 & 18 *b*, maxima, Greg., S.V. and F.V.
— 19. Pinnularia subtilis, n. sp.
— 20. ... rostellata, n. sp.
— 21. ... Allmaniana, n. sp.
— 22. ... Pandura, Bréb., var. *β*. elongata.
— 23. Cocconeis distans, Greg.
— 24. ... ornata, n. sp.
— 25. ... dirupta, n. sp.
— 26. ... nitida, n. sp.
— 27. ... pseudomarginata, n. sp.
— 28. ... major, n. sp.
— 29. ... splendida, n. sp.

All the above, except figs. 5 and 5 *b*, are magnified 400 diameters.

EXPLANATION OF PLATE X.

Fig. 30. Denticula (?) interrupta, n. sp.
— 31. ... (?) capitata, n. sp.
— 32. ... (?) ornata, n. sp.
— 33, 33 *b*, & 33 *c*, ...(?) lævis.
Fig. 34. Denticula nana, n. sp., F.V. 34 *b*; do. S.V.
— 35, 35 *b*, & 35 *c*, minor, n. sp., F.V. 35 *d*; do. S.V.
— 36. ... distans, n. sp., F.V. 36 *b*; do. S.V.
— 37 & 37 *b*, ...staurophora, n. sp., F.V. 37 *c*; do. S.V.

Fig. 38. Denticula fulva, n. sp., F.V. 38 *b*; do. S.V.
— 39. ... marina, n. sp., 39 *b*; do. S.V.
— 40. Diadesmis (?) Williamsoni, F.V., 40 *b*; do. S.V.
— 41. Meridion (?) marinum, n. sp., F.V., 41 *b*; do. S.V.
— 42. Pyxidicula cruciata, Ehr.
— 43. Orthosira angulata, n. sp., F.V., 43 *b*; do. F.V.
Fig. 44. Melosira (?) or Coscinodiscus (?) qu. (?) sp. (?) n. sp.
— 45. Coscinodiscus nitidus, n. sp.
— 46. ... punctulatus, n. sp.
— 47. ... concavus, Ehr.
— 48. ... umbonatus, n. sp.
All the above are magnified 400 diameters.

EXPLANATION OF PLATE XI.

Fig. 49. Coscinodiscus centralis, Ehr.
— 50. Eupodiscus subtilis, n. sp., Ralfs.
— 51. Campylodiscus centralis, n. sp.
— 52. ... Ralfsii (?) Sm.
Fig. 53. Campylodiscus angularis, n. sp.
— 54 & 54 *b*, ... eximius, n. sp.
— 55. ... limbatus, Bréb.
All the above are magnified 400 diameters.

EXPLANATION OF PLATE XII.

Fig. 56. Amphiprora pusilla, n. sp., F.V., 36 *b*; do. S.V.
— 57. ... plicata, n. sp., F.V.
— 58. ... elegans, Sm. S.V., 58 *b*; do. F.V.
— 59. ... lepidoptera, n. sp., F.V., 59 *b*; do., S.V., 59 *c*; do. peculiar view.
— 60. ... obtusa, n. sp., F.V.
— 61. ... maxima, Greg., F.V., 61 *b*; do. S.V.
— 62 & 62 *b* (?) complexa, n. sp., F.V. entire; 62 *c*; do. half frustule, 62 *d* and 62 *e*; do. detached segments.
— 63. Amphora turgida, n. sp.
Fig. 64. Amphora nana, n. sp.
— 65. ... macilenta, n. sp.
— 66. ... angusta, n. sp.
— 67. ... binodis, n. sp.
— 68 & 68 *b*, ventricosa, n. sp.
— 69. ... monilifera, n. sp.
— 70. ... lineata, n. sp.
— 71. ... Ergadensis, n. sp.
— 72. ... lævissima, n. sp.
— 73. ... pellucida, n. sp., 73 *b*; half frustule of do.
— 74, 74 *b*, & 74 *c*, lævis, n. sp., 74 *d*; Amphora, qu.? a form of A. lævis?
All the above are magnified 400 diameters.

EXPLANATION OF PLATE XIII.

Fig. 75. Amphora exigua, n. sp.
— 76. ... dubia, n. sp.
— 77. ... truncata, n. sp.
— 78 & 78 *b*, oblonga, n. sp.
— 79. ... robusta, n. sp., 79 *b* and 79 *c*; half frustules.
— 80. ... spectabilis, n. sp.; 80 *b*; do. var. β, 80 *c*; do. var. γ; 80 *d*; do. view showing the complex structure of the species; 80 *e*; do. detached segment.
— 81, 81 *b*, 81 *c*, 81 *d*, & 81 *e*, ...Proteus, n. sp.
Fig. 82. Amphora lyrata, n. sp.
— 83. ... Milesiana, n. sp.
— 84. ... elongata, n. sp.
— 85. ... quadrata, n. sp.
— 86. ... excisa, n. sp.
— 87. ... nobilis.
— 88. ... Arcus, Greg.
— 89. ... Grevilliana, Greg.
— 90. ... fasciata, n. sp.
— 91. ... complexa, n. sp.
All the above are magnified 400 diameters.

EXPLANATION OF PLATE XIV.

Fig. 93. Amphora acuta, n. sp., detached segment; 93 *b*; do. pack of similar segments.
— 94. ... crassa, n. sp, 94 *b*, 94 *c*, and 94 *d*; do. detached segments.
— 95 & 95 *b*, pusilla, n. sp., simple and complex views.
Fig. 96, 96 *b*, 96 *c*, & 96 *d*, Amphora granulata, n. sp., simple and complex views of two frustules; 96 *f*, detached segment; 96 *e*; form qu? allied to A granulata?

Fig. 97 & 97 *b*, Amphora cymbifera, n. sp., simple and complex views, 97 *c*; detached segment of do.
— 98 & 98 *b*, ... proboscidea, n. sp., simple and complex views, 98 *c*, and 98 *d*; detached segmentsof do.
— 99. Amphora costata, Sm., detached segment.
— 100 & 100 *b*, ... bacillaris, n. sp., simple and complex views.
— 101. Navicula (?) Libellus, n. sp., 101 *b*; do, edge view.
— 102. Nitzschia (?) panduriformis, n. sp.
— 103. ... distans, n. sp., 103 *b*, do. S.V.
— 104. ... hyalina, n. sp., 104 *b*, do. S.V.
Fig. 105 & 105 *b*, Pleurosigma ? reversum, n. sp.
— 106. Sceptroneis Caduceus, Ehr.
— 107. Synedra undulata, Greg. (Toxarium undulatum, Bailey). Two specimens of the S.V., the one straight, the other arcuate.
— 108. ... Hennedyana, n. sp., S.V.

APPENDIX.

— 109. Creswellia (nov. gen.) Turris, n. sp., Arnott.
All the above are magnified 400 diameters.

Postscript.

While the preceding pages were passing through the press, I have been able to examine with care numerous specimens of most of the forms there described, and I wish here to modify to a small extent some of the views I have expressed. In every instance, I speak after the comparison of a very large number of fine examples.

1. *Navicula nebulosa*, fig. 8. I wish to observe, that after a very careful comparison of this form with *N. Hennedyi*, I have no longer any doubts as to its being a distinct species. I find it remarkably uniform in its characters, and particularly in its oval form, with the ends on the whole broadly rounded, while it has a slight angularity in the middle, and a slight trace of acumination at the apices. It is equally uniform in the narrowness of the marginal band of striæ, in the fineness of the striation, and in its very peculiar colour and nebulous aspect. In all these points, *N. Hennedyi* differs from it, as I have stated. But while these points of difference appear trifling, and are difficult to express in words, I must observe, that there is no real resemblance between the forms, and that when, as often happens, both being frequent, they occur close together, and of equal size (though *N. Hennedyi* is usually a larger form), it is quite impossible, even under a low power, to confound them together, the whole aspect of the two forms being remarkably different.

2. *Navicula spectabilis*, fig. 10. Having found, in certain densities, many very fine specimens of this form, I have to state, that it occurs of nearly twice the size of the individual figured, and that it is perfectly uniform in its characters.

3. *Navicula Bombus*, Ehr., fig. 12. This form also has occurred abundantly in certain densities, and I am now quite satisfied that it is a distinct and well-marked species. In the description I have omitted to mention an important character, namely, that it is never, literally,—not in one out of thousands of examples—sym-

metrical. One-half is always more or less larger than the other, and the amount of surface on each side of the median line is unequal. This does not occur in any other panduriform *Navicula*, except only occasionally in *N. didyma*, which cannot be confounded with the present species. *N. splendida*, *N. incurvata*, *N. Musca*, and *N.* (or *P.*) *Pandura*, are all remarkable for symmetry. In addition to this want of symmetry, which is invariable, it may be stated that, although several Naviculæ, and even some of the panduriform group, vary a good deal in shape, there is no species which is more uniform in this respect than *N. Bombus*.

4. *Navicula Smithii*, var. *β*, *fusca*, fig. 15. A careful study of very numerous specimens, both of this form, and of that which I take to be the typical *N. Smithii*, has now entirely satisfied me that *N. fusca* is truly a variety of *N. Smithii*. But it must be added, that this, like the fresh-water *N. elliptica*, is one of the most variable species, not only in form, but also in the striation, which varies from what may be called fine to exceedingly coarse; in colour, which varies from colourless to dark brown; and in general aspect,—*N. Smithii* being usually destitute of the remarkable longitudinal ridge or shade on each side of the median line, so conspicuous in *N. fusca*. In all these points, a perfect gradation may be traced without difficulty.

5. *Navicula Smithii*, var. *γ*, *nitescens*, fig. 16. Having found this form abundantly in one density, I have now come to the conclusion that it is no variety, but a distinct species. I find it perfectly uniform in all its characters, and the remarkable peculiarity of the median line, which is invariably a broadish white line with perfectly parallel sides; while that of *N. Smithii*, including *N. fusca*, is always doubly conical, being much broader in the middle, and forming a very acute point at each apex, seems effectually to separate it from that species. The shining aspect of the striæ is also peculiar.

6. *Navicula Smithii*, var. *δ*, *suborbicularis*, fig. 17. This form has also occurred abundantly, and I am now able to state that it is so uniform in its characters, and so peculiar in its aspect, that it must be admitted as a distinct and well-marked species. The only variation, except one to be presently mentioned, is in size, as it now and then occurs of twice the length of the figure, or even more, in which case it is more oval in shape, though always very broad. But the peculiar structure about the median line, giving the appearance of two white, elliptical bands meeting in the nodule, or of one long elliptical band, suddenly constricted in the middle, seems to be quite invariable, and sufficient to distinguish it. The fact, also, that the striæ are hardly visible, except on a broad marginal band, where they are very conspicuous, having the shining aspect of those in *N. nitescens*, though coarser than in that species, as well as the permanence of its very peculiar form, seem to indicate that it ought to be separated. Neither in this

form nor in the preceding, have I seen the slightest trace of any tendency to pass into *N. Smithii*, or into its variety *N. fusca*.

I have, indeed, lately noticed one variety of the present form, namely, a panduriform variety, agreeing with the type in size, in general aspect, and in the peculiar median line. This I shall describe and figure on a future occasion.

I may here add, that I shall also have to describe and figure another new form of Navicula, occurring abundantly with the preceding ones, which at first I was disposed to refer, like them, to *N. Smithii*. But I find it so uniformly peculiar, that I must separate it also.

7. *Denticula* (?) *lævis*, fig. 33. I have some reason to think that I have detected the S.V. of this species. The F.V. is frequent in some densities, but it would appear that the entire frustule is so much broader on the F.V., that it never lies on the S.V., and that the valves are never, or hardly ever, separated. Even when separate, the S.V. must be so very narrow, and perhaps so convex, that this side is not usuallyseen. In one case, however, where one of a group has been partly turned, I think I can see that the S.V. resembles in shape that of *D. fulva*, only smaller and narrower.

I have also to add to the list of British species two forms, both remarkable, which occur in Lamlash Bay.

These are,—1. *Cocconeis Morrisiana*, Sm., a very curious species lately found by Professor Smith, I believe, in a gathering from the Levant or from the Black Sea. 2. *Pleurosigma compactum*, Grev.; described and figured by Dr Greville, from Trinidad. I propose, in a future paper, to figure these two species as British forms.

I have just now been able to add to the figures, one of the very remarkable detached segment of *Amphora spectabilis*, as described in the text. It will be found in Pl. XIII., fig. 79 *e*.

28*th May* 1857.

CORRIGENDA.

I have to request the reader's attention to the following corrections, which I wish to make in regard to some points in the preceding paper.

1. The form represented in fig. 52, Plate XI., as a modification of *Campylodiscus Ralfsii*, Sm., is, as I am now convinced, entirely distinct from that species, which in fact, occurs in some of the dredgings along with it; and, in addition to its being uniformly small, exhibits a very different aspect. Fig. 52 agrees with the description given by De Brebisson of his *C. decorus*, and I have no doubt belongs to that species, which must therefore be added to the list of British Diatoms.

2. Additional observations have satisfied me, that the form represented in fig. 95 *e*, Plate XIV., is not a form of *Amphora granulata*, but an entirely distinct species, to be more fully described at a future time.

3. I wish to mention, that although I cannot see any reason to separate the two forms, represented in figs. 80 *b*, and 80 *c*, Plate XIII., from *Amphora spectabilis*, fig. 80, so far as the simple view of the latter is concerned, I have not yet been able to trace the complex structure of *A. spectabilis*, shown in fig. 80 *d*, in these smaller forms. Whether this is because I have not yet employed the highest powers of the microscope for this purpose, since these small forms have much finer markings than the larger one, or whether the complex structure occurs in the larger alone, while the smaller remain always simple; or whether, finally, the two smaller forms belong to a different species, are questions which I cannot yet answer.

4. Having, by the kindness of Dr A. S. Donkin, of Morpeth, been enabled to examine a most interesting marine gathering made by him, in which several of the forms described in this paper, as well as several of those yet to be described as occurring in the Clyde, are met with, I have now to state, that I find *Amphora Grevilliana* in that gathering, with almost exactly the form and aspect of *Amphora complexa*, fig. 90, Plate XIII., while detached segments also occur, evidently belonging to it, and having the straight dorsal margin, but yet in all other points agreeing with those of *A. Grevilliana*, as shown in fig. 36*, of my third plate of Glenshira forms, and as seen in the present paper in the entire *A. Grevilliana*, fig. 89, Plate XIII. I have therefore to withdraw *A. complexa* as a distinct species, and to request the reader to consider the figure (fig. 90), as representing one view of a straight-sided form of *A. Grevilliana*. In this variety, as seen in Dr Donkin's gathering, and as I have also observed in the Glenshira Sand and in the Clyde, the detached segments are much narrower than when the dorsum is convex. I have specimens of the convex segments, from the Clyde, nearly three times as broad as Dr Donkin's, with the straight dorsum. I would further observe, that in all probability, fig. 89 represents a frustule, or possibly a half-frustule, viewed from the flat or concave side, while the frustule in fig. 90 is seen from the convex side, so that the flat-lying lateral segments are not so distinctly seen. I must remind the

reader also, that the names and descriptions of figs. 90 and 91, *A. Complexa*, and *A. fasciata*, have by some mistake been transposed, as stated in the errata, which I did not discover till it was too late to correct it. Whether *A. fasciata*, fig. 91, shall also prove a form of *A. Grevilliana*, for which I gave it in my last paper on the Glenshira Sand, I cannot at present determine.

5. The form represented in fig. 74 *d*, Plate XII., is, as I am now quite satisfied, entirely distinct from *Amphora lævis*, and must be hereafter described as a new species.

6. I have been able to introduce, at fig. 68 *c*, Plate XII., a figure of the remarkable detached valve of *Amphora ventricosa*, which resembles a very slender Cymbella, and occurs even longer and narrower than in the figure. In one dredging, I find it tolerably frequent.

7. In the disc of *Coscinodiscus centralis*, Ehr., as represented in fig. 49, Plate XI., the central cells are shown considerably larger than is usually the case in the specimens which I have of this Diatom. Indeed the cells, in the figure, are more like what is seen in the centre of *C. Asteromphalus*, Ehr., and the question arises, whether the specimen figured may not belong to the last-named species, or whether these two species may not, in reality, be essentially one and the same. In a large number of specimens of *C. centralis* which I have lately examined, the central cells are invariably but a little larger than the rest, so that the form represented in fig. 49, if it be *C. centralis*, and I see no other difference, must have been, in this respect, abnormal. I may add, that in these beautiful discs, some of which are considerably larger than the one figured, the cells are distinctly arranged in spiral lines, as in engine-turning, and as is seen also in *C. radiatus*. This character is but slightly indicated in the figure.

W. G.

1*st August* 1857.

ERRATA.

P. 510, line 3, for "vertical" read "ventral"

P. 514, line 13 from bottom, for "Plate XIII. fig. 75" read "Plate XII. fig. 75"

P. 518, line 3 from bottom, for "80 *e*" read "81 *e*"

P. 523. Transpose the paragraphs numbered 90 and 91, so that they shall be read thus:—

90. *Amphora complexa*, n. sp., &c. &c. And

91. *Amphora fasciata*, n. sp., &c. &c.

Also same page, line 2, for "to which I now proceed," read "to be presently described."

P. 531, line 22, for "Echmelleis" read "Echinelleis"

„ „ 23, for "nonconcatenale" read "nonconcatenata"

„ „ 23, for "utrusque" read "utriusque"

P. 539. In the Explanation of Plate XII., after —74, insert "—75...exigua, n. sp."

In the Explanation of Plate XIII., expunge the first item.

Also, transpose the words opposite to — 90 and — 91, so that they shall be as follows:

— 90. ... complexa, n. sp.

— 91. ... fasciata, n. sp.

And after — 91 insert

— 92. ... sulcata, Bréb.

— 93. ... acuta, n. sp. Pack of united segments.

In the Explanation of Plate XIV., for "Fig. 93" read "Fig. 93" *b*, and expunge the words of the second and third lines.

The reader is requested to make, in the Explanations, as repeated opposite Plates XII., XIII., and XIV., the corrections above indicated as required on page 539.

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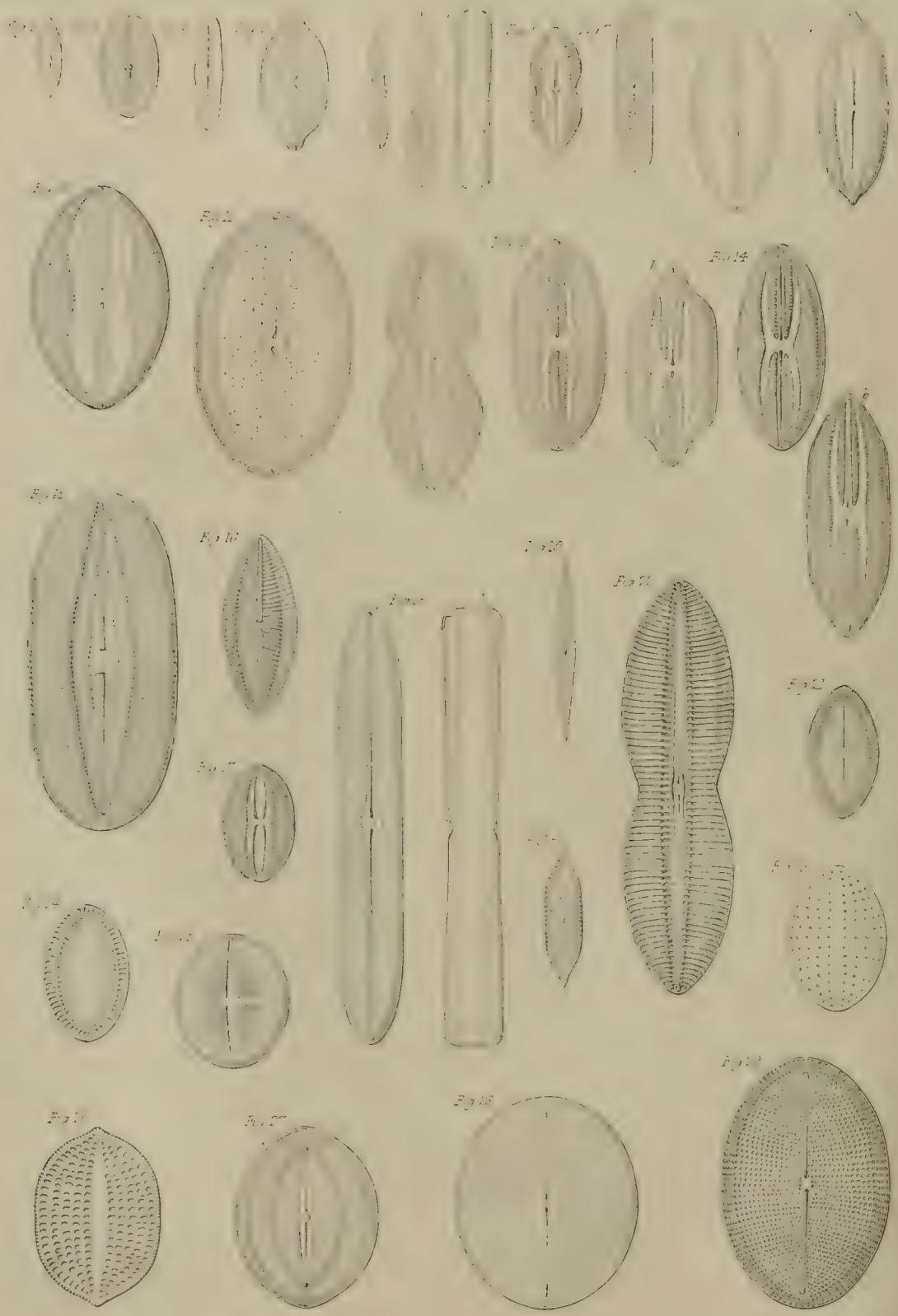

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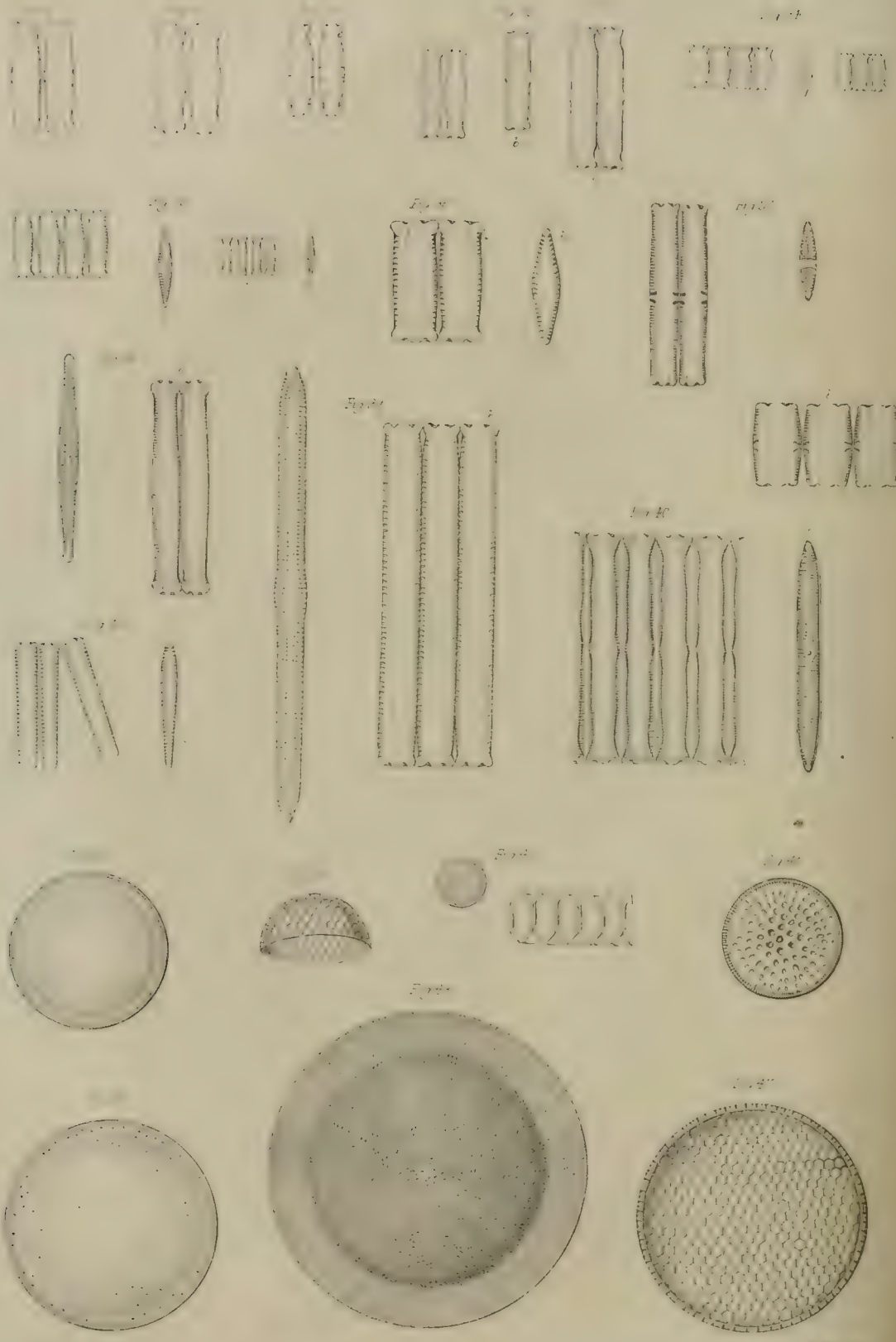
EXPLANATION OF PLATE IX

Fig. 1. Navicula minor, n. sp.
— 2. ... Cluthensis, n. sp.
— 3. ... inconspicua, n. sp.
— 4. ... brevis, n. sp.
— 5. ... Claviculus, n. sp., S.V. × 800. 5 *b*. do., F.V. × 800. 5 *c*, do. S.V. × 400.
— 6. ... Musca, n. sp.
— 7. ... rectangulata, n. sp.
— 8. ... nebulosa, n. sp.
— 9. ... Barclayana, n. sp.
— 10. ... spectabilis, n. sp.
— 11. ... praetexta, Ehr.
— 12. ... Bombus, Ehr.
— 13 & 13 *b*, Lyra, Ehr.
— 14 & 14 *b*, Lyra, var. β.
— 15. ... Smithii, var. β, fusca.

Fig. 16. Navicula Smithii, var. γ, nitescens.
— 17. ... Smithii, var. δ, suborbicularis.
— 18 & 18 *b*, maxima, Greg., S.V. and F.V.
— 19. Pinnularia subtilis, n. sp.
— 20. ... rostellata, n. sp.
— 21. ... Allmaniana, n. sp.
— 22. ... Pandura, Bréb., var. β. elongata.
— 23. Cocconeis distans, Greg.
— 24. ... ornata, n. sp.
— 25. ... dirupta, n. sp.
— 26. ... nitida, n. sp.
— 27. ... pseudomarginata, n. sp.
— 28. ... major, n. sp.
— 29. ... splendida, n. sp.

All the above, except figs. 5 and 5 *b*, are magnified 400 diameters.

British
NATURAL HISTORY.



R. K. C. del. Tuffen West. sc.

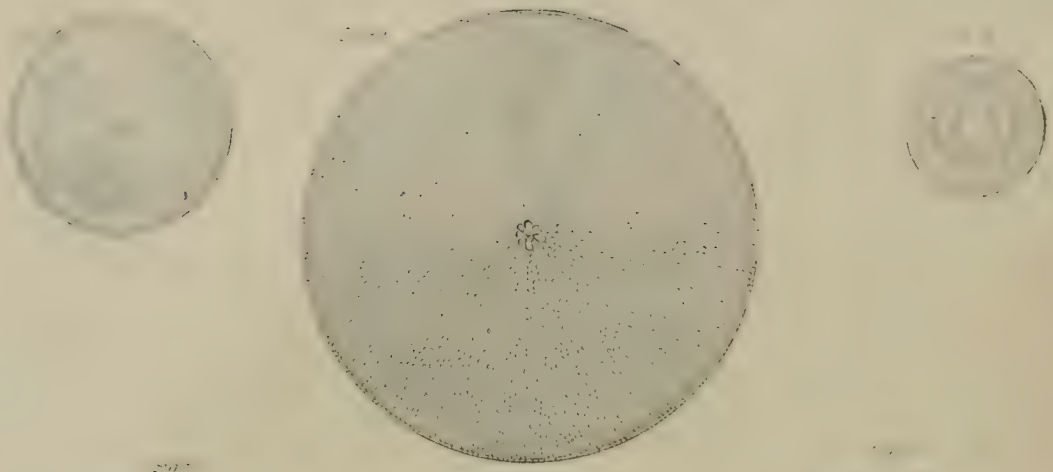
EXPLANATION OF PLATE X.

Fig. 30. Denticula (?) interrupta, n. sp.
— 31. ... (?) capitata, n. sp.
— 32. ... (?) ornata, n. sp.
— 33, 33 *b*, & 33 *c*, ...(?) lævis.
— 34. ... nana, n. sp., F.V. 34 *b*; do. S.V.
— 35, 35 *b*, & 35 *c*, minor, n. sp., F.V. 35 *d*; do. S.V.
— 36. ... distans, n. sp., F.V. 36 *b*; do. S.V.
— 37 & 37 *b*, ...staurophora, n. sp., F.V. 37 *c*; do. S.V.
— 38. ... fulva, n. sp., F.V. 38 *b*; do. S.V.
— 39. ... marina, n. sp., 39 *b*; do. S.V.

Fig. 40. Diadesmis (?) Williamsoni, F.V., 40 *b*; do. S.V.
— 41. Meridion (?) marinum, n. sp., F.V., 41 *b*; do. S.V.
— 42. Pyxidicula cruciata, Ehr.
— 43. Orthosira angulata, n. sp., F.V., 43 *b*; do. F.V.
— 44. Melosira (?) or Coscinodiscus (?) qu. (?) sp. (?) n. sp.
— 45. Coscinodiscus nitidus, n. sp.
— 46. ... punctulatus. n. sp.
— 47. ... concavus, Ehr.
— 48. ... umbonatus, n. sp.

All the above are magnified 400 diameters.

NATURAL HISTORY.



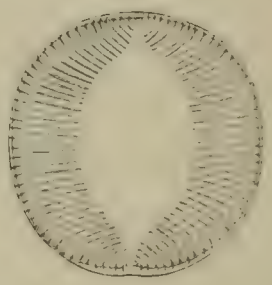
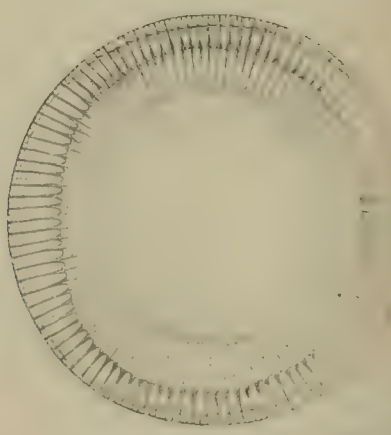


Fig 54

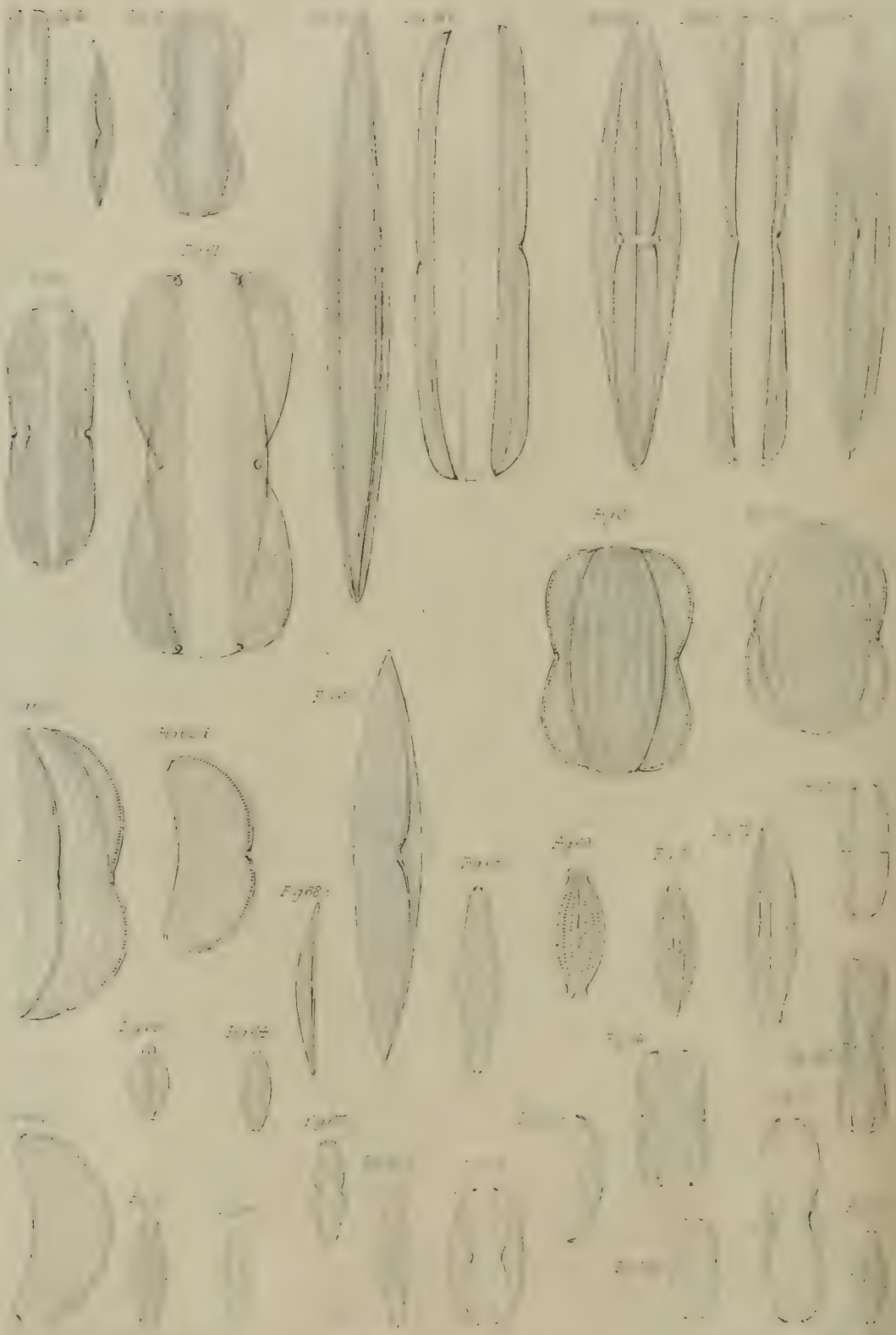
Fig 55



EXPLANATION OF PLATE XI.

Fig. 49. Coscinodiscus centralis, Ehr.
— 50. Eupodiscus subtilis, n. sp., Ralfs.
— 51. Campylodiscus centralis, n. sp.
— 52. ... Ralfsii (?) Sm.
Fig. 53. Campylodiscus angularis, n. sp.
— 54 & 54 *b*, ... eximius, n. sp.
— 55. ... limbatus, Bréb.
All the above are magnified 400 diameters.

HISTORY.



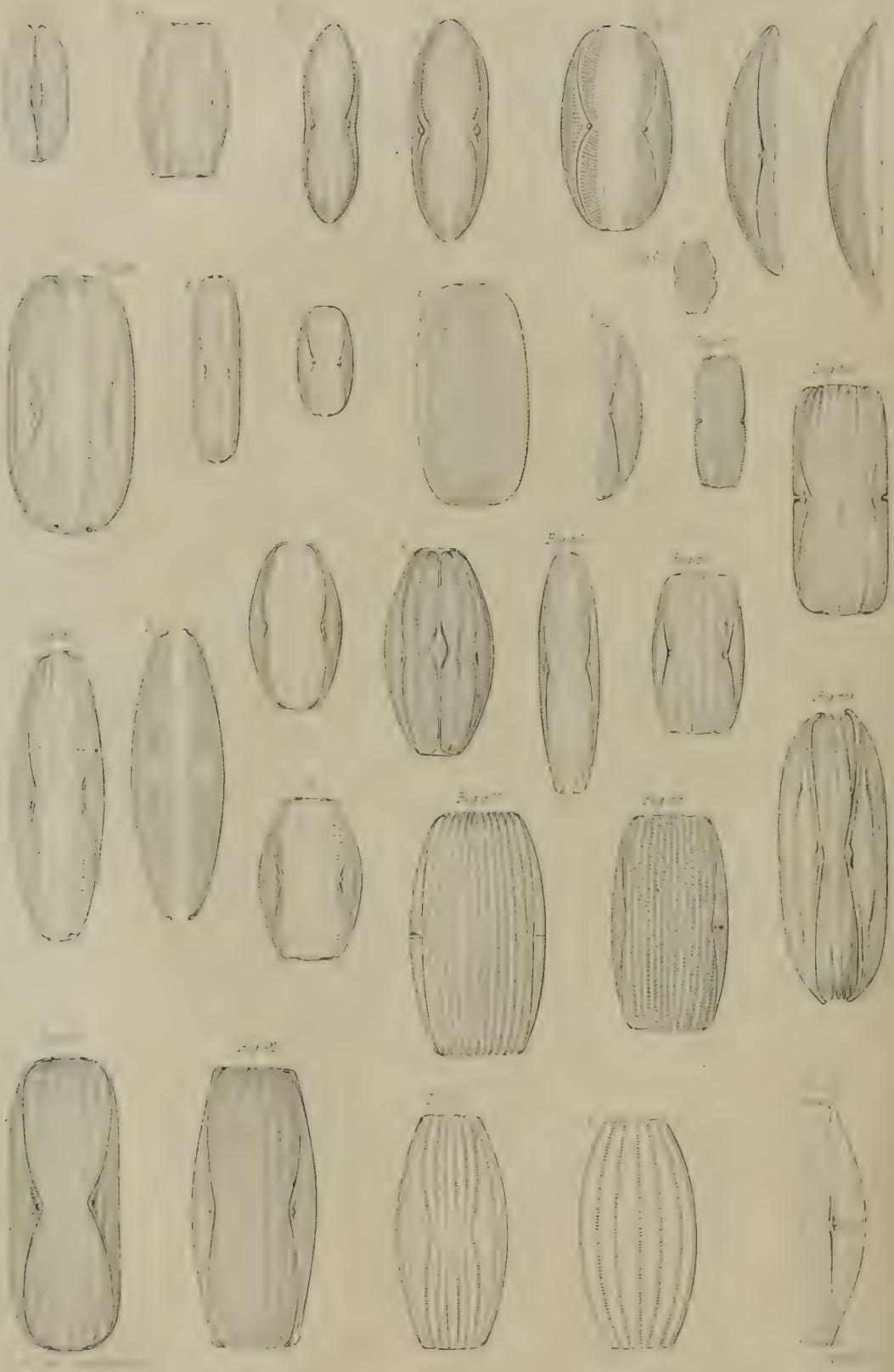
EXPLANATION OF PLATE XII.

Fig. 56. Amphiprora pusilla, n. sp., F.V., 36 *b*; do. S.V.
— 57. ... plicata, n. sp., F.V.
— 58. ... elegans, Sm. S.V., 58 *b*; do. F.V.
— 59. ... lepidoptera, n. sp., F.V., 59 *b*; do., S.V., 59 *c*; do. peculiar view.
— 60. ... obtusa, n. sp., F.V.
— 61. ... maxima, Greg., F.V., 61 *b*; do. S.V.
— 62 & 62 *b* (?) complexa, n. sp., F.V. entire; 62 *c*; do. half frustule, 62 *d* and 62 *e*; do. detached segments.
— 63. Amphora turgida, n. sp.

Fig. 64. Amphora nana, n. sp.
— 65. ... macilenta, n. sp.
— 66. ... angusta, n. sp.
— 67. ... binodis, n. sp.
— 68 & 68 *b*, ventricosa, n. sp.
— 69. ... monilifera, n. sp.
— 70. ... lineata, n. sp.
— 71. ... Ergadensis, n. sp.
— 72. ... lævissima, n. sp.
— 73. ... pellucida, n. sp., 73 *b*; half frustule of do.
— 74, 74 *b*, & 74 *c*, { lævis, n. sp., 74 *d*; Amphora, qu. ? a form of A. lævis ?

All the above are magnified 400 diameters.

BRITISH MUSEUM
HISTORY.

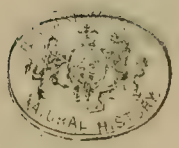


EXPLANATION OF PLATE XIII.

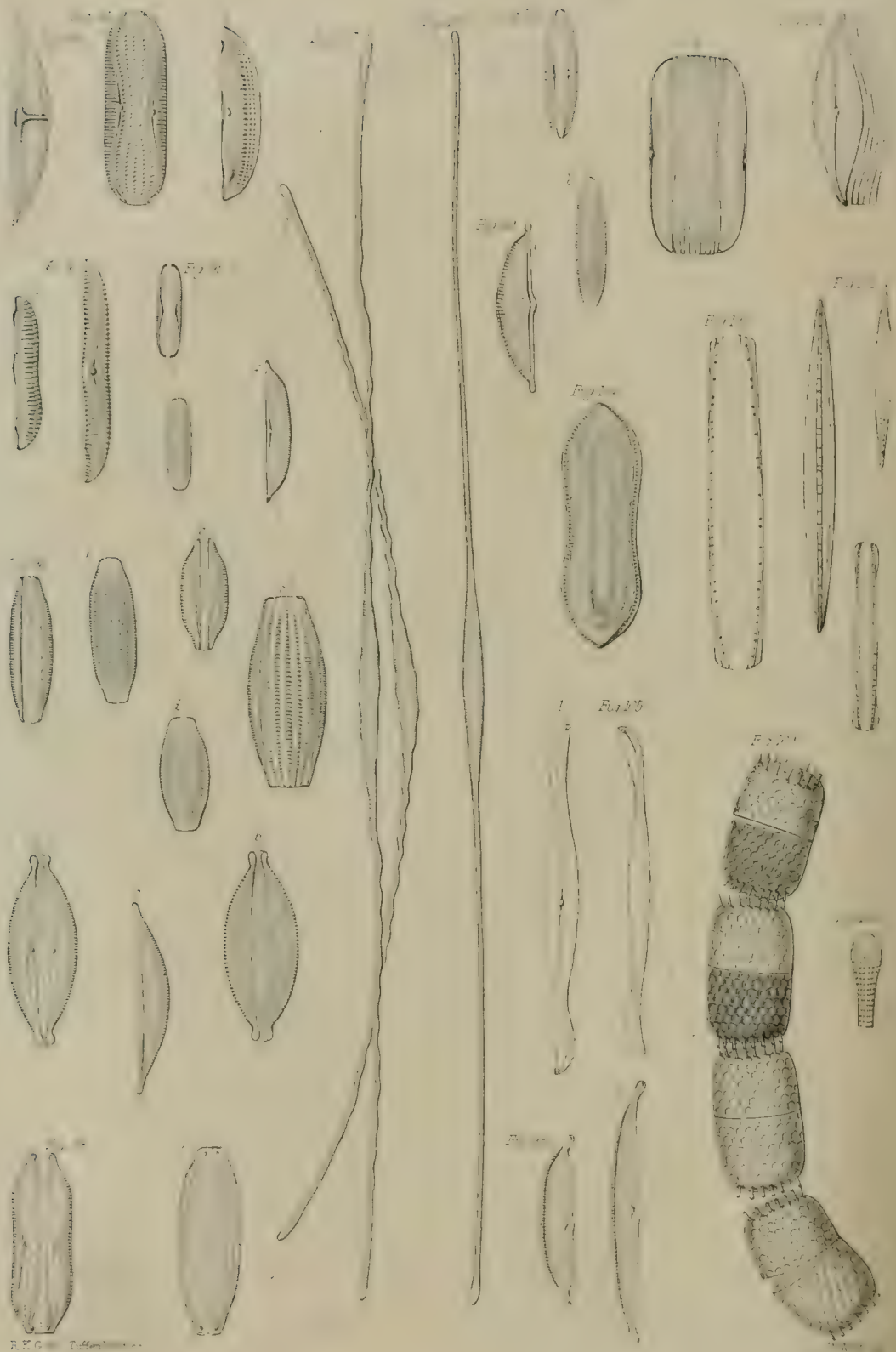
Fig. 75. Amphora exigua, n. sp.
— 76. ... dubia, n. sp.
— 77. ... truncata, n. sp.
— 78 & 78 *b*, oblonga, n. sp.
— 79. ... robusta, n. sp., 79 *b* and 79 *c*; half frustules.
— 80. ... spectabilis, n. sp.; 80 *b*; do. var. β, 80 *c*; do. var. γ; 80 *d*; do. view showing the complex structure of the species; 80 *e*; do. detached segment.
— 81, 81 *b*, 81 *c*, 81 *d*, & 81 *e*, ...Proteus, n. sp.

Fig. 82. Amphora lyrata, n. sp.
— 83. ... Milesiana, n. sp.
— 84. ... elongata, n. sp.
— 85. ... quadrata, n. sp.
— 86. ... excisa, n. sp.
— 87. ... nobilis.
— 88. ... Arcus, Greg.
— 89. ... Grevilliana, Greg.
— 90. ... fasciata, n. sp.
— 91. ... complexa, n. sp.

All the above are magnified 400 diameters.



BRITISH MUSEUM
NATURAL HISTORY.



EXPLANATION OF PLATE XIV.

Fig. 93. Amphora acuta, n. sp. detached segment; 93 *b*; do. pack of similar segments.

— 94. ... crassa, n. sp, 94 *b*, 94 *c*, and 94 *d*; do. detached segments.

— 95 & 95 *b*, ...pusilla, n. sp., simple and complex views.

— 96, 96 *b*, 96 *c*, & 96 *d*, ...granulata, n. sp., simple and complex views of two frustules; 96 *f*, detached segment; 96 *e*; form qu.? allied to A. granulata?

— 97 & 97 *b*, ...cymbifera, n. sp., simple and complex views, 97 *c*; detached segment of do.

— 98 & 98 *b*, ...proboscidea, n. sp., simple and complex views, 98 *c*, and 98 *d*; detached segments of do.

— 99. ... costata, Sm., detached segment.

Fig. 100 & 100 *b*, Amphora bacillaris, n. sp., simple and complex views.

— 101. Navicula (?) Libellus, n. sp., 101 *b*; do, edge view.

— 102. Nitzschia (?) panduriformis, n. sp.

— 103. ... distans, n. sp., 103 *b*, do. S.V.

— 104. ... hyalina, n. sp., 104 *b*, do. S.V.

— 105 & 105 *b*, Pleurosigma ? reversum, n. sp.

— 106. Sceptroneis Caduceus, Ehr.

— 107. Synedra undulata, Greg. (Toxarium undulatum, Bailey). Two specimens of the S.V., the one straight, the other arcuate.

— 108. ... Hennedyana, n. sp., S.V.

APPENDIX.

— 109. Creswellia (nov. gen.) Turris, n. sp., Arnott.

All the above are magnified 400 diameters.

XXXII.—*On the Urinary Secretion of Fishes, with some Remarks on this Secretion in other Classes of Animals.* By John Davy, M.D., F.R.SS. Lond. and Edin., &c.

(Read 2d February 1857.)

Notwithstanding the progress made of late years in animal chemistry in connection with comparative anatomy, I am not aware of any observations that have yet been published on the urinary secretion of fishes. The neglect of this inquiry probably has arisen from several circumstances,—the nature of the element inhabited, the peculiarities of the urinary organs, the difficulty of collecting the matter voided, and its having no well-marked distinctive qualities obvious to the senses.

For some years, as leisure and opportunities offered, I have given attention to the subject, and in the paper which now I have the honour to submit to the Society, I beg to communicate the observations I have made. Few and imperfect as these are, they are given mainly with the hope of attracting notice to the inquiry and of inducing others more favourably situated to engage in its prosecution.

The fishes I have examined in search of their urinary secretion have been the following,—the salmon, sea-trout, charr, common trout, pike, and perch; the skate, ling, conger, cod, pollack, haddock, turbot, bream, and mackerel.

Of these the salmonidæ, pike, perch, ling, and ray, have a small urinary bladder; and in all but the last communicating directly with the kidneys. In the last mentioned, the ray, the communication appears to be indirect, after the manner observable in some of the batrachians, in which the ureters terminate in the cloaca.

The other fishes named seem to be destitute of a urinary bladder, or, if possessed of one, it was so small as to have escaped observation. The ureter in these, when distinct, was found to terminate near the verge of the anal aperture; in several instances it was so large and dilated as to serve the place of a bladder.

In the small urinary bladder of the salmonidæ (so small as to be little more than rudimentary), I have never found any fluid collected. In the bladder of a trout (*Salmo fario*) taken in June, in Windermere, when in highest condition, there was seen a little whitish mucus-like matter. Tested by nitric acid and heat properly graduated, it became yellow, without the slightest purplish tinge, indicative of the presence of lithic acid.

The urinary bladder of the perch (*Perca fluviatilis*) is larger, and internally plicated and spongy, and has been found to contain a fluid. In that of one,—a fish,

weighing about a pound and a half, taken in the same lake, and in the same month as the trout,—there was a little mucus-like matter suspended in its fluid contents. The fluid was rendered turbid by admixture with alcohol. It cleared on rest, from the subsidence of the precipitated matter. The clear solution, decanted and evaporated gently, yielded crystals approaching in form those afforded by a weak solution of muriate of ammonia similarly treated. Redissolved on the addition of a minute portion of nitric acid, and again evaporated, crystalline plates were obtained very like those of the nitrate of urea. Subjected to the temperature required for detecting the presence of lithic acid, the result was negative,—the hue produced was yellow, without the slightest tinge of purple;—and the mucus-like matter similarly tested afforded a like result.

The urinary bladder of the pike (*Esox lucius*) is very small. I have always found it empty. In the ureter* of one of about two pounds, taken in Windermere in May, a few delicate yellowish flakes were detected. These, under the microscope, exhibited no characteristic appearance; acted on by dilute nitric acid, however, they were in great part dissolved; and when evaporated with a graduated heat to dryness on a support of thin glass, the purple stain distinctive of lithic acid was produced, and it was so strong, that it coloured a proportionally large quantity of water.

The ling (*Lota molva*) has a comparatively large urinary bladder. From the bladder of one,—a fish of about four feet long, taken in the Mount's Bay, in Cornwall, in the month of June,—a small quantity, about a drachm, of nearly colourless fluid was obtained, in which a few flakes resembling lymph were suspended. These flakes were tested for lithic acid, but with a negative result. The fluid was coagulated by heat, by nitric acid, and by alcohol, indicating the presence of a notable proportion of albumen. The alcoholic solution, after the separation of the precipitated albumen, evaporated to dryness at a low temperature, yielded, after the addition of a minute portion of nitric acid, crystals which, seen under the microscope—they were too small to be seen without this aid—resembled so closely those of nitrate of urea, that I had little hesitation in coming to the conclusion that they were this compound.

The common ray (*Raia batis*) is provided with two small bladders, each distinct, and neither of them communicating directly with the kidneys. In a male, examined in November, they were found distended with a nearly colourless limpid fluid, in which, placed under the microscope, were seen many small globules, and a few spermatozoa. This fluid, evaporated at a low temperature, yielded a colourless residue, in which were minute crystals of common salt; and, acted on

* Professor Owen, in his *Lectures on the Comparative Anatomy of the Vertebrate Animals* (Part i., p. 223), describes the bladder of the pike as communicating with the kidneys by a single common ureter; in most instances I have found the communication such, but in one fish, one of six pounds, it was by two.

by alcohol and nitric acid, indications were afforded of the presence also of a little albumen and urea, but without any trace of lithic acid.

Of the fishes before named, destitute of a urinary bladder, the ureter, in the instance of the haddock (*Morrhua ægleſinus*), of the cod (*Morrhua vulgaris*), of the pollack (*Merlangus pollachius*), of the turbot (*Rhombus maximus*), was found so capacious, that it might answer the purpose of a receptacle or bladder. In each its inner surface was wet; but only in one, that of the turbot, was there any fluid collected. The quantity obtained, by cutting out the duct, after a ligature had been passed above and below, was about ten drops. It was colourless, not quite clear, and had suspended in it a few white flakes. These were not dissolved by nitric acid, nor did they, when the acid was evaporated by heat, afford any the slightest indications of lithic acid. The residue was yellow; nor could urea be detected in the minute portion of fluid.

Of the bream (*Pagillus centrodontus*), the ureter is narrow, and of little capacity; as is also that of the conger (*Conger vulgaris*), and that of the mackerel (*Scomber scombrus*). Of all three the ureter was found merely moist—wet—as if a fluid had passed; in neither could any solid matter be detected. At the termination of the ureter of the bream a minute portion of whitish matter was seen adhering, suggesting lithate of soda or ammonia, but not confirmed when tested; for, when acted on by nitric acid and heat, the colour acquired was yellow, without the slightest tinge of purple.

I may mention, generally, that in most of the fishes, the names of which have been given, I did not omit examining the cloaca, but with results so unsatisfactory, that they might be said to have been negative. Often there was an appearance as if of the presence of an alkaline lithate; but, when tested, it was found to be different, and the matter chiefly intestinal excrement. In the instance of one only, and that a sea-trout (*Salmo trutta*), was a trace of urea indicated, judging from the form of the minute microscopic crystals obtained on evaporation, after treatment with alcohol and nitric acid.

I may also mention, generally, that in each fish I carefully inspected the structure of the kidneys; but without success as to the finding of any matter conspicuous to the eye, such as is commonly seen in the same organs in the instance of serpents and lizards, viz., the opaque lithate.

In one instance only, that of the haddock, have I examined these organs chemically. The result, too, was negative. The trial was made, first by digesting the kidneys in alcohol, decanting the clear spirit, evaporating it at a low temperature, and to the concentrated extract obtained adding nitric acid; secondly, by digesting the organs with aqua ammoniæ, filtering the solution, and testing the little extract obtained by nitric acid and heat.

If any conclusions are permissible from the preceding few and imperfect observations, I would venture to submit the following:—1*st*, that the urinary secre-

tion of fishes is very limited as to quantity; *2dly*, that it is commonly liquid; *3dly*, that the nitrogenous compound eliminated is variable,—either urea or a lithate (the latter probably very seldom), or some nearly allied compound of azote.

A brief glance at this secretion in other classes of animals may here not be out of place, as bearing on these conclusions. I need not dwell on the importance of the urinary secretion, denoted by its generality, and how, in all the great divisions of the animal kingdom in which it has hitherto been examined, viz., the mammalia, birds, reptiles, insects, spiders, the mollusca, it has been found to consist chiefly of compounds abounding in nitrogen, authorizing the commonly-received conclusion that the secerning organs are depurating in their function, and the main channel by which the excess of this element (nitrogen) is removed from the system.

The differences however compatible with this intent, — differences in the nature of the secretion,—are not a little remarkable. I allude merely to the quality—to the chemical ingredient; and they seem to be regulated more by the structure of the urinary apparatus, or secerning vessels, than by any other circumstance, not even excepting the kind of diet, whether animal or vegetable, or an admixture of the two.

In the mammalia, provided with an ample urinary bladder, the normal secretion is seen to be entirely liquid, and the principal ingredient, so far as it has yet been determined, always soluble urea: Such it has been found to be in man; such in the carnivorous animals; such in the herbivorous; with the addition, in that of some of them, of the hippuric acid.

In birds, on the contrary, and in those reptiles which, like them, are destitute of a urinary bladder, viz., snakes and lizards, invariably the secretion, judging from my own pretty extended experience, is chiefly solid,—a soft, plastic one, owing its consistence to admixture with water, and composed principally of lithate of ammonia and lithic acid. Yet in others of the latter class, which have a receptacle corresponding to the urinary bladder, and destined to hold the secretion,* the secretion is fluid, as in the instance of the toads and frogs; and the nitrogenous matter eliminated is again the soluble urea. The same remark applies to the tortoises, with this difference, that sometimes, though their food be vegetable solid matter, flakes of a lithate are occasionally found suspended in the fluid contents of their urinary bladder.

In insects, also in spiders and scorpions, all which, it is presumed, have no

* Whether this receptacle be considered,—as it is by Mr T. R. Jones, in his *General Outlines of the Animal Kingdom* (p. 585)—the unobliterated remains of the allantois, or a true urinary bladder, its primary use, I apprehend, can hardly now be questioned, since all the later examinations that have been made of the fluid contained in it prove that in composition it is urinous, as stated above: whether, in the instance of the frog, it may not subserve to aid, as some distinguished physiologists suppose, by transpiration in keeping the skin duly moist, is open to question.

receptacle for the secretion but the cloaca, we find it in consistence analogous to that of birds, snakes, and lizards, a soft solid; in insects, as far as my observations have extended, and they have been numerous,* it is composed chiefly of an alkaline lithate; but in the others, the spiders and scorpions, of guanine.†

Of the secretion in the mollusca, also without a urinary bladder, I can venture to say little. In two instances I have found it to be lithic acid; the individuals in the excrement of which I detected this compound were our common slug (*Lima agrestis*), and the large snail of Tobago (*Helix oblonga?*).

Of animals lower in the organic scale, the only ones I have examined with any positive result have been two of the Myriapoda,—the common centipede of the West Indies (*Scolopendra morsitans*), and our millipede (*Iulus terrestris*), the one voracious, feeding on insects, the other feeding on vegetable matter. In the mixed excrement of the scolopendra, lithate of ammonia in abundance was detected;‡ but in that of the millipede, merely a trace of lithic acid.

In this brief notice of the urinary secretion in the several classes of animals mentioned, I have, as I premised, taken notice only of its principal ingredient; I would further beg to remark, that in stating that the quality of the secretion is independent of the quality of the food, I would wish to be understood as not holding the opinion that it is not in some measure modified by the kind of food,—especially as regards the quantity of matter eliminated. As might be expected, the larger the proportion of nitrogen in the food consumed, the larger, *cæteris paribus*, seems to be the quantity of the nitrogenous compound excreted, and *vice versa*. Moreover, when the food is entirely vegetable, there seems to be in some instances a tendency towards the production of the hippuric acid rather than of the lithic. MM. Magnon and Lehmann have found this compound in the urine of the tortoise feeding on lettuce;§ and have found it mixed with lithic acid in the urine of caterpillars feeding exclusively on vegetables,—a result which accords with my own experience.

In the animal economy we see commonly, amongst the different classes of animals, a certain relation and accordance of functions conducive in action to the elaboration and wellbeing of each individual structure. Such a relation is manifest between the kidneys and the lungs; the former the depurator of nitro-

* *Trans. Ent. Society*, vol. iii., N. S.

† When I first examined the excrement of spiders and scorpions in 1847–1848, operating on minute quantities, I inferred that it consisted chiefly of xanthic oxide: Guanine was not then known. Since its discovery by Bodo Unger, I have re-examined portions of the excrement of each, which I brought from the West Indies, and have satisfied myself that the principal ingredient of both is this compound; I have also found it, in accordance with the researches of Will and Gorup-Besanez, to form the chief portion of the excrement of our spiders. The very low degree in which this excrement is soluble in *cold* muriatic acid may account for its having been first confounded with the xanthic oxide.

‡ *Edin. Phil. Jour.*, vol. xlv. p. 383.

§ Lehmann's *Physiological Chemistry*, vol. ii., p. 458.

gen, as much as the latter is of carbonic acid. How strongly is this exemplified in birds;—of high temperature, consuming much atmospheric air, evolving much carbonic acid,—their urinary secretion, also, is remarkably abundant, and abounding in nitrogen.* And in other classes of animals, such as insects in their several stages, such as serpents and lizards, and the hybernating ones of different classes, whether active or torpid, a like accordance, though perhaps not so strongly shown, is yet clearly observable.

Reasoning hence, guided by analogy, might it not be expected that in the instance of fishes, inasmuch as their temperature is low, and the quantity of carbonic acid evolved small, that their urinary secretion also would be small—proportionally small? And, granted that it is so, as the results of the experiments described would seem to indicate, does it not lead to another conclusion, viz., that subsisting, with few exceptions, exclusively on animal food, this their food, under the influence of a high digestive power, is almost entirely assimilated, and that no more is expended on the urinary secretion than is requisite to balance the small amount consumed in carrying on the aërating process? And if this be admitted, does it not help to explain some of their peculiarities,—their remarkable rapidity of growth when supplied with abundance of food,—their little waste of substance when sparingly supplied, and their long endurance without loss of life, under a total, or nearly total, privation of aliment?

The history of the salmon and its congeners, which of late years has been so carefully and successfully studied, might be adduced in illustration,—exemplifying, 1*st*, The great activity and power of the organs carrying on the digestive functions,—the stomach itself of the captured fish, with the parietes adjoining, being found more or less dissolved by the action of the gastric juice in the short space of a few hours, and in being always found empty in the migrating fish; 2*dly*, The extraordinary increase in weight during the short sojourn of the young salmon in the sea, when, without stint of food, it passes from the smolt stage of growth to that of the grilse; and, 3*dly*, The comparatively very slow growth of the young salmon in its parr stage, during the months of winter and early spring, when its food is scarce.

Lesketh How, Ambleside, *Dec.* 1, 1856.

* I may mention as an instance the swallow, feeding like the trout, when the food of the latter is chiefly insects, and, as regards the secretion in question, showing a remarkable difference. From the nest of a pair I had an opportunity of observing, the young of which were only a few days old, the droppings on a flag-stone beneath, in one day, were as many as forty-five; those collected and dried thoroughly weighed 78·3 grains; the following day, the droppings were seventy. They consisted chiefly of lithate of ammonia with a little urea, and of the indigestible remains of insects,—the urinous portion by far the largest. The excrement, it may be inferred, was chiefly from the young birds, as the parent birds were almost constantly on the wing providing food. How large in quantity was this excrement in comparison with the bulk of the birds! I have found an old swallow to weigh only about 300 grains, and when thoroughly dried no more than 105 grains, so that the amount of excrement in two days exceeded considerably in weight one of the old birds!

XXXIII.—*On the Minute Structure of Involuntary Muscular Fibre.* By Joseph Lister, Esq., F.R.C.S. Eng. and Edin., Assistant-Surgeon to the Royal Infirmary, Edinburgh. Communicated by Dr Christison.

(Read 1st December 1856.)

It has been long known that contractile tissue presents itself in the human body in two forms, one composed of fibres of considerable magnitude, and therefore readily visible under a low magnifying power, and marked very characteristically with transverse lines at short intervals, the other consisting of fibres much more minute, of exceedingly soft and delicate aspect, and destitute of transverse striæ. The former variety constitutes the muscles of the limbs, and of all parts whose movements are under the dominion of the will; while the latter forms the contractile element of organs, such as the intestines, which are placed beyond the control of volition. There are, however, some exceptions to this general rule, the principal of which is the heart, whose fibres are a variety of the striped kind.

Till within a recent period the fibres of unstriped or involuntary muscle were believed to be somewhat flattened bands of uniform width and indefinite length, marked here and there with roundish or elongated nuclei; but in the year 1847, Professor Kölliker of Würzburg announced that the tissue was resolvable into simpler elements, which he regarded as elongated cells, each of somewhat flattened form, with more or less tapering extremities, and presenting at its central part one of the nuclei above mentioned. These "contractile" or "muscular fibre-cells," as he termed them, were placed in parallel juxtaposition in the tissue, adhering to each other, as he supposed, by means of some viscid connecting substance. In the following year the same distinguished anatomist gave a fuller account of his discovery in the 1st volume of the *Zeitschrift für Wissenschaftliche Zoologie*, and described in a most elaborate manner the appearances which the tissue presented in all parts of the body where unstriped muscle had been previously known to occur, and also in situations, such as the iris and the skin, where its existence had before been only matter of conjecture, but where the characteristic form of the fibre-cells, and of their "rod-shaped" nuclei had enabled him to recognise it with precision. Confirmations of this view of the structure of involuntary muscular fibre were afterwards received from various quarters, one of the most important being the observation made in 1849 by Reichert, a German histologist, that dilute nitric or muriatic acid loosens the cohesion of the fibre-cells, and enables them to be isolated with much greater facility. In 1852 I wrote a paper "On the Contractile Tissue of the Iris," published in the *Micro-*

scopical Journal, in which I gave an account of the involuntary muscular fibre contained in that organ in man and some of the lower animals, stating that the appearances I had met with corresponded exactly with KÖLLIKER's descriptions, and illustrating my remarks with careful sketches of several fibre-cells from the human iris, isolated by tearing a portion of the sphincter pupillæ with needles in a drop of water. In 1853, another paper by myself appeared in the same Journal, "On the Contractile Tissue of the Skin," confirming KÖLLIKER's recent discovery of the "*arrectores pili*," and describing the distribution of those little bundles of unstriped muscle in the scalp. These and other investigations into the involuntary muscular tissue convinced me of the correctness of KÖLLIKER's observations, and led me to regard his discovery as one of the most beautiful ever made in anatomy; and this is now, I believe, the general opinion of histologists.

Still, however, there are those who are not yet satisfied upon this subject. In MÜLLER's Archives for 1854, is a paper by Dr J. F. MAZONN of Kiew, in which the author expresses his belief that the muscular fibre-cells of KÖLLIKER are created by the tearing of the tissue in preparing it, and denies the existence of nuclei in unstriped muscle altogether; but he gives so very obscure an account of his own ideas respecting the tissue, that his objections seem to me to carry very little weight, more especially as the appearances which he describes require, according to his own account, several days' maceration of the muscle in acid for their development. In June of the present year (1856), Professor ELLIS of University College, London, communicated to the Royal Society of London a paper entitled "Researches into the Nature of Involuntary Muscular Fibre." In the abstract given in the "Proceedings" of the Society, recently issued, we are informed that, "having been unable to confirm the statements of Professor KÖLLIKER respecting the cell-structure of the involuntary muscular fibre, the author was induced to undertake a series of researches into the nature of that tissue, by which he has been led to entertain views as to its structure in vertebrate animals, but more especially in man, which are at variance with those now generally received." In the "summary of the conclusions which the author has arrived at," we find the following: "In both kinds of muscles, voluntary and involuntary, the fibres are long, slender, rounded cords of uniform width . . . ." "In neither voluntary nor involuntary muscle is the fibre of the nature of a cell, but in both is composed of minute threads or fibrils. Its surface-appearance, in both kinds of muscle, allows of the supposition that in both it is constructed in a similar way, viz., of small particles or "sarcous elements," and that a difference in the arrangement of these elements gives a *dotted* appearance to the involuntary, and a transverse striation to the voluntary fibres." "On the addition of acetic acid, fusiform or rod-shaped corpuscles make their appearance in all muscular tissue; these bodies, which appear to belong to the sheath of the fibre, approach nearest in their characters to the corpuscles belonging to the yellow or elastic fibres which pervade va-

rious other tissues; and from the apparent identity in nature of these corpuscles in the different textures in which they are found, and especially in voluntary, as compared with involuntary muscle, it is scarcely conceivable that in the latter case exclusively they should be the nuclei of oblong cells constituting the proper muscular tissue."

Mr Ellis, then, agrees with Mazonn in believing that the tapering fibre-cells of Kölliker owe their shape to tearing of the tissue; and he regards the nuclei as mere accidental accompaniments of the proper muscular structure, probably belonging to the sheath of the fibres, which, according to him, are of rounded form and uniform width.

The distinguished position of Mr Ellis as an anatomist makes it very desirable that his opinion on this important subject should be either confirmed or refuted, and the object of the present paper is to communicate some facts which have recently come under my observation, and which, I hope, may prove to others as unequivocally as they have done to myself, the truth of Kölliker's view of this question.

In September last, being engaged in an inquiry into the process of inflammation in the web of the frog's foot, I was desirous of ascertaining more precisely the structure of the minute vessels, with a view to settling a disputed point regarding their contractility.

Having divided the integument along the dorsal aspect of two contiguous toes, I found that the included flap could be readily raised, so as to separate the layers of skin of which the web consists, the principal vessels remaining attached to the plantar layer. Having raised with a needle as many of the vascular branches as possible, I found, on applying the microscope, that they included arteries of extreme minuteness, some of them, indeed, of smaller calibre than average capillaries. A high magnifying power showed that these smallest arteries consisted of an external layer of longitudinally arranged cellular fibres in variable quantity, an internal exceedingly delicate membrane, and an intermediate circular coat, which generally constituted the chief mass of the vessel, but which proved to consist of neither more nor less than a single layer of muscular fibre-cells, each wrapped in a spiral manner round the internal membrane, and of sufficient length to encircle it from about one-and-a-half to two-and-a-half times. Fig. 18. (Plate XV.) represents one of these vessels as seen under a rather low power, and shows the general spiral arrangement of the fibres of the middle coat. Fig. 19. is a camera lucida sketch of the same artery highly magnified, in which I have for the most part traced the outline of the fibres on the nearer side of the vessel only, but one fibre-cell is shown in its entire length wrapped round nearly two-and-a-half times in a loose spiral. In some other vessels the muscular elements were arranged in closer spirals, as in figs. 20 and 21. They are seen to have more or less pointed extremities, and are provided with an oval nucleus at

their broadest part, discernible distinctly, though somewhat dimly, without the application of acetic acid. The tubular form of the vessels enables the observer, by proper adjustment of the focus, to see the fibre-cells in section; they are then observed to be substantial bodies, often as thick as they are broad, though the latter dimension generally exceeds the former. Here and there a nucleus is so placed in the artery as to appear in section with the fibre-cell, as shown in figs. 20, 22, and 23. The section of the nucleus is in such cases invariably found surrounded by that of the substance of the fibre-cell, though occasionally placed eccentrically in it. From the circular form of its section the nucleus appears to be cylindrical. These fibre-cells are from $\frac{1}{200}$ inch to $\frac{1}{100}$ inch in length, from $\frac{1}{2500}$ inch to $\frac{1}{2000}$ inch in breadth, and about $\frac{1}{2500}$ inch in thickness, measurements on the whole rather greater than those given by Kölliker for the human intestine, the chief difference being that in the frog's arteries they are somewhat broader and thicker.

Now, the middle coat of the small arteries is universally admitted to be composed chiefly of involuntary muscular fibre; but in the vessels just described it consists of nothing whatever else than elongated, tapering bodies, corresponding in dimensions with Kölliker's fibre-cells, and each provided with a single cylindrical nucleus embedded in its substance. Considering, then, that no tearing of the tissue had been practised in the preparation of the objects, but that the parts were seen undisturbed in their natural relations, it appeared to me that the simple observation above related settled the point at issue conclusively.

It was, however, suggested to me by an eminent physiologist, that the various forms in which contractile tissue occurs in the animal kingdom forbid our drawing any positive inference regarding the structure of human involuntary muscle from an observation made on the arteries of the frog. Being anxious to avoid all cavil, and understanding that Mr Ellis's researches had been directed chiefly to the hollow viscera, I thought it best to examine the tissue in some such organ. For this purpose I obtained a portion of the small intestine of a freshly killed pig, selecting that animal on account of the close general resemblance between its tissues and those of man. The piece of gut happened to be tightly contracted, and on slitting it up longitudinally, the mucous membrane, which was thrown into loose folds, was very readily detached from the subjacent parts. I raised one of the thick, but pale and soft fasciculi of the circular coat, and teased it out with needles in a drop of water, reducing it without difficulty to extremely delicate fibrils. On examining the object with the microscope, I found that it was composed of involuntary muscular fibre, almost entirely unmixed with other tissue, reminding me precisely of what I had seen in the human sphincter pupillæ, except that the appearances were more distinct, especially as regards the nuclei, which were clearly apparent without the application of acetic acid. Several of the fibre-cells were isolated in the first specimen I ex-

amined, each one presenting tapering extremities about equidistant from a single elongated nucleus. The fibre-cells were of soft and delicate aspect, generally homogeneous or faintly granular, with sometimes a slight appearance of longitudinal striæ, such as is represented in fig. 4.

I had now seen enough to satisfy my own mind that the involuntary muscular fibre of the pig's intestine was similarly constituted with that of the human iris and the frog's artery: but before throwing up the investigation, I thought it right to examine carefully some short, substantial-looking bodies of high refractive power, which at first sight appeared, both from their form and the aspect of their constituent material, totally different in nature from the rest of the tissue. Several of these bodies are represented in figs. 10–15. Each is seen to be of somewhat oval shape, with more or less pointed extremities, and presents several strongly marked, thick, transverse ridges upon its surface; and each, without exception, possesses a roundish nucleus whose longer diameter lies across that of the containing mass. Yet between these bodies and the long and delicate homogeneous fibre-cells above described, every possible gradation could be traced. Figs. 8 and 9, are somewhat longer than those just indicated, and are also remarkable for their regularity. In figs. 5, 6, and 7, are represented fibre-cells of considerable length, marked here and there with highly refracting transverse bands, in the intervals of which they are of soft and delicate aspect. In several cells one half was short, with closely approximated rugæ, the other half long and homogeneous. Hence it was pretty clear that the appearances in question were due to contraction of the fibre-cells, and that the shortest of these bodies were examples of an extreme degree of that condition; their substantial aspect and considerable breadth being produced by the whole material of the long muscular elements being drawn together into so small a compass. The rounded appearance of the nuclei was accounted for by supposing either that they had themselves contracted, or that they had been pinched up by the contracting fibres, of which explanations the latter appears the more probable.

In order to place the matter if possible beyond doubt, I prepared two contiguous portions of the circular coat of a contracted piece of intestine in different ways; the one by simply cutting off a minute portion with sharp scissors, so as to avoid as much as possible any stretching of the tissue, the other by purposely drawing out a fasciculus to a very considerable length, and then teasing it with needles. In the former preparation, the fibre-cells appeared all of them more or less contracted, except in parts where the slight traction inseparable from any mode of preparation had stretched the pliant tissue, which in the fresh state appears to yield as readily to any extending force as does a relaxed muscle of a living limb. In the other object, where the tissue had been purposely stretched, most of the fibre-cells were extended, and possessed elongated nuclei. Here and there one

would be seen of excessive tenuity, scarcely broader at its thickest part than the nucleus, looking, under the highest magnifying power, like a delicate thread of spun glass. To how great a length the fibre-cells admit of being drawn out in this way without breaking I cannot tell. Fig. 1 represents a portion of such a fibre with the contained nucleus. Among these extended fibres, however, there lay, here and there, an extremely contracted one, the result, I have no doubt, of the irritation produced by the needles upon the yet living tissue. In order to guard against this source of fallacy, I kept a piece of contracted gut 48 hours, and then examined two contiguous parts of the circular coat in the way above described. The muscle was much less readily extended than in the fresh state, and I found that, where stretching of the tissue had been avoided as much as possible, it was composed entirely of fibre-cells marked with transverse ridges of varying thickness and proximity; a minute fibril having, under a rather low power, the general aspect represented in fig. 17. But I saw no distinct examples of the extreme degree of contraction so frequent in muscle from the same piece of intestine in the fresh state. This confirmed my suspicion that the latter had been induced by the irritation of the mode of preparation. On the other hand, a fully stretched fasciculus showed its fibres everywhere destitute of transverse rugæ, so that the point was now distinctly proved. Kölliker, in his original article in the *Zeitschrift für Wissenschaftliche Zoologie*, figured some long fibre-cells with transverse lines upon them,—"knotty swellings," as he termed them, which he supposed probably due to contraction, and he repeats this hypothesis in the part of his *Mikroskopische Anatomie*, published in 1852. The *proof* of the correctness of this idea is now, I believe, given for the first time.

The bearings of these observations on the main question respecting the structure of involuntary muscular fibre are obvious and important. In the first place, if the short, substantial bodies were mere contracted fragments of rounded fibres of uniform width, we should expect them to be as thick at their extremities as at the centre, instead of which they are always more or less tapering, and often present a very regular appearance of two cones applied to each other by their bases. Secondly, the uniform central position of the nuclei in the contracted fibres, proves clearly that the former are no accidental appendages of the latter, to which it seems difficult to refuse Kölliker's appellation of *cells*.

The effect of acetic acid on the involuntary muscular tissue is to render the fibres indistinct, but the nuclei more apparent; and if this reagent be applied to a piece of contracted muscle, many of the nuclei are seen to be of more or less rounded form. The deviation of the nuclei from the "rod-shape" has hitherto been a puzzling appearance, but is now satisfactorily accounted for.

In examining a fasciculus that had been fully stretched, 48 hours after death, I met with several good specimens of isolated fibre-cells, two of which are represented in figs. 2 and 3. I would draw particular attention to the delicate, spirally-twisted extremities of the fibre-cell 3, such as no tearing of a continuous fibre could possibly have produced. Though these fibres are very long, yet we have no reason to believe that anything near the extreme degree of extension has been attained in them, and we cannot but contemplate with amazement the extent of contractility possessed by this tissue.

In fig. 16 is represented a portion of a fibre-cell curled up, which has been introduced for the sake of the clear manner in which it shows the position of the nucleus embedded in it. Just as in the case of the fibres wrapped round the arteries of the frog's foot, this cell might be seen in section by proper adjustment, and that section is observed to be oval; proving that the fibre is not round, but somewhat flattened. It happens that the nucleus appears at this point; its section is circular, and is surrounded on all sides by the substance of the cell.

The pig's intestine seems to be a peculiarly favourable situation for the investigation of unstriped muscle. Judging from KÖLLIKER's measurements, the fibres appear to be of much larger size there than in the same situation in the human body. The length of the fibre-cell 3 is $\frac{1}{37}$ inch. The fibre 2 is imperfect at one extremity; but, taking the double of the distance from its pointed end to the nucleus, its length is $\frac{1}{33}$ inch. These measurements are between three and four times greater than any which Professor KÖLLIKER has given for the human intestine, and considerably exceed the length of the "colossal fibre-cells" which he describes as occurring in the gravid uterus. The individual fibre-cells, with their nuclei and transverse markings, if they have any, are quite distinctly to be seen with one of SMITH and BECK's $\frac{4}{10}$ object-glasses. But in order to examine their structure minutely, a higher power is required: that which I use is a first-rate $\frac{1}{12}$, made several years ago by Mr POWELL of London. All the figures in Plate XV., except 17 and 18, are from camera lucida sketches, reduced to the same scale. The principal measurements of the fibre-cells from the pig's intestine are as under:—

Length of fibre-cell, 3,	$\frac{1}{37}$ inch.
Breadth of ditto,	$\frac{1}{3300}$ "
Length of nucleus of ditto,	$\frac{1}{1000}$ "
Breadth of ditto,	$\frac{1}{8000}$ "
Breadth of fibre-cell, 16,	$\frac{1}{3000}$ "
Thickness of ditto,	$\frac{1}{4000}$ "
Length of fibre-cell, 13,	$\frac{1}{750}$ "
Breadth of ditto,	$\frac{1}{1250}$ "
Longitudinal measurement of nucleus of ditto,	$\frac{1}{5000}$ "
Transverse, ditto,	$\frac{1}{3500}$ "
Length of fibre-cell, 15,	$\frac{1}{1000}$ "

Hence it appears that the length of the most contracted fibre-cell is the same as that of the nucleus of an extended one. The fibres vary somewhat in breadth, independently of the results of contraction. Thus, one in the extended condition which I sketched, but which is not here shown, measured only $\frac{1}{4000}$ inch across. The nuclei of the uncontracted fibres are very constantly of the same length, and are good examples of the rod-shape to which KÖLLIKER has directed particular attention. They always possess one or two nucleoli, and have often a slightly granular character; occasionally, as in fig. 21, they present an appearance of transverse markings. One frequently sees near the nucleus of a fibre that has been artificially extended from the contracted state, an appearance of a gap in the substance of the cell, forming a sort of extension of the nucleus, as if the fibre generally had been stretched more completely than the nucleus: an example of this is presented by fig. 7. Mr ELLIS lays great stress on a dotted appearance which he considers characteristic of involuntary muscular fibre. I must say I agree with KÖLLIKER in finding the fibre-cells, for the most part, homogeneous when extended, or faintly marked with longitudinal striæ.* No doubt dots are present in abundance; but these, so far as I have observed them in the pig's intestine, are distinctly exterior to the fibres, though adherent to their surface; and I suspect them to be little globules of a tenacious connecting fluid. That the fibre-cells do stick very tightly together, may be seen by drying a minute portion of the tissue, after which they will be found shrunk, and slightly separated from one another, but connected more or less by minute threads.

To sum up the general results to which we are led by the facts above mentioned. It appears that in the arteries of the frog, and in the intestine of the pig, the involuntary muscular tissue is composed of slightly-flattened elongated elements, with tapering extremities, each provided at its central and thickest part with a single cylindrical nucleus embedded in its substance.

Professor KÖLLIKER's account of the tissue being thus completely confirmed in these two instances, and the description here given of its appearance in the arteries of the frog's foot being an independent confirmation of the general doctrine, there seems no reason any longer to doubt its truth.

* The longitudinal striæ above referred to, are probably due to a fine fibrous structure in the substance of the fibre-cells. When in London, last Christmas, I had, through the kindness of Dr SHARPEY, the opportunity of examining a specimen of muscle from the stomach of a rabbit, which he had prepared after REICHERT's method. The nitric acid had not only detached the fibre-cells from one another, but also brought out very distinctly in each muscular element the appearance of minute parallel longitudinal fibres, which seemed to make up the entire mass of the fibre-cell except the nucleus. In a plate accompanying the paper on the Iris, before referred to, I gave figures of some fibre-cells with distinct granules arranged in longitudinal and transverse rows. This appearance, which, however, so far as my experience goes, is exceptional, and is hardly sufficiently marked to deserve the appellation "dotted," is probably caused by unequal contractions in the constituent material.—2*d April* 1857.

It further appears, that in the pig's intestine the muscular elements are, on the one hand, capable of an extraordinary degree of extension, and, on the other hand, are endowed with a marvellous faculty of contraction, by which they may be reduced from the condition of very long fibres to that of almost globular masses In the extended state they have a soft, delicate, and usually homogeneous aspect, which becomes altered during contraction by the supervention of highly refracting transverse ribs, which grow thicker and more approximated as the process advances. Meanwhile, the "rod-shaped" nucleus appears to be pinched up by the contracting fibre till it assumes a slightly oval form, with the longer diameter transversely placed.

I will only further remark, that these properties of the constituent elements of involuntary muscular fibre explain, in a very beautiful manner, the extraordinary range of contractility which characterizes the hollow viscera.

EXPLANATION OF PLATE XV.

Fig. 1 represents part of a fibre-cell from the pig's intestine, drawn out into a very fine thread.
Figs. 2 and 3, fibre-cells from the same situation, considerably extended.
Fig. 4, fibre-cells exhibiting faint longitudinal striation.
Figs. 5, 6, and 7, fibre-cells imperfectly contracted.
Figs. 8 and 9, small fibre-cells considerably contracted.
Figs. 10, 11, 12, 13, 14 and 15, fibre-cells extremely contracted.
Fig. 16, a fibre-cell curled up, showing the position of the nucleus embedded in its substance.
Fig. 17, part of a moderately contracted fasciculus of unstriped muscle from the pig's intestine, as seen under a rather low magnifying power.
Fig. 18, a small artery from the frog's web, under a rather low magnifying power.
Fig 19, part of the same vessel highly magnified, showing the spiral arrangement of the muscular fibre-cells.
Figs. 20 and 21, muscular fibre-cells from another artery. In fig. 20, the spirals are much closer than in fig. 19; and in fig. 21, the spiral is quite close.
Figs. 22 and 23 represent some fibre-cells in arteries of extreme minuteness, and show the section of the nucleus surrounded by that of the fibre-cell.

Fig 18

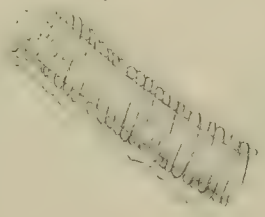


Fig 19

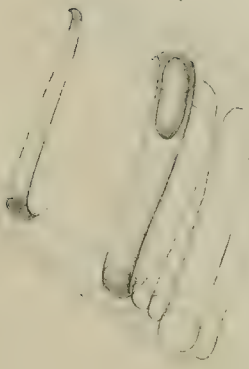


Fig 20

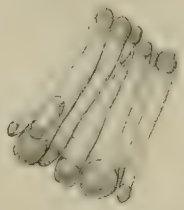


Fig 21

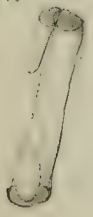
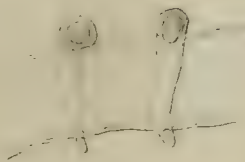
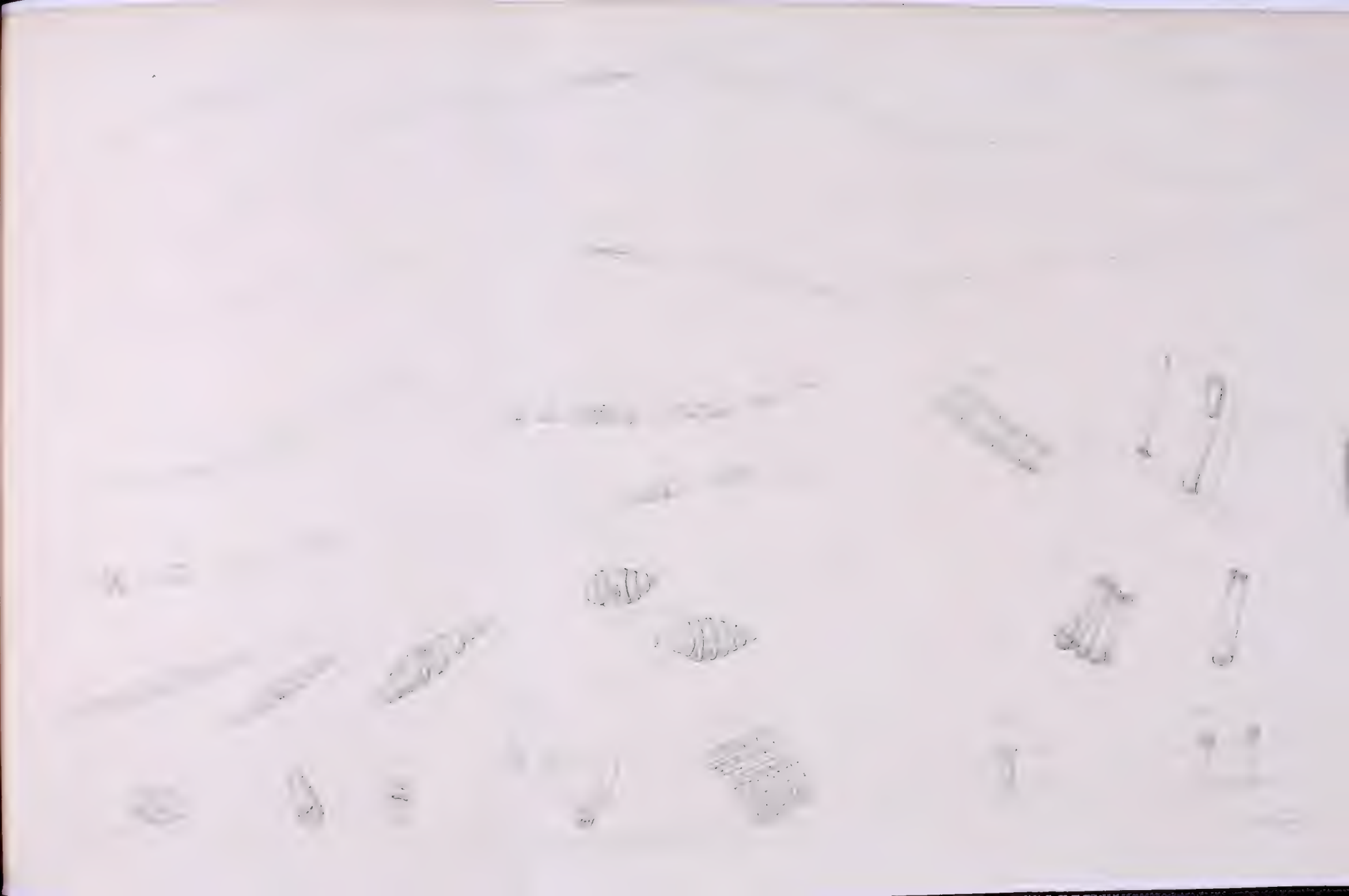


Fig 22

Fig 23





XXXIV.—*On a Dynamical Top, for exhibiting the phenomena of the motion of a system of invariable form about a fixed point, with some suggestions as to the Earth's motion.* By J. C. Maxwell, B.A., Professor of Natural Philosophy in Marischal College, Aberdeen.

(Read 20th April 1857.)

To those who study the progress of exact science, the common spinning-top is a symbol of the labours and the perplexities of men who had successfully threaded the mazes of the planetary motions. The mathematicians of the last age, searching through nature for problems worthy of their analysis, found in this toy of their youth, ample occupation for their highest mathematical powers.

No illustration of astronomical precession can be devised more perfect than that presented by a properly balanced top, but yet the motion of rotation has intricacies far exceeding those of the theory of precession.

Accordingly, we find Euler and D'Alembert devoting their talent and their patience to the establishment of the laws of the rotation of solid bodies. Lagrange has incorporated his own analysis of the problem with his general treatment of mechanics, and since his time M. Poinsot has brought the subject under the power of a more searching analysis than that of the calculus, in which ideas take the place of symbols, and intelligible propositions supersede equations.

In the practical department of the subject, we must notice the rotatory machine of Bohnenberger, and the nautical top of Troughton. In the first of these instruments we have the model of the Gyroscope, by which Foucault has been able to render visible the effects of the earth's rotation. The beautiful experiments by which Mr J. Elliot has made the ideas of precession so familiar to us are performed with a top, similar in some respects to Troughton's, though not borrowed from his.

The top which I have the honour to spin before the Society, differs from that of Mr Elliot in having more adjustments, and in being designed to exhibit far more complicated phenomena.

The arrangement of these adjustments, so as to produce the desired effects, depends on the mathematical theory of rotation. The method of exhibiting the motion of the axis of rotation, by means of a coloured disc, is essential to the success of these adjustments. This optical contrivance for rendering visible the nature of the rapid motion of the top, and the practical methods of applying the theory of rotation to such an instrument as the one before us, are the grounds on which I bring my instrument and experiments before the Society as my own.

I propose, therefore, in the first place, to give a brief outline of such parts of

the theory of rotation as are necessary for the explanation of the phenomena of the top.

I shall then describe the instrument with its adjustments, and the effect of each, the mode of observing of the coloured disc when the top is in motion, and the use of the top in illustrating the mathematical theory, with the method of making the different experiments.

Lastly, I shall attempt to explain the nature of a possible variation in the earth's axis due to its figure. This variation, if it exists, must cause a periodic inequality in the latitude of every place on the earth's surface, going through its period in about eleven months. The amount of variation must be very small, but its character gives it importance, and the necessary observations are already made, and only require reduction.

On the Theory of Rotation.

The theory of the rotation of a rigid system is strictly deduced from the elementary laws of motion, but the complexity of the motion of the particles of a body freely rotating renders the subject so intricate, that it has never been thoroughly understood by any but the most expert mathematicians. Many who have mastered the lunar theory have come to erroneous conclusions on this subject; and even NEWTON has chosen to deduce the disturbance of the earth's axis from his theory of the motion of the nodes of a free orbit, rather than attack the problem of the rotation of a solid body.

The method by which M. POINSOT has rendered the theory more manageable, is by the liberal introduction of "appropriate ideas," chiefly of a geometrical character, most of which had been rendered familiar to mathematicians by the writings of MONGE, but which then first became illustrations of this branch of dynamics. If any further progress is to be made in simplifying and arranging the theory, it must be by the method which POINSOT has repeatedly pointed out as the only one which can lead to a true knowledge of the subject,—that of proceeding from one distinct idea to another, instead of trusting to symbols and equations.

An important contribution to our stock of appropriate ideas and methods has lately been made by Mr R. B. HAYWARD, in a paper, "On a Direct Method of estimating Velocities, Accelerations, and all similar quantities, with respect to axes, moveable in any manner in Space." (*Trans. Cambridge Phil. Soc.* vol. x. part i.)

* In this communication I intend to confine myself to that part of the subject which the top is intended to illustrate, namely, the alteration of the position of the axis in a body rotating freely about its centre of gravity. I shall, therefore, deduce the theory as briefly as possible, from two considerations only,—the per-

* 7th May 1857.—The paragraphs marked thus have been rewritten since the paper was read.

manence of the original *angular momentum* in direction and magnitude, and the permanence of the original *vis viva*.

* The mathematical difficulties of the theory of rotation arise chiefly from the want of geometrical illustrations and sensible images, by which we might fix the results of analysis in our minds.

It is easy to understand the motion of a body revolving about a fixed axle. Every point in the body describes a circle about the axis, and returns to its original position after each complete revolution. But if the axle itself be in motion, the paths of the different points of the body will no longer be circular or re-entrant. Even the velocity of rotation about the axis requires a careful definition, and the proposition that, in all motion about a fixed point, there is always one line of particles forming an instantaneous axis, is usually given in the form of a very repulsive mass of calculation. Most of these difficulties may be got rid of by devoting a little attention to the mechanics and geometry of the problem before entering on the discussion of the equations.

Mr Hayward, in his paper already referred to, has made great use of the mechanical conception of Angular Momentum.

Definition.—*The Angular Momentum of a particle about an axis is measured by the product of the mass of the particle, its velocity resolved in the normal plane, and the perpendicular from the axis on the direction of motion.*

* The angular momentum of any system about an axis is the algebraical sum of the angular momenta of its parts.

As the *rate of change* of the *linear momentum* of a particle measures the *moving force* which acts on it, so the *rate of change* of *angular momentum* measures the *moment* of that force about an axis.

All actions between the parts of a system, being pairs of equal and opposite forces, produce equal and opposite changes in the angular momentum of those parts. Hence the whole angular momentum of the system is not affected by these actions and re-actions.

* When a system of invariable form revolves about an axis, the angular velocity of every part is the same, and the angular momentum about the axis is the product of the *angular velocity* and the *moment of inertia* about that axis.

* It is only in particular cases, however, that the *whole* angular momentum can be estimated in this way. In general, the axis of angular momentum differs from the axis of rotation, so that there will be a residual angular momentum about an axis perpendicular to that of rotation, unless that axis has one of three positions, called the principal axes of the body.

By referring everything to these three axes, the theory is greatly simplified. The moment of inertia about one of these axes is greater than that about any other axis through the same point, and that about one of the others is a mini-

mum. These two are at right angles, and the third axis is perpendicular to their plan, and is called the mean axis.

* Let A, B, C be the moments of inertia about the principal axis through the centre of gravity, taken in order of magnitude, and let ω_1 ω_2 ω_3 be the angular velocities about them, then the angular momentum will be $A\omega_1$, $B\omega_2$ and $C\omega_3$.

Angular momentum may be compounded like forces or velocities, by the law of the "parallelogram," and since these three are at right angles to each other, their resultant is

$$\sqrt{A^2\omega_1^2+B^2\omega_2^2+C^2\omega_3^2}=H \quad . \quad . \quad . \quad . \quad (1)$$

and this must be constant, both in magnitude and direction in space, since no external forces act on the body.

We shall call this axis of angular momentum the *invariable axis*. It is perpendicular to what has been called the invariable plane. Poinsot calls it the axis of the couple of impulsion. The *direction-cosines* of this axis in the body are,

$$l=\frac{A\omega_1}{H} \quad m=\frac{B\omega_2}{H} \quad n=\frac{C\omega_3}{H}$$

Since l, m, and n vary during the motion, we need some additional condition to determine the relation between them. We find this in the property of the *vis-viva* of a system of invariable form in which there is no friction. The *vis-viva* of such a system must be constant. We express this in the equation

$$A\omega_1^2+B\omega_2^2+C\omega_3^2=V^2 \quad . \quad . \quad . \quad (2)$$

Substituting the values of ω_1 ω_2 ω_3 in terms of l, m, n

$$\frac{l^2}{A}+\frac{m^2}{B}+\frac{n^2}{C}=\frac{V}{H^2}$$

$$\text{Let } \frac{1}{A}=a^2, \quad \frac{1}{B}=b^2, \quad \frac{1}{C}=c^2, \quad \frac{V}{H^2}=e^2$$

and this equation becomes

$$a^2\ l^2+b^2\ m^2+c^2\ n^2=e^2 \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and the equation to the cone, described by the invariable axis within the body, is

$$(a^2-e^2)x^2+(b^2-e^2)\ y^2+(c^2-e^2)z^2=0 \quad . \quad . \quad . \quad (4)$$

The intersections of this cone with planes perpendicular to the principal axes are found by putting x, y, or z, constant in this equation. By giving e various values, all the different paths of the pole of the invariable axis, corresponding to different initial circumstances, may be traced.

* In the figures, I have supposed $a^2=100$, $b^2=107$, and $c^2=110$. The first figure represents a section of the various cones by a plane perpendicular to the axis of x, which is that of greatest moment of inertia. These sections are ellipses having their major axis parallel to the axis of b. The value of e^2 corresponding to each of these curves is indicated by figures beside the curve. The ellipticity

increases with the size of the ellipse, so that the section corresponding to $e^2=107$ would be two parallel straight lines (beyond the bounds of the figure), after which the sections would be hyperbolas.

* The second figure represents the sections made by a plane, perpendicular to the *mean* axis. They are all hyperbolas, except when $e^2=107$, when the section is two intersecting straight lines.

The third figure shows the sections perpendicular to the axis of least moment of inertia. From $e^2=110$ to $e^2=107$ the sections are ellipses, $e^2=107$ gives two parallel straight lines, and beyond these the curves are hyperbolas.

* The fourth and fifth figures show the sections of the series of cones made by a cube and a sphere respectively. The use of these figures is to exhibit the connexion between the different curves described about the three principal axes by the invariable axis during the motion of the body.

* We have next to compare the velocity of the invariable axis with respect to the body, with that of the body itself round one of the principal axes. Since the invariable axis is fixed in space, its motion relative to the body must be equal and opposite to that of the portion of the body through which it passes. Now the angular velocity of a portion of the body whose direction-cosines are l, m, n, about the axis of x is

$$\frac{\omega_1}{1-l^2}-\frac{l}{1-l^2}(l\omega_1+m\omega_2+n\omega_3)$$

Substituting the values of ω_1, ω_2, ω_3, in terms of l, m, n, and taking account of equation (3), this expression becomes

$$\mathrm{H}\frac{(a^2-e^2)}{1\quad l^2}l$$

Changing the sign and putting $l=\frac{\omega_1}{a^2}\mathrm{H}$ we have the angular velocity of the invariable axis about that of x.

$$=\frac{\omega_1}{1-l^2}\quad\frac{e^2-a^2}{a^2}$$

always positive about the axis of greatest moment, negative about that of least moment, and positive or negative about the mean axis according to the value of e^2. The direction of the motion in every case is represented by the arrows in the figures. The arrows on the outside of each figure indicate the direction of rotation of the body.

* If we attend to the curve described by the pole of the invariable axis on the sphere in fig. 5, we shall see that the areas described by that point, if projected on the plane of $y\ z$, are swept out at the rate

$$\omega_1\frac{e^2-a^2}{a^2}$$

Now the axes of the projection of the spherical ellipse described by the pole are,

$$\sqrt{\frac{e^2-a^2}{b^2-a^2}} \text{ and } \sqrt{\frac{e^2-a^2}{c^2-a^2}}$$

Dividing the area of this ellipse by the area described during one revolution of the body, we find the number of revolutions of the body during the description of the ellipse—

$$=\frac{a^2}{\sqrt{b^2-a^2}\ \sqrt{c^2-a^2}}$$

The projections of the spherical ellipses upon the plane of yz are all similar ellipses, and described in the same number of revolutions; and in each ellipse so projected, the area described in any time is proportional to the number of revolutions of the body about the axis of x, so that if we measure time by revolutions of the body, the motion of the projection of the pole of the invariable axis is identical with that of a body acted on by an attractive central force varying directly as the distance. In the case of the hyperbolas in the plane of the greatest and least axis, this force must be supposed repulsive. The dots in the figures 1, 2, 3, are intended to indicate roughly the progress made by the invariable axis during each revolution of the body about the axis of x, y, and z respectively. It must be remembered, that the rotation about these axes varies with their inclination to the invariable axis, so that the angular velocity diminishes as the inclination increases, and therefore the areas in the ellipses above mentioned are not described with uniform velocity in absolute time, but are less rapidly swept out at the extremities of the major axis than at those of the minor.

* When two of the axes have equal moments of inertia, or $b = c$, then the angular velocity ω_1 is constant, and the path of the invariable axis is circular, the number of revolutions of the body during one circuit of the invariable axis, being

$$\frac{a^2}{b^2-a^2}$$

The motion is in the same direction as that of rotation, or in the opposite direction, according as the axis of x is that of greatest or of least moment of inertia.

* Both in this case, and in that in which the three axes are unequal, the motion of the invariable axis in the body may be rendered very slow by diminishing the difference of the moments of inertia. The angular velocity of the axis of x about the invariable axis in space is

$$\omega_1 a^2(1-l^2)$$

which is greater or less than ω_1, as e^2 is greater or less than a^2, and, when these quantities are nearly equal, is very nearly the same as ω_1 itself. This quantity indicates the rate of revolution of the axle of the top about its mean position, and is very easily observed.

* The *instantaneous axis* is not so easily observed. It revolves round the invariable axis in the same time with the axis of x, at a distance which is very small in the case when a, b, c, are nearly equal. From its rapid angular motion in space, and its near coincidence with the invariable axis, there is no advantage in studying its motion in the top.

* By making the moments of inertia very unequal, and in definite proportion to each other, and by drawing a few strong lines as diameters of the disc, the combination of motions will produce an appearance of epicycloids, which are the result of the continued intersection of the successive positions of these lines, and the cusps of the epicycloids lie in the curve in which the instantaneous axis travels. Some of the figures produced in this way are very pleasing.

In order to illustrate the theory of rotation experimentally, we must have a body balanced on its centre of gravity, and capable of having its principal axes and moments of inertia altered in form and position within certain limits. We must be able to make the axle of the instrument the greatest, least, or mean principal axis, or to make it not a principal axis at all, and we must be able to *see* the position of the invariable axis of rotation at any time. There must be three adjustments to regulate the position of the centre of gravity, three for the magnitudes of the moments of inertia, and three for the directions of the principal axes, nine independent adjustments, which may be distributed as we please among the screws of the instrument.

The form of the body of the instrument which I have found most suitable is that of a bell, (Plate XVI. fig. 6.) C is a hollow cone of brass, R is a heavy ring cast in the same piece. Six screws, with heavy heads, x, y, z, x', y', z', work horizontally in the ring, and three similar screws, l, m, n, work vertically through the ring at equal intervals. AS is the axle of the instrument, SS is a brass screw working in the upper part of the cone C, and capable of being firmly clamped by means of the nut c. B is a cylindrical brass bob, which may be screwed up or down the axis, and fixed in its place by the nut b.

The lower extremity of the axle is a fine steel point, finished without emery, and afterwards hardened. It runs in a little agate cup set in the top of the pillar P. If any emery had been embedded in the steel, the cup would soon be worn out. The upper end of the axle has also a steel point by which it may be kept steady while spinning.

When the instrument is in use, a coloured disc is attached to the upper end of the axle.

It will be seen that there are eleven adjustments, nine screws in the brass ring, the axle screwing in the cone, and the bob screwing on the axle. The advantage of the last two adjustments is, that by them large alterations can be made, which are not possible by means of the small screws.

The first thing to be done with the instrument is, to make the steel point at the end of the axle coincide with the centre of gravity of the whole. This is done roughly by screwing the axle to the right place nearly, and then balancing the instrument on its point, and screwing the bob and the horizontal screws till the instrument will remain balanced in any position in which it is placed.

When this adjustment is carefully made, the rotation of the top has no tendency to shake the steel point in the agate cup, however irregular the motion may appear to be.

The next thing to be done, is to make one of the principal axes of the central ellipsoid coincide with the axle of the top.

To effect this, we must begin by spinning the top gently about its axle, steadying the upper part with the finger at first. If the axle is already a principal axis the top will continue to revolve about its axle when the finger is removed. If it is not, we observe that the top begins to spin about some other axis, and the axle moves away from the centre of motion and then back to it again, and so on, alternately widening its circles and contracting them.

It is impossible to observe this motion successfully, without the aid of the coloured disc placed near the upper end of the axis. This disc is divided into sectors, and strongly coloured, so that each sector may be recognised by its colour when in rapid motion. If the axis about which the top is really revolving, falls within this disc, its position may be ascertained by the colour of the spot at the centre of motion. If the central spot appears red, we know that the invariable axis at that instant passes through the red part of the disc.

In this way we can trace the motion of the invariable axis in the revolving body, and we find that the path which it describes upon the disc may be a circle, an ellipse, an hyperbola, or a straight line, according to the arrangement of the instrument.

In the case in which the invariable axis coincides at first with the axle of the top, and returns to it after separating from it for a time, its true path is a circle or an ellipse having the axle in its *circumference*. The true principal axis is at the *centre* of the closed curve. It must be made to coincide with the axle by adjusting the vertical screws l, m, n.

Suppose that the colour of the centre of motion, when farthest from the axle, indicated that the axis of rotation passed through the sector L, then the principal axis must also lie in that sector at half the distance from the axle.

If this principal axis be that of *greatest* moment of inertia, we must *raise* the screw l in order to bring it nearer the axle A. If it be the axis of least moment we must *lower* the screw l. In this way we may make the principal axis coincide with the axle. Let us suppose that the principal axis is that of greatest moment of inertia, and that we have made it coincide with the axle of the instrument. Let us also suppose that the moments of inertia about the other axes are equal,

FIG. 3.

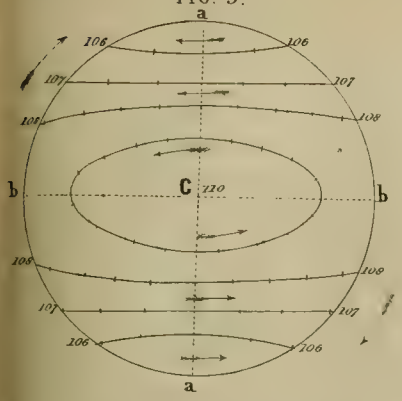


FIG 2.

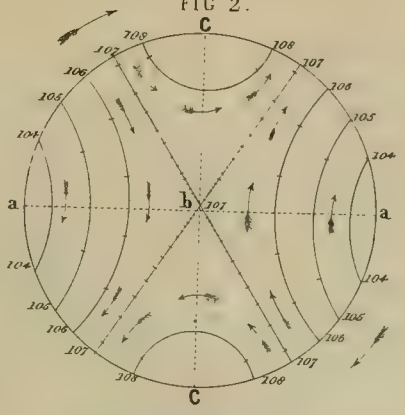


FIG 1.

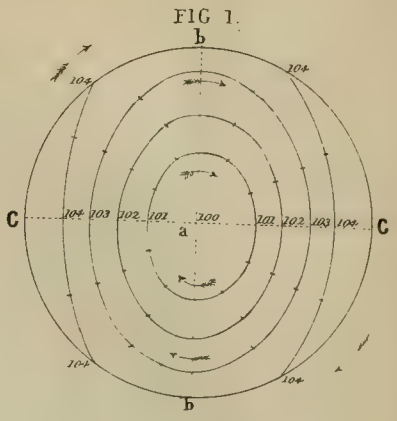


FIG 5.

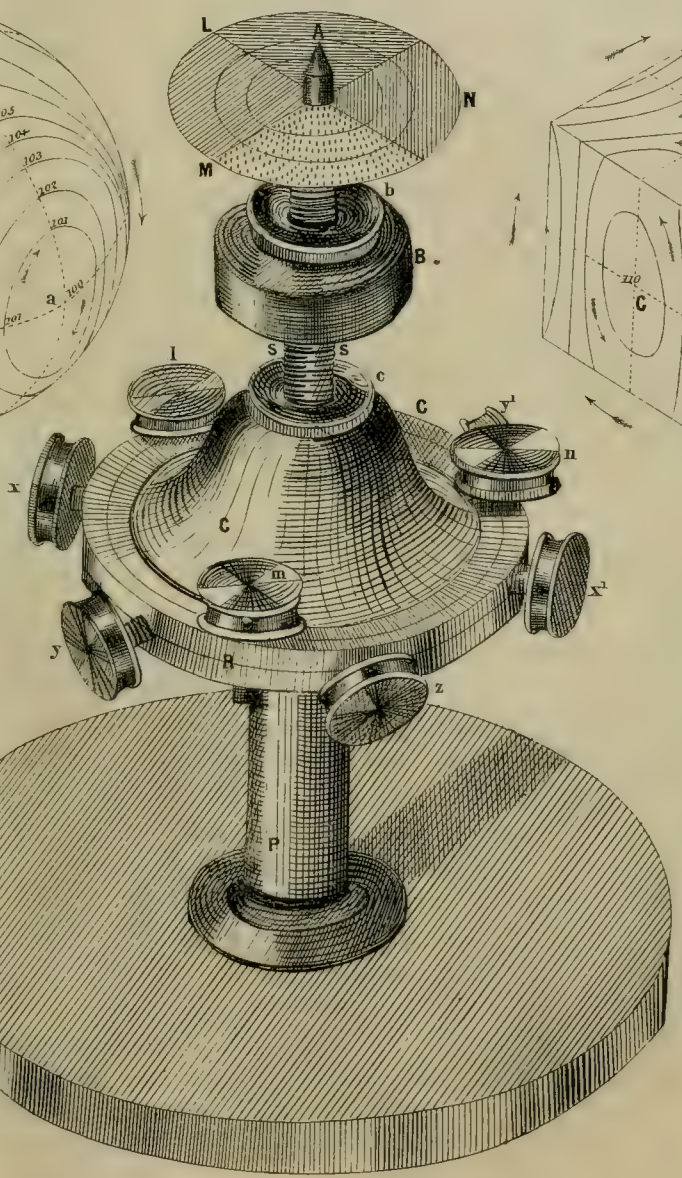


FIG. 6.

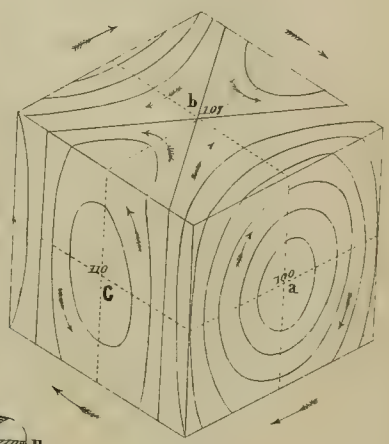


FIG. 4.

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NATURAL HISTORY.

and very little less than that about the axle. Let the top be spun about the axle and then receive a disturbance which causes it to spin about some other axis. The instantaneous axis will not remain at rest either in space or in the body. In space it will describe a right cone, completing a revolution in somewhat less than the time of revolution of the top. In the body it will describe another cone of larger angle in a period which is longer as the difference of axes of the body is smaller. The invariable axis will be fixed in space, and describe a cone in the body.

The relation of the different motions may be understood from the following illustration. Take a hoop and make it revolve about a stick which remains at rest and touches the inside of the hoop. The section of the stick represents the path of the instantaneous axis in space, the hoop that of the same axis in the body, and the axis of the stick the invariable axis. The point of contact represents the pole of the instantaneous axis itself, travelling many times round the stick before it gets once round the hoop. It is easy to see that the direction in which the instantaneous axis travels round the hoop, is in this case the same as that in which the hoop moves round the stick, so that if the top be spinning in the direction L, M, N, the colours will appear in the same order.

By screwing the bob B up the axle, the difference of the axes of inertia may be diminished, and the time of a complete revolution of the invariable axis in the body increased. By observing the number of revolutions of the top in a complete cycle of colours of the invariable axis, we may determine the ratio of the moments of inertia.

By screwing the bob up farther, we may make the axle the principal axis of *least* moment of inertia.

The motion of the instantaneous axis will then be that of the point of contact of the stick with the *outside* of the hoop rolling on it. The order of colours will be N, M, L, if the top be spinning in the direction L, M, N, and the more the bob is screwed up, the more rapidly will the colours change, till it ceases to be possible to make the observations correctly.

In calculating the dimensions of the parts of the instrument, it is necessary to provide for the exhibition of the instrument with its axle either the greatest or the least axis of inertia. The dimensions and weights of the parts of the top which I have found most suitable, are given in a note at the end of this paper.

Now let us make the axes of inertia in the plane of the ring unequal. We may do this by screwing the balance screws x and x^1 farther from the axle without altering the centre of gravity.

Let us suppose the bob B screwed up so as to make the axle the axis of least inertia. Then the mean axis is parallel to xx^1, and the greatest is at right angles to xx^1 in the horizontal plane. The path of the invariable axis on the disc is no longer a circle but an ellipse, concentric with the disc, and having its major axis parallel to the mean axis xx^1.

The smaller the difference between the moment of inertia about the axle and about the mean axis, the more eccentric the ellipse will be; and if, by screwing the bob down, the axle be made the mean axis, the path of the invariable axis will be no longer a closed curve, but an hyperbola, so that it will depart altogether from the neighbourhood of the axle. When the top is in this condition it must be spun gently, for it is very difficult to manage it when its motion gets more and more eccentric.

When the bob is screwed still farther down, the axle becomes the axis of greatest inertia, and aa^1 the least. The major axis of the ellipse described by the invariable axis will now be perpendicular to aa^1, and the farther the bob is screwed down, the eccentricity of the ellipse will diminish, and the velocity with which it is described will increase.

I have now described all the phenomena presented by a body revolving freely on its centre of gravity. If we wish to trace the motion of the invariable axis by means of the coloured sectors, we must make its motion very slow compared with that of the top. It is necessary, therefore, to make the moments of inertia about the principal axes very nearly equal, and in this case a very small change in the position of any part of the top will greatly derange the *position* of the principal axis. So that when the top is well adjusted, a single turn of one of the screws of the ring is sufficient to make the axle no longer a principal axis, and to set the true axis at a considerable inclination to the axle of the top.

All the adjustments must therefore be most carefully arranged, or we may have the whole apparatus deranged by some eccentricity of spinning. The method of making the principal axis coincide with the axle must be studied and practised, or the first attempt at spinning rapidly may end in the destruction of the top, if not of the table on which it is spun.

On the Earth's Motion.

We must remember that these motions of a body about its centre of gravity, are *not* illustrations of the theory of the precession of the Equinoxes. Precession can be illustrated by the apparatus, but we must arrange it so that the force of gravity acts the part of the attraction of the sun and moon in producing a force tending to alter the axis of rotation. This is easily done by bringing the centre of gravity of the whole a little below the point on which it spins. The theory of such motions is far more easily comprehended than that which we have been investigating.

But the earth is a body whose principal axes are unequal, and from the phenomena of precession we can determine the ratio of the polar and equatorial axes of the "central ellipsoid;" and supposing the earth to have been set in motion about any axis except the principal axis, or to have had its original axis disturbed

in any way, its subsequent motion would be that of the top when the bob is a little below the critical position.

The axis of angular momentum would have an invariable position in space, and would travel with respect to the earth round the axis of figure with a velocity $=\omega\frac{C-A}{A}$ where ω is the sidereal angular velocity of the earth. The apparent pole of the earth would travel (with respect to the earth) from west to east round the true pole, completing its circuit in $\frac{A}{C-A}$ sidereal days, which appears to be about 325·6 solar days.

The instantaneous axis would revolve about this axis in space in about a day, and would always be in a plane with the true axis of the earth and the axis of angular momentum. The effect of such a motion on the apparent position of a star would be, that its zenith distance would be increased and diminished during a period of 325·6 days. This alteration of zenith distance is the same above and below the pole, so that the polar distance of the star is unaltered. In fact the method of finding the pole of the heavens by observations of stars, gives the pole of the *invariable axis*, which is altered only by external forces, such as those of the sun and moon.

There is therefore no change in the apparent polar distance of stars due to this cause. It is the latitude which varies. The magnitude of this variation cannot be determined by theory. The periodic time of the variation may be found approximately from the known dynamical properties of the earth. The epoch of maximum latitude cannot be found except by observation, but it must be later in proportion to the east longitude of the observatory.

In order to determine the existence of such a variation of latitude, I have examined the observations of *Polaris* with the Greenwich Transit Circle in the years 1851-2-3-4. The observations of the upper transit during each month were collected, and the mean of each month found. The same was done for the lower transits. The difference of zenith distance of upper and lower transit is twice the polar distance of Polaris, and half the sum gives the co-latitude of Greenwich.

In this way I found the apparent co-latitude of Greenwich for each month of the four years specified.

There appeared a very slight indication of a maximum belonging to the set of months,

March, 51. Feb. 52. Dec. 52. Nov. 53. Sept. 54.

This result, however, is to be regarded as very doubtful, as there did not appear to be evidence for any variation exceeding half a second of space, and more observations would be required to establish the existence of so small a variation at all.

I therefore conclude that the earth has been for a long time revolving about

an axis very near to the axis of figure, if not coinciding with it. The cause of this near coincidence is either the original softness of the earth, or the present fluidity of its interior. The axes of the earth are so nearly equal, that a considerable elevation of a tract of country might produce a deviation of the principal axis within the limits of observation, and the only cause which would restore the uniform motion, would be the action of a fluid which would gradually diminish the oscillations of latitude. The permanence of latitude essentially depends on the inequality of the earth's axes, for if they had been all equal, any alteration of the crust of the earth would have produced new principal axes, and the axis of rotation would travel about those axes, altering the latitudes of all places, and yet not in the least altering the position of the axis of rotation among the stars.

Perhaps by a more extensive search and analysis of the observations of different observatories, the nature of the periodic variation of latitude, if it exist, may be determined. I am not aware of any calculations having been made to prove its non-existence, although, on dynamical grounds, we have every reason to look for some very small variation having the periodic time of 325·6 days nearly, a period which is clearly distinguished from any other astronomical cycle, and therefore easily recognised.

Note.

Dimensions and Weights of the parts of the Dynamical Top.

I. Body of the top—
Mean diameter of ring, 4 inches.
Section of ring, $\frac{1}{2}$ inch square.
The conical portion rises from the upper and inner edge of the ring, a height of $1\frac{1}{2}$ inches from the base.
The whole body of the top weighs . . . . . 1 lb. 7 oz.
Each of the nine adjusting screws has its screw 1 inch long, and the screw and head together weigh 1 ounce. The whole weigh . . . 9 „

II. Axle, &c.—
Length of axle 5 inches, of which $\frac{1}{2}$ inch at the bottom is occupied by the steel point, $3\frac{1}{2}$ inches are brass with a good screw turned on it, and the remaining inch is of steel, with a sharp point at the top. The whole weighs . . . . . $1\frac{1}{2}$ „
The bob B has a diameter of 1·4 inches, and a thickness of ·4. It weighs . $2\frac{3}{4}$ „
The nuts *b* and *c*, for clamping the bob and the body of the top on the axle, each weigh $\frac{1}{2}$ oz. . . . . 1 „

Weight of whole top 2 lb. $5\frac{1}{4}$ oz.

The best arrangement, for general observations, is to have the disc of card divided into four quadrants, coloured with vermilion, chrome yellow, emerald green, and ultramarine. These are bright colours, and, if the vermilion is good, they combine into a grayish tint when the revolution is about the axle, and burst into brilliant colours when the axis is disturbed. It is useful to have some concentric circles, drawn with ink, over the colours, and about 12 radii drawn in strong pencil lines. It is easy to distinguish the ink from the pencil lines, as they cross the invariable axis, by their want of lustre. In this way, the path of the invariable axis may be identified with great accuracy, and compared with theory.

XXXV.—*On the Products of the Destructive Distillation of Animal Matters.* Part IV.

By Thomas Anderson, Professor of Chemistry, University of Glasgow.

(Read 20th April 1857.)

Owing to the great length of time over which the investigation of the products of the destructive distillation of animal substances has stretched, and various circumstances which it is unnecessary to detail, the inquiry has been pursued in a somewhat fragmentary manner, and with less continuity than might have been desired. The difficulties attending many of the experiments, and the occasional exhaustion of materials prepared by laborious processes, extending in many instances over considerable periods, have occasioned long intervals in the regular course of the inquiry which it became necessary to occupy with the examination of such matters as could be taken up at the moment. In this way a number of facts required to complete the history of the bases already described have gradually been accumulated, some of the products of their decomposition examined, and the pyrrol so frequently adverted to in the previous parts of this paper has been subjected to a full investigation. The details of these experiments form the subject of the present communication.

It has been already shown that the whole series of the alcohol bases, from methylamine to butylamine, can be obtained from bone oil, and the probable existence of amylamine in the portion boiling, about 200°, has been pointed out. The quantity of base obtained at that temperature is by no means large; but enough was collected not only to prove the existence of amylamine, but to substantiate the fact that it was unquestionably that base, and not one of its isomeres. After sufficient rectification it gave, with bichloride of platinum, an extremely beautiful platinum salt, which, when the fluid was sufficiently concentrated, deposited itself after some time in fine golden yellow scales, very soluble in water. The mother liquor, on evaporation, yielded another crop, agreeing with the first in properties and composition. A platinum determination of each gave the subjoined results:—

I. 5·39 grains of the platinum salt gave 1·815 grains of platinum.
II. 2·99 grains gave 1·010 grs. platinum.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	...	...	20·46	C_{10}	60
Hydrogen, . . .	...	...	4·77	H_{14}	14
Nitrogen, . . .	...	...	4·77	N	14
Chlorine, . . .	...	...	36·34	Cl_3	106·5
Platinum, . . .	33·67	33·77	33·66	Pt	98·7
			100·00		293·2

When the base was treated with iodide of amyle in a sealed tube, it rapidly dissolved; and, on cooling, the fluid became filled with fine crystalline plates. These crystals, when treated with potash, evolved a smell quite distinct from that of amylamine, much more pleasant, and devoid of that putrid odour which distinguishes the whole of the alcohol amide bases. When a quantity of the iodide was introduced into a retort, with an excess of potash, and distilled into a moderately dilute solution of hydrochloric acid, a salt immediately deposited itself as a crystalline powder of sparing solubility, and possessing all the characters of the hydrochlorate of diamylamine. Analysis gave the following results:—

{ 6·400 grains dried at 212° gave
14·505 grains of carbonic acid and
7·280 grains water.

{ 8·240 grains dried at 212° gave
6·165 grains of chloride of silver.

	Experiment.	Calculation.		
Carbon, . . .	61·80	62·10	C_{20}	120·
Hydrogen, . . .	12·63	12·40	H_{24}	24·
Nitrogen, . . .	...	7·16	N	14·
Chlorine, . . .	18·50	18·34	Cl	35·5
		100·00		193·5

These experiments prove incontestably that the base is amylamine, and they afford an indirect refutation of the opinion expressed by some chemists, that the substance described in a previous part of this paper as propylamine, might possibly be trimethylamine. The occurrence of the whole series of the alcohol bases, with their proper boiling points, as well as many facts observed during the investigation, had fully convinced me of the accuracy of my original opinion; but the experiments now detailed, showing that one of them is really an amide base, may be taken as affording the strongest possible evidence that the others are similarly constituted.

Various attempts have been made to ascertain whether any of the higher members of the alcohol series of bases, and particularly caprylamine, exist in bone oil, but without success. Some difficulty attends the examination, because the boiling points of these substances do not differ very greatly from those of the different members of the pyridine series, and a small quantity existing along with the latter might easily escape detection, but a careful examination of the first portions passing over during the distillation of pyridine, and which ought to contain any caprylamine, has satisfied me that it is not present. Taking into account the great difference in the quantity of hydrogen in these two bases; the former containing 6·3, and the latter 14·9 per cent. of that element, we should anticipate, that in the analysis of the first portions of pyridine, the hydrogen would be in excess if it contained caprylamine, and farther, as the bases of the alcohol series are stronger than those of the pyridine series, it would follow

that, if a mixture of two such bases were partially saturated by an acid, the salt produced should consist chiefly of the stronger base, and consequently should give a large excess of hydrogen. Salts prepared in this way gave the exact results required for pyridine, as will be seen in a subsequent page.

Pyridine and its Compounds.

In the second part of this paper a very cursory account was given of pyridine and its platinum salt, at a time when I had obtained this beautiful base in comparatively small quantity. Subsequent experiments have afforded me a much larger supply, and rendered it possible to submit it and its compounds to a more minute investigation. It is a transparent and colourless oil, with a powerful pungent smell, soluble in water in all proportions, and obtained absolutely dry only with some difficulty. It boils at 242°, and its specific gravity at 32° F. is 0·9858. It precipitates the salts of zinc, iron, manganese, and alumina in the cold, nickel only on the application of heat, and the precipitate dissolves in excess. Copper gives a pale blue precipitate, soluble in excess of base with a deep blue colour, not distinguishable from that produced by ammonia. It has a remarkable tendency to form double salts, most of which are highly crystallizable, and retain the metallic oxide in a state in which it cannot be precipitated by excess of pyridine. An analysis gave—

3·175 grains of carefully dried pyridine gave
8·830 ... carbonic acid, and
1·950 ... water.

		Calculation.		
Carbon,	75·84	75·94	C_{10}	60
Hydrogen,	6·82	6·33	H_5	5
Nitrogen,	...	17·73	N	14
		100·00		79

The density of the vapour of pyridine determined by Dumas's method, gave—

	I.	II.
Temperature of the air,	14° cent.	15° c.
„ vapour,	164 „	143°
Excess of weight of the balloon,	0·3088 grammes.	0·4060 gr.
Capacity of the balloon,	305 c. c.	324 c. c.
Barometer,	765 m. m.	752 m. m.
Residual air,	14 c. c.	„
Density of vapour,	2·912	2·920

The formula C_{10} H_5 N requires

10 vol. carbon vapour = 0·8290 × 10 = 8·2900
10 ... hydrogen ... = 0·0692 × 10 = 0·6920
2 ... nitrogen ... = 0·9713 × 2 = 1·9426

$$\frac{10\ 9246}{4} = 2{\cdot}734$$

These experimental results are somewhat in excess of the theoretical density; but this is in all probability due to the presence of a small quantity of picoline, which, from the nature of the experiment, must necessarily remain in the balloon, and tend to produce an appreciable error in the density, even when its quantity is far too minute to be distinguished by an ordinary analysis. The specimen of pyridine used in these experiments had been purified with great care, and its platinum salt gave results corresponding completely with theory.

Salts of Pyridine.

Hydrochlorate of Pyridine.—When hydrochloric acid is saturated with pyridine, and the solution evaporated on the water bath, the salt remains in the form of a thick syrup, so long as it is warm, but on cooling, crystals slowly make their appearance, and gradually shoot through the fluid, which is eventually converted into a hard radiated mass. The salt deliquesces when exposed to moist air, and sublimes unchanged at a high temperature. It is very soluble in alcohol, but less so than in water. It is insoluble in ether.

Hydriodate of Pyridine.—This salt crystallizes in tabular crystals, readily soluble both in water and alcohol, but not deliquescent. An analysis of the salt in an impure state will be afterwards given.

Hydrobromate of Pyridine.—A deliquescent salt, obtained on evaporation as a mass of acicular crystals.

Nitrate of Pyridine.—This salt is easily obtained by mixing nitric acid and the base. If the acid be concentrated, and the base dry, or nearly so, much heat is produced, and the mixture rapidly solidifies into a mass of short needles, which, when expressed between folds of blotting-paper, closely resembles loaf-sugar. The salt is purified by solution in hot water, or better in boiling spirit. On cooling, it is deposited from the latter solution in fine needles, which can easily be obtained an inch long, even when operating on a very small scale. Sometimes it appears in short thick prisms. It is not deliquescent, but is extremely soluble in water, less so in alcohol, and not at all in ether. When heated in a retort it melts; and if the temperature be raised very gradually it sublimes as a white woolly mass; but, if briskly heated, it distils in the form of a thick oily fluid, which solidifies in the neck of the retort to a mass of acicular crystals. If the heat be carefully regulated it sublimes without undergoing the least change, but if rapidly distilled, a small quantity of red fumes are occasionally seen. Heated on a platinum knife it catches fire and burns with great brilliancy, and a rapidity almost approaching to deflagration. Analyses made on different preparations gave the following results:—

I. { 5·954 grains of nitrate dried at 212° gave
9·238 ... carbonic acid and
2·358 ... water.

II. { 6·036 grains of nitrate dried *in vacuo* gave
9·377 ... carbonic acid and
2·324 ... water.

III. { 4·455 grains obtained from the mother liquor of the previous crop gave
6·889 ... carbonic acid and
1·733 ... water.

IV. { 7·118 grains of nitrate dried at 212° gave
11·009 ... carbonic acid and
2·767 ... water.

	Experiment.				Calculation.		
	I.	II.	III.	IV.			
Carbon,	42·31	42·37	42·17	42·18	42.25	C_{10}	60
Hydrogen,	4·40	4·28	4·32	4·31	4·22	H_6	6
Nitrogen,	...	...	...	...	19·73	N_2	28
Oxygen,	...	...	...	...	33·80	O_6	48
					100·00		142

These results correspond completely with the formula $C_{10}H_5N$ HO NO_5.

Bisulphate of Pyridine.—When sulphuric acid is supersaturated with pyridine, and evaporated in the water bath, a crystalline mass is left which is deliquescent, and soluble in all proportions in water and alcohol, but insoluble in ether. Its reaction is highly acid, and analysis showed it to be a bisulphate.

{ 21·012 grains bisulphate of pyridine, gave
27·867 ... sulphate of baryta = 45·50 per cent. of sulphuric acid.

The formula $C_{10}H_5N$ 2HO SO_3 requires 45·19.

Double Salts of Pyridine.

The platinochloride of pyridine has been already described in the second part of this investigation.

Aurochloride of Pyridine.—This salt is immediately thrown down as a fine lemon-yellow crystalline powder when chloride of gold is added to a solution of hydrochlorate of pyridine. It dissolves readily in hot water, and is deposited, on cooling, in fine yellow needles, little soluble in cold water, and insoluble in alcohol. Analysis gave—

{ 8·525 grains aurochloride gave
46·40 ... carbonic acid and
1·450 ... water.

I. { 7·350 grains aurochloride gave
3·440 ... gold.

II. { 5·480 grains aurochloride gave
2·565 ... gold.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	14·84	...	14·32	C_{10}	60
Hydrogen, . . .	1·89	...	1·43	H_5	6
Nitrogen, . . .	...	...	3·34	N	14
Chlorine, . . .	...	...	33·90	Cl_4	142
Gold, . . .	46·80	46·62	47·01	Au	197
			100·00		419

Corresponding with the formula $C_{10}H_5N\ HCl\ Au\ Cl_3$.

When pyridine is added to a moderately dilute solution of sulphate of zinc in considerable excess, oxide of zinc is precipitated. And if a quantity of hydrochloric acid insufficient to neutralize the pyridine be then added, the fluid instantly becomes clear; but if it be stirred briskly, it rapidly fills with an abundant crystalline precipitate of a double salt. The salt dissolves with facility in boiling water, and is deposited, on cooling, in long, brilliant needles. Sulphate of copper, when treated in a similar manner, gives a pale greenish-blue precipitate, soluble in boiling water, from which it crystallizes in fine bluish needles. The salts of manganese and nickel, and protoxide of iron, appear also to form double salts, but they are very soluble, and have not been particularly examined.

Products of the Decomposition of Pyridine.

Pyridine, like all its homologues, is an exceedingly stable base, and resists the action of oxidising agents. It may be boiled with the most concentrated nitric acid, or with chromic acid, without undergoing decomposition; and treatment with the former acid affords an invaluable means of freeing those bases from any empyreumatic matters with which they may be mixed.

Action of Chlorine on Pyridine.—The action of chlorine on pyridine depends upon the mode in which that agent is employed. When a current of the gas is passed through an aqueous solution of the base it is rapidly absorbed, the fluid acquires a dark brown colour, and evolves a peculiar pungent odour; and on the addition of potash, the smell of unchanged pyridine becomes apparent, while a quantity of a dark brown resinous matter is separated. But if an excess of pyridine be thrown into a large bottle of dry chlorine, and distributed over the sides as rapidly as possible, in order to prevent rise of temperature, it remains perfectly colourless, and is converted into a mass of radiated crystals. On the addition of water the crystals dissolve, leaving a quantity of a snow-white amorphous powder, and hydrochlorate of pyridine is found in the solution. The white powder has a faint smell, not unlike that of bleaching powder. It is insoluble in water, but dissolves in alcohol, and is precipitated again in white flocks on the addition of water. When boiled for some time with water it softens, but

does not thoroughly melt, and at the same time exhales a peculiar irritating vapour, due, apparently, to partial decomposition. It is insoluble in hydrochloric acid; strong nitric acid dissolves it, and the solution on boiling gives off red fumes; but on the addition of water the original substance is deposited apparently unchanged. Potash colours it brown, and on boiling dissolves it, giving a dark brown solution, from which acids precipitate brown flocks. Ammonia, and even carbonate of ammonia, produce a similar decomposition. When heated, it swells up, giving off a pungent smell, and leaving a bulky charcoal. This substance has not been analysed, but the corresponding product of the decomposition of picoline has been examined, and there can be no doubt that the two substances are of analogous constitution. I shall defer any observations on this point until I come to treat of the picoline compound.

Action of Bromine on Pyridine.—When bromine water is gradually added to a solution of pyridine, the fluid becomes muddy, and as the quantity of bromine increases, an abundant precipitate appears, and collects at the bottom of the vessel in the form of a reddish mass of a more or less resinous appearance. This substance is insoluble in water, but soluble in alcohol and ether. When boiled with water it melts and emits a pungent and irritating odour, resembling that of bromine. Hydrochloric acid decomposes it, dissolving pyridine, and liberating bromine, which collects at the bottom of the fluid. Potash likewise decomposes it, evolving pyridine, and combining with bromine. These characters lead to the conclusion that the substance is a direct compound of pyridine, with in all probability, several equivalents of bromine; but its properties were not so definite as to induce me to prepare it on a scale sufficiently large for a detailed examination and analysis. When dry pyridine is thrown into dry bromine vapour, it immediately solidifies into a crystalline mass, which dissolves in water, with the exception of a small quantity of a brownish flocky matter, probably analogous to the compound produced by similar treatment with chlorine. The solution in water becomes dark-coloured on evaporation, and yields a syrup which solidifies on standing, into a mass of minute crystals of hydrobromate of pyridine.

Action of Iodine on Pyridine.—When a mixture of pyridine and tincture of iodine is evaporated to dryness on the water-bath, a dark brown mass is left, which dissolves partially in water, leaving a quantity of brown crystals, too small in amount to admit of examination, and which are very easily decomposed. They appear to be a product similar to the iodine compounds of the fixed bases. The watery solution contains a quantity of a brown matter, removable by animal charcoal, and the fluid, on evaporation, yielded crystals which analysis proved to be the hydriodate of pyridine, although not quite pure.

{ 5·774 grains dried at 212° gave
{ 7·673 ... iodide of silver.

	Experiment.	Calculation.		
Carbon, . . .	...	29·01	C_{10}	60
Hydrogen, . . .	...	2·90	H_6	6
Nitrogen, . . .	...	6·74	N	14
Iodine, . . .	62·43	61·35	I	127
		100·00		207

Corresponding with the formula $C_{10}H_5N$ HI.

Picoline and its Compounds.

The compounds of picoline have already been pretty fully described in my original paper on that base, but the possession of a larger quantity has induced me to examine more in detail some of the products of its decomposition, and to determine with greater exactitude certain of its physical properties. In the paper just referred to I fixed its boiling point at 272°; but an experiment made on a larger scale has convinced me that this is too low, and that when quite pure it boils at 275°. The specific gravity at 32° is 0·9613. The density of its vapour was determined by DUMAS's method with the following results:—

Temperature of the air, ...	13° cent.
... vapour, ...	166° ...
Excess of weight of the balloon, . . .	0·3490 gramme.
Capacity of do., ...	288 c. c.
Barometer, ...	762 m. m.
Residual air, ...	22 c. c.
Specific gravity of vapour, ...	3·29

The formula $C_{12}H_7N$ requires—

12 vol. carbon vapour = 0·8290 × 12 = 9·9480
14 ... hydrogen ... = 0·0492 × 14 = 0·9685
2 ... nitrogen ... = 0·9713 × 2 = 1·9626

$$\frac{12{\cdot}8591}{4} = 3{\cdot}214$$

Nitrate of Picoline.—This salt has been already described as a deliquescent crystalline mass, but I have now succeeded in obtaining it in prismatic crystals of considerable size, which are formed when a quantity of the dry salt, covered with a saturated solution, is left for some weeks in a closely-stoppered bottle. At the end of that time the salt has been converted into a small number of four-sided prisms terminated by dihedral summits. Analysis gave—

5·080 grains dried at 212° gave
8·580 ... carbonic acid and
2·395 ... water.

	Experiment.	Calculation.		
Carbon, . . .	46·06	46·15	C_{12}	72
Hydrogen, . . .	5·23	5·12	H_8	8
Nitrogen, . . .	...	17·96	N_2	28
Oxygen, . . .	...	30·77	O_6	48
		100·00		156

And its formula is $C_{12}H_7N\ HO\ NO_5$.

Products of the Decomposition of Picoline.

Action of Chlorine on Picoline.—The action of a current of chlorine on picoline, both dry and dissolved in water, has been already described, and the results were not such as to induce further experiments in this way. But when an excess of picoline is projected into dry chlorine gas, it is rapidly converted into a more or less distinctly crystallized mass, which, when treated with water, leaves a quantity of an amorphous powder of dazzling whiteness. The properties of this substance are so like those of the corresponding pyridine compound, that the same words would almost serve to describe it. It is insoluble in water; but alcohol dissolves it easily, and the solution, when boiled, undergoes decomposition; an etherial odour, not unlike that of hydrochloric ether, and probably due to the formation of that substance, is first produced, and that is followed by a pungent vapour. It is insoluble in the dilute acids, but soluble in concentrated nitric acid. Potash decomposes it in the cold, and more rapidly if heated. Heated on platinum, it gives off a very pungent vapour and leaves a bulky charcoal. It is decomposed when heated in the water bath. The portion used for analysis is dried *in vacuo*. The results were,—

{ 5·550 grains dried *in vacuo* gave
{ 6·170 ... carbonic acid and
{ 1·100 ... water.

{ 4·445 grains burnt with lime gave
{ 9·265 ... chloride of silver.

	Experiment.	Calculation.		
Carbon, . . .	30·32	30·9°	C_{12}	72
Hydrogen, . . .	2·20	2·14	H_5	5
Nitrogen, . . .	...	5·02	N	14
Chlorine, . . .	61·54	60·94	Cl_4	142
				233

This corresponds very closely with the formula

$$C_{12}\left.\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N\ H\ Cl.$$

or that of the hydrochlorate of a base produced by the substitution of three

equivalents of chlorine for three of hydrogen in picoline, and which would be called trichloropicoline. This view derives confirmation, from the fact, that when exposed to 212°, the substance loses an equivalent of hydrochloric acid, as shown by the subjoined experiment.

{ 2·815 grains heated to 212° gave
{ 5·960 ... chloride of silver.

This corresponds to 52·35 per cent. of chlorine, while the formula $C_{12}\left.\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N$ requires 54·2. Although this is only a very distant approximation to the theoretical number, the discrepancy is not greater than might be expected when the properties of the substance, and the fact that it is coloured brown in the water bath, are taken into account.

Action of Sodium on Picoline.

When sodium is thrown into picoline in the cold, it remains unchanged, and preserves its metallic lustre; but if the picoline be heated to its boiling point, an action begins to manifest itself, brown streaks are seen to appear on the surface of the sodium, and after continued boiling, the whole fluid becomes dark brown, and at length nearly black and viscid. In order to examine this change more minutely, picoline was introduced into a Florence flask, with a quantity of sodium, which varied in different experiments from a fourth to an eighth of its weight; and a long tube being fixed into the mouth of the flask, it was heated in the oil bath in such a manner that the picoline cohobated freely. The action requires some days for its completion, and at the end of that time the contents of the flask are converted into a dark brown hard resinous mass, containing lumps of unchanged sodium. The resinous matter contained sodium in some form of combination which could not be determined; the properties of the substance not being such as to induce an extended examination. It burnt with a smoky flame, leaving soda; and, when exposed to the air showed a tendency to deliquesce, and became sticky on the surface. The pieces of sodium having been carefully removed, the resinous matter was thrown into water, and on standing it was slowly converted into a thick viscid and very dark-coloured oil, much heavier than water, while soda was found in the solution. The oil smelt more or less distinctly of picoline, according to the length of time during which the action had been carried on. After having been carefully washed, so as to remove the soda, and then distilled with water, picoline passed over, and there was left behind a thick oily base, requiring a very high temperature for its distillation, and to which, for reasons to be afterwards explained, I give the name of parapicoline.

Parapicoline.—In the preparation of this base it was found not to be advantageous to push the action of sodium to the extreme; and the cohobation was

generally stopped at the end of the second day, when a considerable quantity of the picoline still remained unchanged, and the contents of the flask had acquired the consistence of treacle. The flask was then broken, the sodium removed as completely as possible, and the whole, along with the pieces of broken glass, to which a considerable quantity of thick matter adhered, was thrown into water. When the oil had collected at the bottom, which generally required some hours, the pieces of glass were removed, the supernatant fluid decanted, and the oily base washed with water, so as to remove the soda and the greater portion of the unchanged picoline. Occasionally a somewhat different process was adopted; the cohobating tube being replaced by another bent at right angles, and the heat continued, so as to distil off and recover the dry picoline; but this was found less convenient, as the increased viscidity of the contents of the flask rendered its after-treatment more troublesome. The well-washed oil was introduced into a small retort, and heat applied. At first a watery fluid containing picoline came over, then dry picoline appeared, and subsequently an oil insoluble in water, began to distil; while a thermometer, placed in the tubulature of the retort, rose at first very rapidly, afterwards more slowly, until, towards the end of the distillation, the temperature reached a point considerably beyond the range of the thermometer. Some crystals of carbonate of ammonia made their appearance in the neck of the retort; traces of pyrrol could be distinguished, and a quantity of charcoal was left. These experiments rendered it sufficiently obvious that the new base possessed a boiling point so high, and so near its point of decomposition as to render necessary the utmost precautions for its purification. The first portion of the distillate which contained unchanged picoline was therefore rejected, and the remainder was heated in a retort immersed in the oil-bath to the boiling point of picoline, while a current of dry hydrogen was passed through it. At first a small quantity of picoline and some crystals of carbonate of ammonia made their appearance; and when these ceased to increase, the temperature of the bath was raised until it reached 380°, when the receiver was changed, and the heat maintained as steadily as possible between that point and 400°, the current of hydrogen being continued all the time. The base was thus made to evaporate at a temperature considerably under its boiling point, and was obtained in a much more satisfactory state, but even then it was not absolutely pure, as it still gave faint indications of pyrrol, although the quantity must have been excessively small; and though wholly soluble in acids, the solution retained a distinctly empyreumatic smell. The small scale on which it was necessary to experiment rendered it impossible to adopt any very efficient means of removing these impurities; but by a second rectification in the current of hydrogen, when the first portions were again rejected, a considerable improvement took place.

Parapicoline is a pale yellow oil of the consistence of a fixed oil, which acquires a brown colour by exposure to the air. It is insoluble in water, although

it communicates its smell to that fluid when shaken with it. It dissolves in all proportions in alcohol, ether, the fixed and volatile oils. It has a highly characteristic empyreumatic smell, quite distinct from that of picoline, without pungency, and closely resembling that of the bases extracted from the portions of Dippel's oil of very high boiling point, and which not improbably contain it. Its smell adheres pertinaciously to the fingers. It fumes slightly when a rod dipped in hydrochloric acid is brought near it, and restores the blue colour of reddened litmus. Boiled with strong nitric acid, it gives off red fumes, and on dilution with water a small quantity of a resinous matter deposits, but the greater part of the base is separated unchanged on the addition of potash. It gives an emerald-green precipitate with sulphate of copper, which dissolves in hydrochloric acid, and forms a green solution, containing a double salt. Most of its compounds are uncrystallizable, and readily soluble in water. Its specific gravity is 1·077, and it boils between 500° and 600 Fahrenheit, and is partially decomposed. The portions employed for analysis were very carefully distilled for that purpose. Owing to the high boiling point, some difficulty was experienced in the combustion, and it was found convenient to weigh the substance in a small open tube, which was passed into the combustion tube. The results were,—

I. 3·060 grains of parapicoline gave
8·730 ... carbonic acid and
2·158 ... water.

II. 3·707 grains of parapicoline gave
10·601 ... carbonic acid and
2·655 ... water.

III. 4·270 grains of parapicoline gave
12·195 ... carbonic acid and
3·072 ; ... water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . . .	77·81	77·99	77·89	77·42	C_{12}	72
Hydrogen, . . .	7·83	7·96	7·99	7·53	H_7	7
Nitrogen, . . .	...	...	...	15·05	N	14
				10·000		93

These numbers correspond almost exactly with those of picoline itself, as indicated by the calculation. The quantity of carbon in all the analyses is considerably above that required by theory; but it is easy to understand how a small quantity of empyreumatic matters formed during the decomposition may produce this effect; and itis sufficiently obvious that the base is isomeric with picoline. This is further confirmed by the analysis of its platinum salt, which is

immediately precipitated when bichloride of platinum is added to a solution of the hydrochlorate of parapicoline, as a pale yellow powder, almost insoluble in water. The results of the analysis were as follows:—

I. { 6·317 grains of platinochloride of parapicoline gave
2·059 ... platinum.

II. { 6·102 grains of platinochloride of parapicoline gave
1·970 ... platinum.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	...	...	24·07	C_{12}	72
Hydrogen, . .	...	...	2·68	H_8	8
Nitrogen, . .	...	...	4·67	N	14
Chlorine, . .	...	...	35·59	Cl_3	106·5
Platinum, . .	32·59	32·28	32·98	Pt	98·7
			100·00		299·2

These numbers correspond with the formula $C_{12}H_7N\,HCl\,PtCl_2$, which is that of the picoline salt; and the analysis would thus lead us to the conclusion that parapicoline is strictly isomeric with that base. But when its high boiling point and other properties are taken into consideration, it is impossible to resist the inference, that its real constitution must be different; and I believe it ought to be represented by the formula $C_{24}H_{14}N_2$, and that it is produced by the combination of two equivalents of picoline. Unfortunately the high boiling point of parapicoline precludes the determination of the specific gravity of its vapour; and as it is not possible in any other way to establish its true constitution, we are compelled to assume, as the most probable hypothesis, that it is produced by a species of reduplication, of which we have already numerous examples in the other classes of organic compounds, although this is the first instance in which it has been observed among the bases. The conversion of cyanic into cyanuric acid is a completely analogous case, the more especially as the three equivalents of cyanic acid which have combined retain their power of neutralizing as many equivalents of base. The simultaneous production of amilene, paramilene, and metamilene, during the action of sulphuric acid on amylic alcohol, may also be referred to as cases in which a somewhat similar reduplication occurs. It is very difficult to explain the mode in which the sodium produces the combination of the two equivalents of picoline, but it may possibly be due to a species of catalytic action, as a large quantity of the sodium employed is always recovered unchanged. A certain quantity of it, however, enters into some sort of combination with the picoline or parapicoline, to produce the resinous compound already mentioned; and it appears most likely that this substance is a sodiopicoline, represented by the formula $C_{12}H_6Na\,N$, in which an equivalent of hydrogen has been replaced by sodium. The action of water upon the resinous matter would then be represented by the following equation:—

$$2\,(C_{12}H_6Na\,N) + 2HO = C_{24}H_{14}N_2 + 2NaO.$$

If this be the case, hydrogen ought to be evolved during the action of sodium on picoline, but owing to the slow nature of the action which takes place, I have not been able to satisfy myself that such is the case.

Whatever be its nature, parapicoline must be considered a very remarkable base, and altogether unique in the mode of its production, but it is completely analogous in its constitution to nicotine, for the determination of the density of the vapour of that base has shown incontestably that its rational formula is $C_{20}H_{10}N_2$, and that of its platinum salt, $C_{20}H_{10}N\ 2HCl\ Pt_2Cl_4$. I think it can scarcely be doubted that nicotine, like parapicoline, has been formed by the combination of two equivalents of a base boiling at a temperature not greatly exceeding 212°, and which will some day be discovered. I have attempted to reconvert parapicoline into picoline, but without success; for though the change appears to be partially effected by rapid distillation, the process is not definite, much carbonate of ammonia being produced.

Salts of Parapicoline.

The salts of parapicoline are chiefly uncrystallizable, and present but few points of interest. I have therefore submitted them to a very cursory examination.

Sulphate of Parapicoline is obtained as a gummy mass, very soluble in water, less so in alcohol. It shows no signs of crystallization.

Nitrate of Parapicoline is obtained by saturating nitric acid with the base, and evaporating. A syrupy fluid is left, which slowly solidifies on cooling into a mass of short needles. It is exceedingly soluble in water, less so in alcohol, and it does not deliquesce.

Hydrochlorate of Parapicoline is an amorphous resin, very soluble in water.

Hydrargochloride of Parapicoline. A solution of corrosive sublimate immediately gives an abundant curdy precipitate of this salt when added to an alcoholic solution of parapicoline. It is insoluble in alcohol and in water, but is instantly dissolved on the addition of a few drops of hydrochloric acid.

Aurochloride of Parapicoline, is a yellow insoluble amorphous substance, decomposed at the boiling heat.

The details now given, as well as those contained in the preceding parts of this investigation, may serve to illustrate with sufficient fulness the general characters of the bases of the pyridine series. It remains for me only to direct attention to their physical properties, which illustrate in a very striking manner the relations subsisting between the different members of a homologous series. The particulars of most of the experiments have been already given, and it is only necessary to add those by which the specific gravity of the vapour of lutidine was determined.

Temperature of the air,	17° cent.
... vapour,	201° „
Excess of weight of the balloon,	0·4493 grammes.
Capacity of do.	302 c. c.
Barometer,	776 m. m.
Residual air,	0
Specific gravity of the vapour,	3·839

The formula $C_{14}H_9N$ requires

14 vol. carbon vapour,	= 0·8290 × 14 =	11·6060
18 vol. hydrogen,	= 0·0692 × 18 =	1·2456
2 vol. nitrogen,	= 0·9713 × 2 =	1·9426
		14·7942

$$\frac{14{\cdot}7942}{4} = 3{\cdot}699$$

In the following table I have collected the whole of the data, all having been carefully redetermined with much purer materials than those used in my original experiments,—

	Formula.	Boiling Point.	Specific Gravity.		Specific Volume at 32°.	Diff.
			Vapour.	Liquid at 32°		
Pyridine, .	$C_{10}H_5N$	242°	2·916	0·9858	80·1	...
Picoline, .	$C_{12}H_7N$	275°	3·290	0·9613	96·7	16·6
Lutidine, .	$C_{14}H_9N$	310°	3·839	0·9467	113·0	16·3
Collidine, .	$C_{16}H_{11}N$	356°	...	0·9439	128·2	15·2

The boiling points of pyridine, picoline, and lutidine agree remarkably well with Kopp's law, but collidine differs very materially from it. Less reliance, however, is to be placed upon the boiling point of the last substance, as it was determined upon a very small quantity of material. The specific gravities of the vapours agree very closely with theory, while those of the fluids themselves, taken at 32°, illustrate also in a very remarkable manner the gradual diminution which is observed when we ascend through a series of homologous substances. To these experimental numbers have been added the specific volumes of the bases at 32°, calculated from the data they afford: but no determinations of the coefficient of expansion of these substances having been made, it is not possible to ascertain their specific volumes at the boiling points, although from the rapidity of their expansion, I believe it will be found that the difference must approach very closely to 22, which is that produced in non-nitrogenous substances by the addition of C_2H_2 to their atom.

Pyrrol.

Reference has frequently been made throughout the course of this investigation to the substance discovered by Runge* in coal-tar, and called by him pyrrol. This substance he described as a gas, although he appears never to have prepared it in a pure state, but simply to have obtained its very singular reaction with fir-wood; and he mentions that it occurs in very small quantity, and accompanies the ammonia produced during destructive distillation. In the second part of this paper, when describing the preparation of the bases from crude bone oil, it was stated that the acid solution afforded on distillation a quantity of an oil possessing in a high degree the characteristic reaction of pyrrol, and which was decomposed when boiled with moderately concentrated acids, with the precipitation of a red resinous matter, while the fluid was found to contain different numbers of the pyridine series of bases. From these facts I was led to infer that this oil contained a series of bases in which pyridine and its homologues were coupled with some substance which was separated by acids, and converted into the red resin,—an opinion which further experiment has entirely refuted.

The oil collected during the distillation of the acid solution of the crude pyridine bases, had a peculiarly fetid and disagreeable smell, and was at first colourless, but soon acquired a reddish colour, and after a few days became nearly black. When freed from water it began to distil about 250°, and a thermometer placed in the tubulature of the retort gradually rose as the distillation proceeded, until at length it reached nearly 400°. The greater proportion of the oil passed between 280° and 310°, but large fractions were obtained at much higher temperatures. All the fractions had a characteristic smell different from that of the pyridine bases, and gave instantaneously the reaction of pyrrol. When treated with acids, the red resinous matter was deposited, and the filtered fluid, on treatment with potash, evolved the smell of different members of the pyridine series, according to the boiling point of the fraction selected for the experiment. The oil containing pyrrol was now subjected to a systematic fractionation; and it was found, after several rectifications, to manifest a decided tendency to concentrate itself towards a fixed point, the fractions collected between 270° and 280°, and 280° and 290°, greatly exceeding the others in bulk. The oil obtained at these temperatures was perfectly transparent and colourless when freshly distilled, but soon acquired a brown colour, though much less rapidly than the crude substance. When agitated with very dilute acids, a certain portion of it immediately dissolved, but the remainder was very slowly acted upon, and required a large excess of acid, and much shaking, in order to make it dissolve, which, however, it eventually did completely. This fact appearing to indicate that the substance

* Poggendorf's Annalen, vols. xxxi. and xxxii.

was a mixture, it was shaken up with a small quantity of dilute acid, and the watery solution withdrawn. On the addition of caustic potash to this solution, an oil separated, which had the smell of picoline mixed with that of pyrrol. For the purpose of separating this picoline, the whole of the larger fractions were mixed and shaken up with a small quantity of very dilute sulphuric acid, and the solution, after being siphoned off, was replaced by another quantity, and this was repeated a third time. The oil was thus diminished by about a third of its bulk, and the whole of the picoline or other bases appearing to have been removed, it was carefully dried by means of sticks of caustic potash, and again rectified, when its boiling point was found to have been materially reduced. It began to boil at much the same temperature as the crude oil, but the largest fraction was now collected between 270° and 280°, while that which boiled above 290° formed only a very small proportion of the whole; and after fifteen rectifications, it was obtained in such a state that it distilled almost entirely between 274° and 280°. In this condition it is a transparent and colourless oil, slowly acquiring a brown colour when exposed to air and light. It has a strong fetid smell, quite distinct from that of picoline, and a hot pungent taste. A piece of fir-wood, dipped in hydrochloric acid brought near its vapour, instantly acquires a fine red colour. When boiled with a dilute acid, it is immediately converted into a red resinous mass, which fills the fluid so completely, that the vessel containing it may be inverted without anything escaping. The fluid filtered from this substance is brown, and contains a small quantity of it in solution. After boiling for some time, so as to get rid of a peculiar smell which adhered to the fluid, and decompose the last traces of pyrrol, caustic potash was added, when the smell of ammonia, faintly contaminated with that of picoline, was evolved. The solution having been distilled, the ammonia was saturated with hydrochloric acid and bichloride of platinum added, when the platinochloride of ammonium was immediately precipitated, and the filtrate, on further evaporation, yielded an additional quantity of that salt, along with some indications of a more soluble platinum compound. For a long time I considered the oil prepared by the process now detailed to be pyrrol in a state of as great purity as it was possible to obtain it; and, as will be afterwards seen, it gave in different preparations, analytical results in perfect accordance with one another, and with its true formula; but in the course of examining the effect of different reagents upon it, it was found that caustic potash exerted a very singular and perfectly unique action, disclosed the presence of a small quantity of some impurity, and afforded the means of removing it, when the properties of the pyrrol underwent a very remarkable change. When pyrrol is mixed with five or six times its weight of caustic potash in coarse powder, and heated over the lamp in a flask fitted with a long tube, it at first cohobates very freely; but if the temperature be gradually raised, the fluid is found to distil up into the tube much less readily, and at

length the bottom of the flask may be heated nearly red hot, while a very insignificant quantity of oil distils up. In performing this process, glass flasks were corroded by the caustic potash long before the action was complete, and it was found very convenient to employ copper flasks made by the electrotype process. A plaster of Paris mould was taken from a glass flask of convenient size and shape; and from that a wax cast was made and electrotyped in the usual way. After about a week the copper was sufficiently thick for use. In such flasks pyrrol was boiled for a day or two with caustic potash, the heat being raised as high as an Argand or Bunsen's gas-lamp would bring it. A bent tube was then fitted into the mouth of the flask, and the heat again applied, so as to distil off all the oil that could be obtained. The distillate had the smell of pyrrol mixed more or less distinctly with that of picoline, and the preponderance of the latter smell depended on the quantity of potash having been sufficiently large to retain the true pyrrol, which, however, it was not possible to do entirely, even when a very large excess of potash was used. When the whole of this oil had distilled, the bent tube was removed from the mouth of the flask, and the still fluid potash poured out on a copper plate.

On cooling, it solidified into a hard white mass with a yellowish tinge, which, when perfectly dry had no smell, but it was only necessary to breathe upon it to cause it to exhale a delightful etherial and fragrant odour, not unlike that of chloroform, but softer and less pungent. When thrown into water the potash gradually dissolved, and a transparent and colourless oil collected on the surface of the solution, from which it was separated either by a pipette or by distillation. The potash solution on saturation with sulphuric acid evolved the smell of a fatty acid, and when distilled, yielded a fluid which reddened litmus strongly, and had a smell resembling that of valerianic acid. The distillate was saturated with carbonate of soda, and the solution evaporated to complete dryness and extracted with absolute alcohol. The alcoholic fluid was again evaporated, the residue dissolved in water, and a quantity of solution of nitrate of silver insufficient for complete precipitation added to it, and the precipitate was collected on a filter and washed. Another quantity of nitrate of silver was then added, and the precipitate collected, and finally, enough of the nitrate was used to throw the remainder of the fatty acids in the fluid. In this way three different silver salts were obtained, which were separately analysed. The first precipitate gave:—

{ 6·273 grains of silver salt gave
{ 6·510 ... carbonic acid and
{ 2·452 ... water.

{ 5·192 grains of silver salt gave
{ 2·697 ... silver.

	Experiment.	Calculation.		
Carbon,	28·30	28·71	C_{10}	50
Hydrogen,	4·34	4·31	H_9	9
Silver,	51·94	51·67	Ag	108
Oxygen,	...	15·31	O_4	32
		100·00		209

which corresponds completely with the valerianate of silver. The second precipitate was manifestly a mixture, and gave variable quantities of silver, generally about 2 per cent. under that required by the valerianate. But the third precipitate consisted of propionate of silver, as shown by the subjoined analyses:—

I. 5·002 grains of the third precipitate gave 2·993 grains silver.
II. 4·796 ... another preparation gave 3·848 ...

	Experiment.			Calculation.		
	I.	II.	Mean.			
Carbon,	...	...	...	19·89	C_6	36
Hydrogen,	...	...	...	2·76	H_5	5
Silver,	59·83	59·38	59·50	59·66	Ag	108
Oxygen,	...	...	...	17·69	O_4	32
				100·00		181

It thus appears that the crude pyrrol contained a small quantity of some substances yielding valerianic and propionic acids when acted on by potash. The exact nature of these compounds it was impossible to determine, as their quantity was extremely minute, and the silver salts obtained from a very considerable quantity of pyrrol, were no more than sufficient for the analyses just detailed.

The fragrant pyrrol separated from the potash solution by distillation is transparent and colourless, when freshly prepared, but acquires a brown colour by exposure to the air. Its taste is hot and pungent, and its smell pleasant and etherial, and recalls that of chloroform. It is sparingly soluble in water, but readily in alcohol, ether, and the oils. It is insoluble in alkaline solutions, but the acids dissolve it, although not very rapidly. Its specific gravity is 1·077, and it boils at 271°. It gives the remarkable reaction on fir-wood described by Runge in a very powerful manner. The reaction is best obtained by dipping a piece of fir-wood in concentrated commercial hydrochloric acid, and holding it near a vessel containing pyrrol, or in a current of its vapour; a pale pink colour immediately makes its appearance, and gradually deepens to an intense carmine. All kinds of fir-wood do not produce the reaction equally well, and it appears to depend in some way upon the resin, for if fir saw-dust be extracted by alcohol or ether, and bits of cotton or linen cloth dipped in the solution, they acquire the property of becoming red, when exposed to pyrrol vapour, after having been moistened with hydrochloric acid, although the colour is by no means so brilliant

as that developed on the wood itself. When agitated with cold dilute acids, pyrrol dissolves unchanged, but on heating the solution, it deposits a red flocky substance, and if not too dilute, the whole is converted into a gelatinous mass, so that the vessel may be inverted without anything escaping. The same change takes place in the cold, when the acid solution is kept for some days. When bichloride of platinum is added to a cold hydrochloric solution of pyrrol, it instantly becomes dark coloured, and in the course of a few minutes an abundant black precipitate, containing platinum, is deposited. When boiled with sesquichloride of iron, the solution becomes first green, and finally black. Bichromate of potash also decomposes it with the formation of an abundant black precipitate, and sulphate of copper, when heated with it for some time, acquires a green colour, and a small quantity of a black powder is deposited. It is rapidly oxidized by nitric acid, with the evolution of abundant red fumes, and formation of a dark-red solution, which, when diluted, permits a yellow resin to fall. By long-continued ebullition, oxalic acid is produced. An alcoholic solution of pyrrol gives white precipitates with corrosive sublimate and chloride of cadmium, but it does not precipitate the metallic oxides generally.

The combustion of pyrrol was very easily effected, and the results are subjoined. The first six analyses were those of crude or fetid pyrrol, and are all from different preparations except the first two. The last is that of the fragrant pyrrol.

I.	5·805 grains of pyrrol gave		
	15·250	...	carbonic acid and
	4·070	...	water.
II.	3·675 grains of pyrrol gave		
	9·625	...	carbonic acid and
	2·550	...	water.
III.	5·250 grains of pyrrol gave		
	13·830	...	carbonic acid and
	3·675	...	water.
IV.	4·033 grains of pyrrol gave		
	10·590	...	carbonic acid and
	2·922	...	water.
V.	4·706 grains of pyrrol gave		
	12·345	...	carbonic acid and
	3·307	...	water.
VI.	5·280 grains of pyrrol gave		
	13·845	...	carbonic acid and
	3·676	...	water.
VII.	5·213 grains of fragrant pyrrol gave		
	13·675	...	carbonic acid and
	3·649	...	water.

	I.	II.	III.	IV.	V.	VI.	VII.	Mean.
Carbon, . .	71·64	71·42	71·84	71·61	71·54	71·51	71·54	71·58
Hydrogen, .	8·17	7·71	7·77	8·05	7·80	7·74	7·77	7·85
Nitrogen, .	...	...	...	...	...	...	...	20·57
								100·00

These results correspond with the formula $C_8 H_5 N$, which requires the following numbers:—

8 eq. carbon	= 48	71·64
5 ... hydrogen,	= 5	7·46
1 ... nitrogen,	= 14	20·90
	67	100·00

As none of the compounds of pyrrol are sufficiently definite to admit of their being used for fixing its atomic weight, recourse was had to the determination of the density of its vapour for this purpose, and three experiments were made at different stages of the investigation. The first was made after the pyrrol had received six rectifications and one treatment with acid, and its deviation from that required by theory showed that the material was not yet quite pure. The second, made after fourteen rectifications, and agitation with three successive portions of sulphuric acid, showed a close approximation to the theoretical number, while the third, made with the fragrant pyrrol, was as exact as could be desired. The details are as follow:—

	I.	II.	III.
Temperature of the air, . .	16° c.	11°	13°
... vapour, . .	198°	186°	201°
Excess of weight of the balloon, . .	0·2285 grammes.	0·2185	0·1610
Capacity of do.	324·5 c. c.	328·5	303
Barometer, ...	767 m. m.	744	764
Residual air, ...	0	1·5	4
Density of the vapour, . . .	2·52	2·49	2·40

The formula $C_8 H_5 N$ requires:—

8 vol. carbon vapour,	0·8290 × 8	= 6·6320
10 ... hydrogen, ...	0·0692 × 10	= 0·6920
2 ... nitrogen, ...	0·9713 × 2	= 1·9426
		9·2666

$$\frac{9·2666}{4} = 2·31$$

Although the properties of the pyrrol now described are entirely distinct from those attributed to this substance by Runge, it cannot be doubted that they are really identical, although it is equally unquestionable that he never isolated his pyrrol, but merely obtained a small quantity of it held in solution by some gas, most probably a hydrocarbon. For this reason I think it right to retain his name,

although it is not formed in accordance with the received nomenclature of organic compounds, the more especially as it would be difficult, in the present state of our knowledge, to find another which would not be open to many objections. As far as its properties and chemical relations go, pyrrol approaches more nearly to the volatile organic bases than to any other class of nitrogenous compounds, but its basic properties are extremely weak, as it has no effect on test papers, and though soluble in dilute acids, can be expelled from the solution at the boiling heat. It forms, however, compounds with corrosive sublimate and chloride of cadmium, both of which are easily decomposed.

Mercury Compound of Pyrrol.—This substance is obtained by mixing alcoholic solutions of pyrrol and corrosive sublimate, when it is immediately precipitated as a white powder with a somewhat crystalline appearance, insoluble in water, and sparingly soluble in cold alcohol. It is more soluble on boiling, but is then partially decomposed. Excess of corrosive sublimate appears also to act upon it in some way, as the solution from which it has been deposited acquires, on standing, a dark red, and sometimes a fine purple colour, due, in all probability, to the oxidation of pyrrol. The substance employed for analysis was dried *in vacuo*, and was from different preparations:—

I. { 7·186 grains of mercury compound gave
2·079 ... carbonic acid.

II. { 8·906 grains of mercury compound gave
2·472 ... carbonic acid and
0·652 ... water.

III. { 7·131 grains of mercury compound gave
4·77 ... mercury.

	Experiment.		Calculation.		
	I.	II.			
Carbon,	7·89	7·57	7·88	C_8	48
Hydrogen,	...	0·81	0·82	H_5	5
Nitrogen,	...	...	2·31	N	14
Chlorine,	...	...	23·31	Cl_4	142
Mercury,	...	66·89	65·68	Hg_2	400
			100·00		609

Corresponding with the formula $C_8 H_5 N + 2 Hg Cl_2$.

Cadmium Compound of Pyrrol is obtained as a white crystalline powder, when alcoholic solutions of pyrrol and chloride of cadmium are mixed. It is insoluble in water, but dissolves readily in hydrochloric acid. It is rapidly decomposed when heated, either dry or in suspension in water or alcohol. Its analysis gave

{ 5·673 grains of cadmium salt gave
4·837 ... carbonic acid and
1·119 ... water.

	Experiment.	Calculation.		
Carbon, ...	23·25	23·50	C_{16}	96
Hydrogen, ...	2·19	2·44	H_{10}	10
Nitrogen, ...	...	6·87	N_2	28
Cadmium, ...	...	41·12	Cd_3	168
Chlorine, ...	...	26·07	Cl_3	106·5
		100·00		408·5

This agrees pretty closely with the formula $2(C_8H_5N)+3CdCl$.

Products of the Decomposition of Pyrrol.

The decompositions of pyrrol have not led to results as definite as might have been anticipated; and I have therefore restricted myself to the examination of the red matter produced by the action of acids, and even that has been attended with no little trouble and difficulty.

Pyrrol Red.—This substance, as has already been frequently observed, is produced whenever pyrrol is boiled with an excess of acid; but notwithstanding the apparently definite nature of the change, it is extremely difficult to obtain it of uniform composition. This is due in part to its tendency to retain a small quantity of acid, and in part also to the fact that continued boiling produces a farther action, attended by the production of a dark colour in the acid liquid. When this occurs, the red matter gives very variable results when analysed, and hence, owing to the impossibility of ascertaining the exact length of time during which the fluid should be boiled to insure complete formation of the red matter, without going too far, the results of the analyses are by no means as concordant as might be desired. After a good many trials, it was found that the most successful results were obtained in the following manner:—Pyrrol was dissolved with the aid of brisk agitation in sulphuric acid diluted with from four to six parts of water, and the solution heated over the gas flame, while the flask was constantly shaken. As soon as the red matter had separated in distinct flocks, it was thrown on a filter and rapidly washed with boiling water, until the acid was almost entirely removed, during which process the pyrrol red acquired a slightly brown colour on the surface. A small quantity of diluted caustic potash was then poured upon the filter, when the product immediately became of a fine orange colour, which it retained after having been washed free of potash.

Pyrrol red is a fine, light, porous substance, with an orange-red colour, which becomes slightly brown by exposure to the air, especially when heated. It is insoluble in water, and is not readily moistened by that fluid. It is slightly soluble in cold, more so in boiling alcohol; and is again deposited on cooling in amorphous flocks. It is sparingly soluble in ether. Neither acids nor alkalies dissolve it, but if boiled with them for some time it is decomposed. Nitric acid oxidizes it, with the production of a resinous substance; and if the action be continued for a sufficient length of time, oxalic acid is found in the solution. When heated in

close vessels it yields an oil of an extremely offensive odour, and which gives the reactions of pyrrol, while a bulky charcoal is left in the retort. In the open air it catches fire, and burns readily. When exposed to 212° in the water-bath, it gains weight, owing to slow oxidation; and the portion used for analysis was therefore dried *in vacuo*. The results were—

I. { 6·381 grains of pyrrol red dried *in vacuo* gave
16·770 ... carbonic acid and
4·172 ... water.

II. { 6·440 grains of pyrrol red gave
0·846 ... nitrogen.

III. { 6·910 grains of pyrrol red gave
18·816 ... carbonic acid and
4·172 ... water.

IV. { 6·043 grains of pyrrol red gave
16·054 ... carbonic acid and
3·764 ... water

V. { 6·578 grains of pyrrol red gave
... nitrogen.

VI. { 6·132 grains of pyrrol red gave
16·248 ... carbonic acid and
3·793 ... water.

	I.	II.	III.	IV.	V.	VI.	Mean.
Carbon, ...	71·52	...	71·77	72·45	...	72·20	71·98
Hydrogen, . . .	7·29	...	6·70	6·66	...	6·87	6·88
Nitrogen, . . .	...	13·14	...	...	14·05	...	13·58
Oxygen, ...	...	...	...	...	...	...	7·56
							100·00

These results approximate most closely to the formula $C_{24}H_{14}N_2O_2$, which requires

24 eq. carbon, . . .	144	71·28
14 ... hydrogen, . . .	14	6·93
2 ... nitrogen, . . .	28	13·86
2 ... oxygen, . . .	16	7·93
	202	100·00

It is true that the numbers obtained by analysis do not accord well with this formula, and in particular the carbon is materially in excess, but this is undoubtedly due to a further decomposition produced by boiling; for if the heat be continued for some time during its preparation, the red matter acquires a dark-brown colour, and contains as much as 74 per cent. of carbon. The nature of the change by which the red matter is produced is readily intelligible, and is thus represented:—

3 equivalents pyrrol, . . .	$C_{24}H_{15}N_3$
−1 eq. ammonia, ...	H_3N
	$C_{24}H_{12}N_2$
+ 2 eq. water, ...	H_2O_2
1 eq. pyrrol red, ...	$C_{24}H_{14}N_2O_2$

The formation of ammonia during this decomposition was demonstrated by distilling the acid filtrate from the red substance with potash. The distillate, which had the smell of ammonia contaminated with an empyreumatic odour, and sometimes with that of picoline, was saturated with hydrochloric acid, and evaporated with excess of bichloride of platinum to nearly complete dryness. Octahedral crystals of platinochloride of ammonium were deposited, which were examined under the microscope, and found to be free from any other salt.

The quantity of pyrrol contained in bone-oil is far from inconsiderable, and now that its properties have been investigated, it is easy to see that a great deal must have been destroyed during the treatment by which the crude bases were extracted. As my previous investigation of the picoline from coal-tar had shown that its neutral sulphate is converted into bisulphate by boiling, I took care to add to the crude sulphates extracted by agitating bone-oil with sulphuric acid, a large excess of acid before boiling it for the purpose of separating pyrrol; and in this way large quantities of the red matter in an impure state were produced during the early part of the investigation. It was only after I had advanced some way in the investigation that the cause of its formation became intelligible, and the crude sulphates were then distilled without the addition of acid, and the pyrrol mixed with empyreumatic oils and bases of the picoline series was obtained in quantity sufficient for investigation. The difficulty experienced in removing the last traces of pyrrol from the bases was very great, and it was necessary to boil the solution for several days; but I have now found that oxidizing agents, such as nitric acid, or, still better, bichromate of potash, offer invaluable means of purification, as they decompose the pyrrol without producing the slightest effect on the bases.

In the present and preceding parts of this investigation, I have directed attention to the basic constituents of bone-oil. In the next part, I propose to treat of its non-basic constituents, in the investigation of which some progress has already been made. In particular, it has been found that, by repeated rectifications, a fine volatile fluid, boiling as low as 150° Fahr., is obtained. This oil consists of at least two different substances, separable by means of a freezing mixture, which causes the fluid to divide into two perfectly distinct strata, with a well-marked line of separation. The higher fractions do not present this peculiarity, but they are also complex, containing benzene, and apparently some of its homologues, along with the alcohol radicals of the fatty series, and also nitrogenous compounds decomposable by alcoholic solution of potash and by sodium.

XXXVI.—*On the Application of the Theory of Probabilities to the Question of the Combination of Testimonies or Judgments.* By George Boole, LL.D., Professor of Mathematics in Queen's College, Cork. Communicated by Bishop Terrot.

(Read 19th January 1857.)

1. The method for the solution of questions in the theory of probabilities applied in this paper, is that which was developed by the author in a treatise entitled, "An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and Probabilities." The practical object of the paper is to deduce from that method certain conclusions relating to the combination of testimonies or judgments. Beside this, however, it will have a speculative reference to some more general questions connected with the theory of probabilities; and especially to the following question, viz.: To what extent the different modes in which the human mind proceeds, in the estimation of probability, may be considered as mutually confirming each other,—as manifestations of a central unity of thought amid the diversity of the forms in which that unity is developed.

The special problems relating to the combination of testimonies or judgments which are considered in this paper are the following: 1*st*, That in which the testimonies to be combined are merely differing numerical measures of a physical magnitude, as the elevation of a star, furnished by different observations taken simultaneously; 2*dly*, That in which the testimonies or judgments to be combined relate not to a numerical measure, but to some fact or hypothesis of which it is sought to determine the probability,—the probabilities furnished by the separate testimonies or judgments constituting our data.

2. I have, in the treatise to which reference has been made, described the method which will be practically applied in this paper as a general one. It will, I think, ultimately appear that there is a true and real sense in which the propriety of the description may be maintained. But at present I am anxious to qualify the appellation, and to speak of the method as general only with respect to problems which have been resolved into purely logical elements, or which are capable of such resolution. A more thorough analysis of the mental phenomena of expectation will, I think, tend to establish the position that all questions of probability, in the mathematical sense, admit of being resolved into primary elements of this nature, or, to speak more strictly, admit of being adequately represented by other problems whose elements are logical only. Postponing the consideration of this question, I will first endeavour to explain what is meant by the logical elements of a problem, and how the consideration of such elements affects the mode of its solution.

I regard the elements of a problem relating to probability as logical, when its data and its quæsitum are the probabilities of *events*. The reason for this appellation will shortly be seen. In *expression*, events may be distinguished as simple or compound. A simple event, *i.e.*, an event simple in expression, is one which is expressed by a single term or predication; a compound event, one which is formed by combining the expressions of simple events. "It rains,"—"it thunders," would be simple events; "it rains and thunders,"—"it either rains or thunders," &c., would be compound events. The constructions by which such combinations are expressed, although they belong to language, have their foundations in Logic. Thus the conjunctions *and*, *either*, *or*, &c., express merely certain operations of the faculty of Conception, the entire theory of which belongs to the science of Logic. The calculus of Logic, to which I shall have occasion to refer, is a development of that science in mathematical forms, in which letters represent things, or events, as subjects of Conception, and signs connecting those letters represent the operations of that faculty, the laws of the signs being the expressed laws of the operations signified. It is simply a mistake to regard that calculus as an attempt to reduce the ideas of Logic under the dominion of number. Such are the grounds upon which the class of problems to which I have referred are said to involve logical elements. The description is, however, not entirely appropriate, for the problems, as they are concerned with probabilities, in the mathematical acceptation of that term, involve numerical as well as logical elements; but it is by the latter that they are distinguished, and of them only is account taken in the nomenclature.

Thus, as an illustration of what has been said, that problem would be composed of logical elements, which, assigning for its numerical data the probabilities of the throwing an ace or six with each single die, should propose to determine the probability that the issue of a throw with two dice should be two aces, or that it should be an ace and a six, or that it should be either two aces or an ace and a six; and so on for any conceivable throw with any number of dice.

3. In the above example, the events whose numerical probabilities are given are simple events, of which the event whose probability is sought is a logical combination. But it might happen that the former events were themselves combinations of simple events. For instance, the data might be the probabilities that certain meteorological phenomena, as rain, thunder, hail, &c., would occur in certain definite combinations, and the object sought might be the probability that they would occur in certain other combinations; all these combinations being, such as it is within the province of language to express by means of conjunctions, and of the adverb *not*. Now this would still be a problem whose elements are logical.

4. But there are questions universally recognised as belonging to the theory of probabilities, whose elements cannot, in their direct significance, be regarded

as logical. The problem of the reduction of astronomical observations belongs to this class. Two observers, equally trustworthy, take an observation at the same place and time of the altitude of a star. One of them declares that it is 50° 20′, the other that it is 50° 22′. From these data, what shall we regard as the most probable altitude? We cannot, in this case, directly affirm that the numerical data are measures of probability at all. They are conflicting measures of a physical magnitude. And that which is sought is not the measure of a probability, but the most probable measure of the same magnitude. This is a problem evidently of a different kind from the one which we last considered. And accordingly it will be found that the principles of solution which have been actually applied to it are different from, perhaps we ought rather to say supplementary to, those which have sufficed for the solution of the others. In the problem of the dice, we have only to apply, and that directly, such principles as the following, viz., that when the probability of the occurrence of an event is p, that of its non-occurrence is $1-p$; that if the probabilities of two independent events are p and q, that of their concurrence is pq; and so on. In the reduction of the conflicting elements of the observers' problem, another and quite distinct principle is usually employed, viz., the principle of the arithmetical mean, which affirms that if two different values are, on equal authority, assigned to a magnitude which is in itself single and definite, the mind is led to consider the arithmetical mean of those values as more likely to be its true measure than any other value. This is not the only principle which has been employed for the reduction in question. We shall refer to others. But it may justly be regarded as the most obvious of all which have been employed; and there is ground for considering it, as some eminent writers have expressly done, as primary and axiomatic in its nature.

5. The following is the typical form of problems whose elements are logical. If we represent the simple events involved in their expression by x, y, z, &c., then may all their data (we will suppose the number of data to be n) be expressed, in accordance with the principles of the calculus of Logic, under the general forms

$$\text{Prob. } \phi_1 (x, y, z \ldots) = p_1, \qquad \text{Prob. } \phi_2 (x, y, z \ldots) = p_2, \ \ldots \ \text{Prob. } \phi_n (x, y, z \ldots) = p_n,$$

and the quæsitum, or object sought, will be the value of

$$\text{Prob. } \psi (x, y, z \ldots),$$

where $\phi_1, \phi_2, \ldots \phi_n$ and ψ denote different but given logical functions of x, y, z.

Although the method for the solution of questions in the Theory of Probabilities whose elements are logical has been developed at considerable length in a special chapter of the *Laws of Thought*, yet much that is essential for its proper and distinctive exhibition, has only been discovered since the publication of that work. For this reason it will be proper to offer some account here of the principles upon which the method rests.

6. I define the mathematical probability of an event as the ratio which the number of distinct cases or hypotheses favourable to that event bears to the whole number of distinct cases possible, supposing that to none of those cases the mind is entitled to give any preference over any other. Fundamentally, this definition agrees with that of LAPLACE. "La théorie des hazards consiste," he remarks, "à réduire tous les évènements du même genre a un certain nombre de cas également possibles c'est à dire tels que nous soyons également indécis sur leur existence et à déterminer le nombre de cas favorables à l'évènement dont on cherche la probabilité. Le rapport de ce nombre a celui de tous les cas possibles est la mesure de cette probabilité."—*Essai Philosophique sur les Probabilités.*

It is implied in this definition, that probability is relative to our actual state of information, and varies with that information. Of this principle LAPLACE gives the following illustration:—"Let there be three urns, A, B, C, of which we are only informed that one contains black and the other white balls; then, a ball being drawn from C, required the probability that the ball is black. As we are ignorant which of the urns contains black balls, so that we have no reason to suppose it to be the urn C rather than the urn A or the urn B, these three hypotheses will appear equally worthy of credit, but as the first of the three hypotheses alone is favourable to the drawing of a black ball from C, the probability of that event is $\frac{1}{3}$. Suppose now that, in addition to the previous data, it is known that the urn A contains only white balls, then our state of indecision has reference only to the urns B and C, and the probability that a ball drawn from C will be black is $\frac{1}{2}$. Lastly, if we are assured that both A and B contain white balls only, the probability that a black ball will issue from C rises into certitude."—*Essai Philosophique sur les Probabilités*, p. 9.—(*Phil. Mag.*, p. 433.) Our estimate of the probability of an event varies not absolutely with the circumstances which actually affect its occurrence, but with our knowledge of those circumstances.

7. When the probabilities of simple events constitute our only data, we can, by virtue of the above definition, determine the probability of any logical combination of those events, and this either, 1*st*, absolutely; or, 2*dly*, conditionally. The reason why we can, in this case, more immediately apply the definition is, that not only is no connection expressed among the events whose probabilities are given, but none is implied, nor is any restraint imposed upon their possible combinations. This, as we shall see, is not the case when the data are the probabilities of compound events.

As an example, let us suppose that the probability of the conjunction of two events, x and y, is required, the data being simply that the probability of the event x is p, and that of the event y is q. Or, to express the problem in a form which we shall hereafter generally employ:

Given	Prob. $x=p$,	Prob. $y=q$,
Required	Prob. xy.	

Let a be the number of distinct cases favourable to the event x, out of m distinct cases equally possible, from the comparison of which the probability p has been assigned to the event x. In like manner let b be the number of distinct cases favourable to the event y, out of n distinct cases equally possible, from the comparison of which the probability q has been assigned to the event y. Then,

$$\frac{a}{m}=p \qquad \text{and} \quad \frac{b}{n}=q.$$

Now the conjunction xy can only come to pass through the combination of some one of the a cases in which x happens, with some one of the b cases in which y happens, at the same time that we have an equal right to suppose that any one of the m cases in which x happens or fails may combine with any one of the n cases in which y happens or fails. To none of these combinations is the mind entitled to attach any preference over any other, Hence there exist ab distinct cases favourable to the conjunction of x and y out of a total of mn distinct and equally possible cases. Thus, by the definition, the probability of the conjunction of x and y will be represented by the product $\frac{ab}{mn}$ or pq.

Here the question may be asked,—Does, then, no difference exist between the case in which the events x and y are known to be independent, and that in which we are simply ignorant of the existence of any connection between them? I reply that there is none, so far as the numerical estimation of probability is concerned. There is, however, an important difference as respects the practical value of the numerical result. If the events x and y are known to be independent, and to have probabilities p and q, we know that, in the long run, the conjunction xy will tend to recur with a frequency which will be proportional to the magnitude of the fraction pq, We do not know that this will be the case if we are simply ignorant of any connection between x and y. This is the difference referred to, and it is an important one. But it does not affect the calculation of probability as flowing from the definition of its numerical measure.

8. As from the data Prob. $x=p$, Prob. $y=q$, we deduce Prob. $xy=pq$, so from the same data we should have, adopting the language of the calculus of Logic,

$$\text{Prob. } x(1-y)=p(1-q) \qquad \text{Prob. } (1-x)(1-y)=(1-p)(1-q),$$

and so on. Here $x(1-y)$ denotes the compound event which consists in the occurrence of x conjointly with the non-occurrence of y; $(1-x(1-y)$, the compound event which consists in the joint non-occurrence of both x and y.

Extending this mode of investigation, we arrive at the theorem

$$\text{Prob. } \phi(x, y, z\ldots) = \phi(p, q, r\ldots) \quad . \quad . \quad . \quad . \quad (1)$$

where x, y, z, &c., denote any simple events whose probabilities (our only data) are p, q, $r\ldots$, and $\phi(x, y, z\ldots)$ denotes any event which can be expressed by

means of the simple events x, y, z, &c., in accordance with the notation of the calculus of Logic.

By the above theorem the probability of any compound event is determined absolutely, when the probabilities of its simple components are given.

9. And by the same mode of investigation, the probability of any combination may be determined conditionally, *i.e.*, the probability which the combination will have under a given condition consisting in the happening of some other combination. Thus, if our data are as before,

$$\text{Prob. } x=p, \qquad \text{Prob. } y=q, \qquad \text{Prob. } z=r, \quad \&c.$$

and if we require the probability that if the event $\phi(x, y, z\ldots)$ present itself, the event $\psi(x, y, z\ldots)$ will be present at the same time, we may demonstrate the following result, viz.:—

Prob. that if $\phi(x, y, z\ldots)$ happen, $\psi(x, y, z\ldots)$ will be present also

$$= \frac{\chi(p, q, r\ldots)}{\phi(p, q, r\ldots)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where the form of the function χ is determined by multiplying together, according to the principles of the calculus of Logic, the functions $\phi(x, y, z\ldots)$ and $\psi(x, y, z\ldots)$, and representing the result by $\chi(x, y, z\ldots)$.—(*Laws of Thought*, p. 258, Prop. I.)

10. I postulate that when the data are not the probabilities of simple events, we must, in order to apply them to the calculation of probability, regard them, not as primary, but as derived from some anterior hypothesis, which presents the probabilities of simple events as its system of data, and exhibits our actual data as flowing out of that system, in accordance with those principles which have already been shown to be involved in the very definition of probability.

The ground of this postulate is, that to begin with the simple and proceed to the complex, seems to be, in all questions involving combinations such as we are here concerned with, a necessary procedure of the understanding. The calculation of probability depends upon combinations subject to a peculiar condition, viz., that they shall always present to us a series of cases or hypotheses, to none of which the mind is entitled to attach any preference over any other. We cannot, in endeavouring to ascend from the complex to the simple, secure the maintenance of this condition; but we can do so in descending from the simple to the complex. We have had an illustration of this truth in the reasoning by which we deduced the expression for the probability of the complex event xy from the probabilities of the simple events x and y, supposed to be given. And the methods which have been actually employed in the solution of problems whose immediate data were not the probabilities of simple events, have in fact rested upon the postulate above referred to. Thus in questions relating to juries, the immediate data are the probabilities, founded upon continued observation, that a decision will be unanimous, or that it will be pronounced by a given majority, &c.

But it is usual, in solving these problems, to regard such events as compound, and to derive them from a hypothesis which presents as its scheme or system of data, the probabilities of individual correctness of judgment in the members of the jury; the correctness of judgment in any such members being regarded as a simple event. And this mode of procedure is a very natural and very obvious one. For the degree of unanimity of a decision will so far depend upon the correctness of judgment in the members, that, if we knew what the probability of correctness in each member was, we could determine *à priori* the probability of any proposed measure of agreement in the body.

The only question which arises, indeed, is not concerning the necessity of the postulate, but concerning the mode in which it may be lawfully applied. How shall we lawfully construct the hypothesis by which the solution of a problem shall be made to depend upon the consideration of simple events. In answering this question, I will endeavour to show, 1*st*, upon what the construction of the hypothesis does not depend; 2*dly*, upon what it does depend.

11. The legitimate construction of the hypothesis in question cannot depend upon the accidents of language, or causes deeper than accident, which have led us to express particular things or events by simple terms, thus regarding them as simple events; and other events by combinations of these simple terms, thus presenting them as compound. The solution of a question in the theory of probabilities must depend upon the *information* conveyed in the data, not upon the peculiar elements and constructions of the language which is the vehicle of that information. Languages differ widely in these respects. Objects and events which in one language are expressed by simple terms, are in another expressed by combinations of simple terms. It is affirmed that a perfectly general method of solution must be independent of, and superior to, differences like these.

I will endeavour to illustrate this principle by an example. Let the problem to be resolved be the following. The probability of the concurrence of rain and snow is p, of the concurrence of snow and wind q and of the concurrence of wind and rain r; required the probability of the concurrence of wind, rain, and snow.

Now suppose that we had to interpret the problem into a language in which there were no simple terms corresponding to the simple terms "wind," "rain," "snow," but in which there were simple terms for the three first of the concurrences above described.

We may, for simplicity, suppose that language to be a dialect of English, and the concurrence of rain and snow to be represented in it by the term "sleet," the concurrence of snow and wind by the term "drift," and the concurrence of wind and rain by the term "storm."

The event whose probability is sought, viz., the concurrence of rain, snow, and wind, would, in such a language, be represented by the combination either of two of the terms above defined (as of sleet with drift), or of all the terms together,

since the presence of any two of the phenomena "sleet," "drift," "storm," implies that of the third, and involves the conjunction of the phenomena of rain, snow, and wind.

The data of the problem we are considering might then, in the imagined dialect, assume the following form :

The probability of sleet is p, of drift q, and of storm r; required the probability of the concurrence of the phenomena of sleet, drift, and storm.

But in this form the problem would not, in its data, express all the knowledge which the person using such language must possess of the connection of the events to which it related. He must know that it was impossible that any two of the events sleet, drift, and storm, should occur without the third, so that the problem, if so stated as to embody the same amount of actual knowledge as is conveyed in the previous statement of it, would assume the following form :

The probability of sleet is p, of drift q, of storm r, and these events are so connected, that no two of them can occur without the third occurring, What is the probability of their concurrence?

Now the principle affirmed declares that the solution of the problem must be the same whichsoever of these forms of statement we adopt.

As languages increase in affluence, the number of their simple terms becomes augmented, partly through the necessity of giving expression to new ideas, partly through the wish to give more convenient expression to definite and oft-recurring combinations of the old ones. With every term invented in subserviency to the latter purpose, a definition must be introduced. A dictionary, setting aside its philological portion (and even this not wholly), is a record of such definitions. As a consequence of such definitions of terms, spring up also propositions innumerable connecting these terms—propositions which in no degree add to the amount of our absolute knowledge, which are quite distinct from the discovered facts and laws of nature and of human history, but are merely logical deductions from the definitions. We might conceive of a language in which all possible combinations of ideas should be expressed by simple terms, with connecting definitions and propositions *ad infinitum*. The realization of such a conception is neither practicable nor desirable; but it is, nevertheless, the *limit* toward which all languages, which are not dead or decaying, do actually tend. The progressive action of this tendency does not affect the laws of expectation, neither, therefore, can it affect any consistent and scientific theory which is founded upon those laws.

We are not, therefore, permitted to assume that any events which, in the language of the problem, may be presented as simple events, must therefore be adopted as such into the hypothesis which is to form the basis of our method of solution. Nor, on the other hand, are we forbidden to employ transformations (sanctioned by the rules of Logic) which will have the effect of introducing an

entirely new scheme of simple events as the elements of the hypothesis in question.

12. To what conditions, then, must the hypothesis be subject? This question I now proceed to answer.

The hypothesis must be such that it may be consistently applied, without imposing upon the data any other conditions than those of possibility, *i.e.*, of accordance with a possible experience.

This principle is so obviously true, that it will only be needful to show how the conditions of possible experience are discovered. I shall subsequently show how their discovery limits and determines the hypothesis upon which the solution of questions in the theory of probabilities, whose elements are logical, depends.

The data of such problems are the probabilities of events. The object sought is also the probability of an event. The numerical values of these probabilities must be expressible by positive proper fractions. At any rate, they must not transcend the limits 0 and 1. This is one condition to which they are subject. Generally, however, there will exist other conditions dependent upon the mutual relations of the events whose probabilities are given.

Thus, if p were the probability that an event x will happen, q the probability that x and y will both happen, we have, as a necessary condition,

$$p \geqq q.$$

Again, if p were the probability that x and y will both happen, q the probability that they will both fail, we must have the condition,

$$p + q \leqq 1,$$

a condition which does not hold in the previous case.

I have, in the *Laws of Thought*, treated of these conditions, and of the principles by which they may be determined, in a special chapter (Cap. xix., *On Statistical Conditions*). A more simple, and at the same time perfectly general method, for their determination was afterwards discovered by me, and published in the *Philosophical Magazine*, Aug. 1854. As the method is of fundamental importance, I shall here illustrate it by an example, at the same time introducing a slight change in the mode of treatment, which leaves nothing to be desired in point of simplicity. The conditions to the discovery of which the method is applicable will be termed, in accordance with the language employed in the *Philosophical Magazine*,—the "Conditions of Possible Experience;" inasmuch, as whenever the numerical data of a problem are derived from actual experience these conditions will be satisfied, and whenever in data professing to be thus derived they are not satisfied, the presence of mistake or fraud may with certainty be affirmed.

Determination of the Conditions of Possible Experience.

13. To explain the method of effecting this object, by an example, I will first symbolically express the problem of Art. 11.

Let us then represent rain by x, snow by y, and wind by z. The problem in question then takes the following form:—

Given Prob. $xy=p$, Prob. $yz=q$, Prob. $xz=r$ . . (1)

Required Prob. xyz . . . . . (2)

The value required we shall represent by u. It is our present object, not to solve this problem, but to ascertain the conditions which must connect p, q, and r, in order that the data may be possible, with the corresponding limitations of u. For if u were itself determined by experience, it would be subject to conditions of possibility similar to those which govern p, q, and r.

Now let us write, resolving the events in the problem into the possible alternations out of which they are formed,

$$\text{Prob. } xyz=u, \quad \text{Prob. } xy\bar{z}=\lambda, \quad \text{Prob. } x\bar{z}y=\mu, \quad \text{Prob. } y\bar{z}x=\nu.$$

We have then

$$u+\lambda=p, \qquad u+\nu=q, \qquad u+\mu=r \qquad . \quad . \quad . \quad (3)$$

The first of these equations only expresses that the probability of the concurrence of x and y is equal to the probability of the concurrence of x, y, and z, and the probability of the concurrence of x and y without z. To the equations (3) we must now add the inequations

$$\left.\begin{gathered} u \gtreqless 0, \quad \lambda \gtreqless 0, \quad \mu \gtreqless 0, \quad \nu \gtreqless 0, \\ u+\lambda+\mu+\nu \lesseqgtr 1 \end{gathered}\right\} \quad . \quad . \quad . \quad (4)$$

expressing the conditions to which u, λ, μ, ν, 1*st*, as probabilities, and, 2*dly*, as probabilities which do not altogether make up certainty, are subject.

First, we will eliminate λ, μ, and ν. Their values found from (3) are

$$\lambda=p-u \qquad \mu=r-u \qquad \nu=q-u.$$

Substituting these in (4) we have

$$\begin{gathered} u \gtreqless 0 \qquad p-u \gtreqless 0 \qquad q-u \gtreqless 0 \qquad r-u \gtreqless 0 \\ p+q+r-2u \lesseqgtr 1, \end{gathered}$$

Whence,

$$\left.\begin{gathered} u \lesseqgtr p, \quad u \lesseqgtr q, \quad u \lesseqgtr r, \\ u \gtreqless 0, \quad u \gtreqless \frac{p+q+r-1}{2} \end{gathered}\right\} \quad . \quad . \quad . \quad (5)$$

Such are the conditions to which the quantity u is subject, conditions which the value of Prob. xyz must *à priori* satisfy.

To determine the conditions connecting p, q, and r, we must from (5) eliminate u. Now, if we have any two inequations of the form

$$u \lesseqgtr a \qquad u \gtreqless b$$

the only condition connecting a and b which they establish is,

$$a \overline{\overline{>}} b.$$

Applying this principle to (5), we have

$$p \overline{\overline{>}} 0 . \quad p \overline{\overline{>}} \frac{p+q+r-1}{2} .$$

$$q \overline{\overline{>}} 0 . \quad q \overline{\overline{>}} \frac{p+q+r-1}{2} .$$

$$r \overline{\overline{>}} 0 . \quad r \overline{\overline{>}} \frac{p+q+r-1}{2} .$$

These may be reduced to the somewhat simpler form

$$\left.\begin{array}{lll} p \overline{\overline{>}} 0 & q \overline{\overline{>}} 0 & r \overline{\overline{>}} 0 \\ p \overline{\overline{>}} q+r-1 & & \\ q \overline{\overline{>}} r+p-1 & & \\ r \overline{\overline{>}} p+q-1 & & \end{array}\right\} \quad . \quad . \quad . \quad . \quad (6)$$

Such are the conditions of possible experience in the data.

Suppose, for instance, it was affirmed as a result of medical statistics, that in two-fifths of a number of cases of disease of a certain character, two symptoms, x and y, were observed; in two-thirds of all the cases, the symptoms y and z were observed; and in four-fifths of all the cases, the symptoms x and y were observed; so that the number of cases observed being large, we might, on a future outbreak of the disease, consider the fractions two-fifths, two-thirds, and four-fifths, as the probabilities of recurrence of the particular combinations of the symptoms x, y, and z, observed. The above formulæ would show that the evidence was contradictory. For representing the respective fractions by p, q, and r, the condition

$$p \overline{\overline{>}} q+r-1$$

is not satisfied.

It is an evident consequence of the principle enunciated in Art. 11, that in determining the conditions of possible experience and of limitation, we may employ any *translated* form of the problem, just as well as the form in which it is originally expressed. Thus, if we take the translated form of the problem of that article, and represent sleet by s, drift by t, storm by u, we shall have as the data

$$\text{Prob. } s=p, \qquad \text{Prob. } t=q, \qquad \text{Prob. } u=r$$

with the conditions

$$st\bar{u}=0, \qquad ut\bar{s}=0, \qquad us\bar{t}=0 \quad . \quad . \quad . \quad . \quad (7)$$

the quæsitum being Prob. stu, which, as before, we shall represent by u.

Now if we write

$$\begin{array}{llll} \text{Prob. } stu=u, & \text{Prob. } s\bar{t}u=0, & \text{Prob. } sut=0, & \text{Prob. } s\bar{u}\bar{t}=\lambda \\ \text{Prob. } tu\bar{s}=0, & \text{Prob. } t\bar{u}\bar{s}=\mu, & \text{Prob. } us\bar{t}=\nu & . \quad . \quad . \quad (8) \end{array}$$

we have the following equations:—

$$\left.\begin{aligned} u+\lambda&=p\\ u+\mu&=q\\ u+\nu&=r \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

with the inequations

$$u \gtreqless 0, \quad \lambda \gtreqless 0, \quad \mu \gtreqless 0, \quad \nu \gtreqless 0, \quad u+\mu+\nu+\lambda \quad \lesseqgtr 1 \quad . \quad (10)$$

Determining from the equations λ, μ, ν, and substituting in the inequations, we get

$$u \gtreqless 0 \quad p-u \gtreqless 0, \quad q-u \gtreqless 0, \quad r-u \gtreqless 0, \quad p+q+r-2u \quad \lesseqgtr 1 \quad . \quad (11)$$

a system which agrees with that obtained by the previous investigation (5) Art. 13.

14. The general rule for the determination of the conditions of possible experience and of limitation in a question of probability may be thus stated.

Resolve the events whose probabilities are either given or sought, into the mutually exclusive alternatives which they involve. If the calculus of Logic is employed, this is done by development.

Represent the probabilities of these alternations by λ, μ, ν, &c., and express the probabilities given and sought by the corresponding sums of these quantities. This will furnish a series of *equations*, which we will suppose to be n in number.

Determine from these equations any n of the quantities λ, μ, ν, in terms of the others.

Substitute the values thus obtained in the *inequations*

$$\lambda \gtreqless 0 \quad \mu \gtreqless 0 \quad \nu \gtreqless 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\lambda+\mu+\nu \ldots \quad \lesseqgtr 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Eliminate in succession such of the quantities λ, μ, ν, . . as are left in the above inequations after the substitution.

The elimination of any quantity as τ from the inequations, is effected by reducing each inequation to the form $\tau \lesseqgtr a$, or to the form $\tau \gtreqless b$, and observing that two such forms as the above give $a \gtreqless b$.

If the "alternations" into which the events whose probabilities are given or sought are resolved, extend to all possible combinations of the simple events out of which they are formed, the inequations (2), must be replaced by the equation

$$\lambda+\mu+\nu \ldots = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The rest of the process will be the same as before.

In the form of the above method developed in the *Philosophical Magazine* the quantities λ, μ, ν, . . represent the probabilities, not of those alternations alone, which are contained in the events whose probabilities are given or sought, but of all possible alternations which can be formed, by combining the simple events x, y, z . . In this form, therefore, we have always an equation of the form (3), in the place of an inequation of the form (2). But though the result is the same, the form given to the method in this section is to be preferred, as it requires the elimina-

tions of a smaller number of symbols, except when the condition referred to in (3) is fulfilled, in which case, the methods are identical.

15. It remains to show how the conditions of possible experience as above determined, restrict us in the choice of the hypothesis, by the aid of which the final solution is to be obtained.

Taking for example the above problem of Art. 13, let us inquire whether it would be lawful to assume x, y, and z as the primary simple events of the problem.

If we make this assumption and then write

$$\text{Prob. } x=\alpha \qquad \text{Prob. } y=\beta \qquad \text{Prob. } z=\gamma$$

we find

$$\text{Prob. } xy=\alpha\beta \qquad \text{Prob. } yz=\beta\gamma \qquad \text{Prob. } zx=\gamma\alpha$$

whence comparing with (1) Art. 13—

$$\alpha\beta=p \qquad \beta\gamma=q \qquad \gamma\alpha=r$$

solving which equations we have

$$\alpha=\sqrt{\frac{qr}{p}} \quad \beta=\sqrt{\frac{rp}{q}} \quad \gamma=\sqrt{\frac{pq}{r}} \quad \therefore \text{ Prob. } xyz=\alpha\beta\gamma=\sqrt{pqr} \qquad . \qquad (4)$$

Now, α, β, γ, being by assumption probabilities, and therefore, lying numerically between the limits 0 and 1, we must have

$$qr \lessgtr p \qquad rp \lessgtr q \qquad pq \lessgtr r \qquad . \quad . \quad . \quad . \quad (5)$$

as the conditions to which p, q, and r (beside being fractional) must be subject. These conditions do not, however, agree with, and are not involved in, the conditions of possible experience, determined in (6) Art. 13. We may conclude, therefore, that the hypothesis upon which our solution is founded, involves elements, the introduction of which is unwarranted, and that the value of Prob. xyz determined is erroneous.

We may show, in fact, that the conditions (5) imply the conditions of possible experience, and something more. If $qr \lessgtr p$ then, *à fortiori*, $qr \lessgtr p+(1-q)(1-r)$ since $(1-q)(1-r)$ is essentially positive. Therefore,

$$qr \lessgtr p+1-q-r+qr$$

whence $$p \gtrless q+r-1$$

which is one of the conditions (6) Art 13. In the same way the other conditions in that article may be deduced from (5). The reverse reduction is, however, impossible.

16. The hypothesis upon which the method developed in the *Laws of Thought*, cap. xvii., for the solution of questions in the theory of probabilities whose elements are logical, is founded, seems to be the only one which satisfies the requirement referred to in Art. 12. It was not, however, upon such considerations as this,

that the method was founded. As presented in the *Laws of Thought*, it rests upon principles which, to my own mind, have something of an axiomatic character. Viewed in this light, its perfect accordance with the requirement above explained may be considered as a verification of it *à posteriori*. In itself, however, this accordance affords a sufficient ground of confidence in the legitimacy of the hypothesis. On the proof of this accordance I shall say something hereafter. At present I will only state the hypothesis, and show in what the accordance consists.

The hypothesis is the following:—Translating our problem by the aid of the calculus of logic into a language in which the events whose probabilities are given, appear as simple events subject to conditions founded on their definitions, Art. 11, we ascend above these simple events to another scheme of simple events, which are free, and which, when actually subjected to the conditions to which the before-mentioned simple events are necessarily subject, shall have the same probabilities, and shall in every respect take their place. The unknown probabilities of the free simple events, which form the elements of this hypothesis, must be so determined as to render the substitution possible, and to permit a formal construction of the problem, both in its data and by its quæsitum, out of those new elements.

The unknown probabilities being thus determined, the problem assumes a form in which its elementary data are the probabilities of simple events unrestricted by any condition. In this form the solution of the problem is possible by mere consequence of the fundamental definition of probability. The ground upon which this hypothesis was presented in the *Laws of Thought* was its intrinsic reasonableness. On this point I will only refer to my observations in the original work. The ground upon which, in the present essay, I wish to rest the hypothesis is, that it is the only one which does not impose upon the data other conditions than those of conformity with a possible experience. The conditions which must be fulfilled in order that p', q', &c., in the substituted and hypothetical data, may be measures of probability at all,—*i.e.*, may be positive proper fractions,—are precisely the conditions of possible experience in the original data. (See Appendix.)

17. The application of this hypothesis is so fully explained in the *Laws of Thought*, cap. xvii., that I shall here only describe the general method for the solution of questions in probabilities to which it leads, and show the connection which exists between the several parts of that method and the foregoing doctrine.

General Method.

Representing the problem to be solved under the form—

Given Prob. $\phi_1(x, y, z, \ldots) = p$ Prob. $\phi_2(x, y, z \ldots) = q$, &c.

Required Prob. $\psi(x, y, z \ldots)$

and expressing the unknown value of Prob. $\downarrow(x, y, z \ldots)$ by u, we form the logical equations:—

$$\phi_1(x, y, z \ldots) = s \qquad \phi_2(x, y, z \ldots) = t, \text{ \&c.}$$
$$\downarrow(x, y, z \ldots) = w$$

and hence, determining w as a developed logical function of $s, t \ldots$ we have a result of the form

$$w = A + 0B + \frac{0}{0}C + \frac{1}{0}D \qquad \ldots \qquad (1)$$

Here A, B, C, D are logical combinations of the simple events, s, t, &c., and the connection in which they stand to the event w and to each other is the following: A expresses those combinations of s, t, &c., which are entirely included in w,—*i.e.*, which cannot happen without our being permitted to say that w happens. B represents combinations which may happen, but are not included under w; so that when they happen, we may say that w does not happen. C represents those combinations, the happening of which leaves us in doubt whether w happens or not. D, those combinations, the happening of which is wholly interdicted.

Thus far we have only translated our problem into a language in which its data are the probabilities of simple events, viz.:—

$$\text{Prob. } s = p \qquad \text{Prob. } t = q, \text{ \&c.} \qquad \ldots \qquad (2)$$

The condition, founded on definition, to which these simple events are subject is,

$$D = 0$$

or, which amounts to the same thing,

$$A + B + C = 1$$

indicating that the combinations expressed by A, B, and C can alone happen. If we represent A + B + C by V, we have

$$w = A + \frac{0}{0}C \qquad \ldots \qquad (3)$$

with the condition

$$V = 1 \qquad \ldots \qquad (4)$$

Of these equations, the latter expresses the conditions to which the simple events, s, t, &c., are subject; the former expresses w as a logical combination of those events.

We now, in accordance with the hypothesis, ascend to a new scheme of simple events, s', t', &c., unrestricted by any condition, and possessed of unknown probabilities, p', q', which are to be so determined that when $s', t' \ldots$ are subjected to the same condition (4) to which $s, t \ldots$ are subject, they will have the same probabilities as $s, t \ldots$ The system of equations to which we are thus led, and which contains the implicit solution of the problem, is the following (*Laws of Thought*, cap. xvii., p. 267):—

$$\frac{V_s}{p} = \frac{V_t}{q} \ldots = \frac{A + C}{u} = V \qquad \ldots \qquad (5)$$

V_s being formed by selecting those terms from V, which contain s as a factor; V

those, which contain t as a factor, &c.; and then regarding s, t, &c., as algebraic quantities. From the system thus formed, we must determine u as a function of p, q . . and the arbitrary constant c, should it be present. This will be the solution of the problem.

The quantities s, t . . are the same as p', q' . . and represent the probabilities of the hypothetical simple events, represented by s', t' . . Accordingly, as probabilities, they must admit of being determined as positive proper fractions, and that the solution may not be ambiguous, they must admit of only one such determination. These conditions will be fulfilled whensoever the problem represents a possible experience, and it will be then only fulfilled. And in this way, or by directly investigating the conditions of possibility by the rule of Art. 14, a solution is made determinate.

The arbitrary constant c does not, as has been intimated, always present itself. When it does, it represents the unknown probability, that if the event C occur, w will occur. It indicates, therefore, the new experience which would be necessary in order to make the solution definite.

18. I will, for the sake of illustration, apply the method to the problem of Art. 11, and in so doing I will limit the solution by the conditions relative to s, t, &c.

The problem, as symbolically expressed in Art. 13, is as follows:—

$$\left.\begin{array}{llll} \text{Given} & \text{Prob. } xy=p & \text{Prob. } yz=q & \text{Prob. } zx=r \\ \text{Required} & \text{Prob. } xyz & & \end{array}\right\} \quad . \quad (1)$$

Translating the problem as directed in the first part of the rule, we write

$$\left.\begin{array}{lll} xy=s & yz=t & zx=v \\ & xyz=w & \end{array}\right\} \quad . \quad . \quad . \quad (2)$$

whence, by the calculus of logic,

$$w=stv+0\,(s\bar{t}\,\bar{v}+t\bar{s}\,\bar{v}+v\bar{s}\,\bar{t}+\bar{s}\,\bar{v}\,\bar{t}) + \frac{1}{0}\,(st\bar{v}+s\bar{v}\,t+t\bar{v}\,s) \quad . \quad . \quad . \quad . \quad (3)$$

Hence we find

$$\mathrm{V}=stv+s\bar{t}\,\bar{v}+t\bar{s}\,\bar{v}+v\bar{s}\,\bar{t}+\bar{s}\,\bar{t}\,\bar{v} \quad . \quad . \quad . \quad . \quad (4)$$

and are led to the algebraic system of equations

$$\frac{stv+s\bar{t}\,\bar{v}}{p} = \frac{stv+t\bar{s}\,\bar{v}}{q} = \frac{stv+v\bar{s}\,\bar{t}}{r} = \frac{stv}{u} = stv+s\bar{t}\,\bar{v}+t\bar{s}\,\bar{v}+v\bar{s}\,\bar{t}+\bar{s}\,\bar{t}\,\bar{v} \quad . \quad . \quad . \quad (5)$$

These equations may be simplified by dividing every term by $\bar{s}\,\bar{t}\,\bar{v}$, and then assuming

$$\frac{s}{\bar{s}}=s' \qquad \frac{t}{\bar{t}}=t' \qquad \frac{v}{\bar{v}}=v' \quad . \quad . \quad . \quad . \quad (6)$$

They thus give

$$\frac{s't'v'+s'}{p} = \frac{s't'v'+t'}{q} = \frac{s't'v'+v'}{r} = \frac{s't'v'}{u} = s't'v'+s'+t'+v'+1 \quad . \quad . \quad . \quad . \quad (7)$$

The condition to which $s'\ t'\ v'$ are subject obviously is, that they shall be positive quantities, for this is equivalent to the condition that s, t, v shall be positive fractions.

From (7) we readily find

$$\frac{s'}{p-u} = \frac{t'}{q-u} = \frac{v'}{r-u} = \frac{s't'v'}{u} = s't'v'+s'+t'+v'+1 \quad . \quad (8)$$

Whence

$$\left.\begin{aligned} s' &= \frac{p-u}{2u-p-q-r+1} \\ t' &= \frac{q-u}{2u-p-q-r+1} \\ v' &= \frac{r-u}{2u-p-q-r+1} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (9)$$

Substitute these values in the equation

$$\frac{s'}{p-u} = \frac{s't'v'}{u}$$

and reducing we get

$$(p-u)\,(q-u)\,(r-u)=u\,(2\,u-p-q-r+1)^2 \quad . \quad . \quad (10)$$

an equation for determining u.

And now let us inquire into the consequences which flow from the condition that $s'\ t'\ v'$ are positive quantities.

In the first place, the last member of (8), and therefore each other member of that system will be positive. This requires that the denominators, $p-u$, $q-u$, $r-u$, and u, should be positive, whence we have

$$\left.\begin{matrix} u \gtrless 0 \\ u \lessgtr p,\ u \lessgtr q,\ u \lessgtr r \end{matrix}\right\} \quad . \quad . \quad . \quad . \quad (11)$$

Again, $p-u$, $q-u$, and $r-u$ being positive, the common denominators, $2u-p-q-r+1$ in (9) must be positive, whence

$$u \gtrless \frac{p+q+r-1}{2} \quad . \quad . \quad . \quad . \quad (12)$$

Such are the conditions relative to u. They agree in all respects with those assigned in the previous investigation, in (5), Art. 13; and, as in that article, the elimination of u leads to the conditions of possible experience,

$$\left.\begin{matrix} p \gtrless 0 & q \gtrless 0 & r \gtrless 0 \\ p \gtrless & q+r-1 & \\ q \gtrless & p+r-1 & \\ r \gtrless & p+q-1 & \end{matrix}\right\} \quad . \quad . \quad . \quad . \quad (13)$$

It may be well to notice, that these conditions involve the necessity of p, q, and r being fractional, though of course this does not exhaust their significance.

19. It remains to show that when the above conditions are satisfied, the system (7) will admit of but one solution in positive values of s', t', v', and that (10) will furnish but one value of u satisfying the conditions (11) and (12).

Let us write 10 in the form

$$u\,(2\,u-p-q-r+1)^2-(p-u)\,(q-u)\,(r-u)=0 \quad . \quad (14)$$

or, for simplicity, in the form

$$U=0.$$

The lower limit of u is, by (11) and (12), either 0 or $\frac{p+q+r-1}{2}$, according as the latter quantity is positive or negative; the upper limit of u is the least of the quantities p, q, r; suppose it p. First, let $\frac{p+q+r-1}{2}$ be positive, then, making u equal to this quantity, the value of U, as given in the first member of (14) becomes negative. Again, let $u=p$, then U becomes positive. Thus, as u varies from $\frac{p+q+r-1}{2}$ up to p, U changes from negative to positive. Now

$$\frac{dU}{du}=(2u-p-q-r+1)^2+4u(2u-p-q-r+1)+(p-u)(q-u)+(q-u)(r-u)+(r-u)(p-u) \quad . \quad . \quad . \quad . \quad . \quad (15)$$

which within the supposed limits is always positive. Hence U varies by continuous increase, and once only in its variation becomes equal to 0.

Secondly, let $\frac{p+q+r-1}{2}$ be negative, then u, varying from 0 up to p, U as before will vary by continuous increase from a negative to a positive value. See the first member of (14). Whence U, changing by continuous increase from a negative to a positive value, will still only once become equal to 0.

Wherefore, in either case, one root only of (10) will lie within the limits assigned to u in (11) and (12). And this one value substituted in (9) will give one set of values for s', t', v'.

20. The solutions which we have now obtained of the same problem on different hypotheses with respect to the selection of the simple events, set in clear light the principles upon which the due selection of such hypotheses depends. The hypothesis which seems most readily to present itself utterly fails, while the other, based quite as much upon an apparently remote speculation on language, as upon the study of the laws of expectation as usually conceived, finds a support and confirmation within the realm of pure mathematics which is of the most remarkable kind.

21. A practical simplification of the general method is suggested by that step of the preceding solution, which reduces (5) to the form (7). If we remove the traces ($'$) from the letters in the latter system (and they do not at all affect the solution), we obtain what (5) would become if we replaced each of the symbols $\bar{s}$, $\bar{t}$, $\bar{v}$, by unity. Practically, therefore, we may modify the general rule in the following manner:—Having obtained V, replace each of the symbols $\bar{s}$, $\bar{t}$, &c., by unity, and proceed with the reduced value of V just as before, *i.e.*, let V_s represent that portion of V of which s is a factor, &c., then form the system of equations

$$\frac{V_s}{p}=\frac{V_t}{q}\ldots=\frac{A+cC}{u}=V \quad . \quad . \quad . \quad . \quad (1)$$

and hence determine u as a function of p, q . . and c. The conditions of possible experience and of limitation will be found by supposing s, t, to admit of a single determination in positive values. Or as before, they may be found independently, and then applied to limit the solution.

22. We now proceed to the consideration of the problems referred to in Art. (1.) We shall first examine the problem which has for its object the determination of the most probable measure of a physical magnitude, two conflicting measures of which have been assigned by observation. The problem is not, as has been said, Art. 2, in its immediate presentation, one whose elements are logical, but it admits, as we shall see, of being so represented as to give it this character.

Problem 1.

Two simultaneous observations of a physical magnitude, as the elevation of a star, assign to it the respective values p_1 *and* p_2. *The probability, when the first observation has been made, that it is correct, is* c_1, *the corresponding probability for the second observation is* c_2. *Required the most probable value of the physical magnitude hence resulting.*

First Solution.

23. The numerical elements which are not, in their immediate presentation, probabilities, are p_1 and p_2. But these become such if we contemplate the problem under another aspect. Let a quadrant be taken as the unit of magnitude, then p_1 and p_2 are proper fractions ; p_1 actually expressing the probability afforded by the first observation, p_2 that afforded by the second observation, that a pointer, directed at random to that quadrant of elevation in which the star, regarded as a physical point, is situated, will point below the star. The problem thus regarded contains the following logical elements, which we shall express by appropriate symbols, viz.

The event which consists in the first observation, such as it is, being made $=x$.

The event which consists in the second observation, such as it is, being made $=y$.

The event which consists in the first observation being correct, $=w$.

The event which consists in the second observation being correct, $=v$.

The event which consists in a pointer, directed at random to the quadrant in which the star is situated, pointing below that star, $=z$.

We must now express symbolically the data, including therein whatever logical connections we can establish among the events, x, y, w, v, and z.

The probability that the first observation, when made, is correct, is c_1. This is a conditional probability; or, to adopt a well-known form of expression, it is a probability *à posteriori*. Viewed from a point of time anterior to the observa-

tion, it is the probability that if the observation be made under its actual circumstances of care, personal fitness, instrumental accuracy, &c., it will be absolutely correct. Symbolically, it is the probability that if the event x take place, the event w will take place. The only mode of expressing this is by writing for the probability of x an arbitrary constant a_1 we have then

$$\text{Prob. } x = a_1 \qquad \text{Prob. } xw = a_1 c_1 \quad . \quad . \quad . \quad (1)$$

The events x and w are not, however, independent. If we can affirm that a given observation is correct, we can affirm that that observation has been made. Symbolically, the occurrence of the event w implies the occurrence of the event x. Expressing this proposition in the language of the calculus of Logic we have the equation.

$$w\bar{x} = 0 \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This forms a part of our data. It permits us to change also the form of one of the previous data, and instead of (1) to substitute

$$\text{Prob. } x = a_1 \qquad \text{Prob. } w = a_1 c_1 \quad . \quad . \quad . \quad (3)$$

In like manner, representing the arbitrary probability of the event y by a_2, we have

$$\text{Prob. } y = a_2 \qquad \text{Prob. } yv = a_2 c_2 \quad . \quad . \quad . \quad (4)$$

With the connecting condition

$$v\bar{y} = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which would permit us to substitute for (4) the system

$$\text{Prob. } y = a_2 \qquad \text{Prob. } v = a_2 c_2 \quad . \quad . \quad . \quad (6)$$

Again, when it is known that the first observation is a correct one, the probability that an indicator directed at random to the quadrant in which the star is situated will point below the star is p_1. This, too, is a conditional probability. Symbolically, it is the probability that if the event w occur, the event z will occur. Hence, as the probability of the occurrence of w as $a_1 c_1$, we have

$$\text{Prob. } wz = a_1 c_1 p_1 \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In like manner we find

$$\text{Prob. } vz = a_2 c_2 p_2 \quad . \quad . \quad . \quad . \quad (8)$$

Lastly, it is supposed that the values p and q are different. This involves the condition that the observations cannot both be correct. Whence we have the logical equation.

$$wv = 0 \quad . \quad . \quad . \quad . \quad . \quad (9)$$

This completes the analysis of the logical elements involved in the data of the problem. We now proceed to analyse those involved in its quæsitum or object proposed.

That object is to determine the probability of the event z, when the occurrence

of the events x and y is known. Symbolically expressed, it is the value of the fraction.

$$\frac{\text{Prob. } x\,y\,z}{\text{Prob. } x\,y}$$

or, as it may, by resolving the denominator, be written,

$$\frac{\text{Prob. } x\,y\,z}{\text{Prob. } x\,y\,z + \text{Prob. } x\,y\,\bar{z}} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

To effect this object, we shall determine the value of Prob. $x\,y\,z$ and Prob. $x\,y\,\bar{z}$ separately.

Collecting the elements furnished by the preceding analysis, the first of the partial problems herein involved may be thus stated:—

$$\text{Given} \quad \left.\begin{array}{ll} \text{Prob. } x = a_1 & \text{Prob. } y = a_2 \\ \text{Prob. } w = a_1 c_1 & \text{Prob. } v = a_2 c_2 \\ \text{Prob. } wz = a_1 c_1 p_1 & \text{Prob. } vz = a_2 c_2 p_2 \end{array}\right\} \quad . \quad . \quad . \quad (11)$$

with the conditions, $w\bar{x}=0$, $v\bar{y}=0$, $wv=0$, ... (12)

Required u, the value of Prob. xyz.

In selecting the above, I have chosen to employ (3) in place of (1), and (6) in place of (4). It makes no difference in the final result.

In accordance with the rule, let us write

$$wz=s, \quad vz=t, \quad xyz=\phi \quad . \quad . \quad . \quad . \quad (13)$$

we must then from (12) and (13) determine ϕ as a developed logical function of x, y, w, v, s, and t.

This problem admits of perfectly definite solution on the principles of the calculus of Logic. I shall here merely give the result, and point out a method by which it may be independently verified. We find

$$\phi = xywsv\bar{t} + xyvt\bar{w}\bar{s} + 0\,(xwsy\bar{v}\bar{t} + yvtx\bar{w}\bar{s} + xy\bar{w}v\bar{s}\bar{t}$$

$$+ y\bar{x}\bar{w}v\bar{s}\bar{t} + \bar{x}y\bar{w}v\bar{s}\bar{t} + xyw\bar{v}\bar{s}\bar{t} + xwy\bar{v}\bar{s}\bar{t} + xyv\bar{w}\bar{s}\bar{t} + yv\bar{x}\bar{w}\bar{s}\bar{t})$$

$$+ \frac{0}{0} xyw\bar{v}\bar{s}\bar{t} + \text{terms whose coefficient is } \frac{1}{0} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

We may verify this expansion by substituting for s and t their values wz and vz, paying attention to the conditions (12), and then comparing the result with the value of ϕ, viz., xyz.

Thus the term $xywsv\bar{t}$ becomes, on substitution

$$xyw\bar{v} \times wz \times (1-vz) = xyzw\bar{v}$$

by the calculus of Logic. Now this represents a class entirely included in the class xyz, whence the coefficient of the term is unity.

The term $xwsy\bar{v}\bar{t}$ reduces to $xwzy\bar{v}$, and represents a class no part of which is included in xyz, whence the coefficient is 0.

The term $xy\bar{w}\,\bar{v}\,\bar{s}\,\bar{t}$, reduces to $xy\,\bar{w}\,\bar{v}$, and represents a class some part of which is included, and some part not included under the class xyz, whence the coefficient $\frac{0}{0}$; for an event included under the former class may or may not be included under the latter.

Lastly, any term whose coefficient in the expansion is $\frac{1}{0}$ would, on effecting the above-named substitutions, become 0, indicating the absolute non-existence of the class which it represents.

Resuming the value of ϕ, and adopting the simplification of Art. 21, we find for V the value

$$\begin{aligned} \mathrm{V} &= xyws + xyvt + xws + yvt + x + y + 1 + xyw + xw + xyv + yv + xy \\ &= (x+1)(y+1) + yv(x+1)(t+1) + xw(y+1)(s+1) \quad . \quad . \quad (15) \end{aligned}$$

And hence we have the following system of algebraic equations:

$$\begin{aligned} &\frac{x(y+1) + xyv(t+1) + xw(y+1)(s+1)}{a_1} = \frac{y(x+1) + yv(x+1)(t+1) + xwy(s+1)}{a_2} \\ &= \frac{xw(y+1)(s+1)}{a_1 c_1} = \frac{yv(x+1)(t+1)}{a_2 c_2} = \frac{xw(y+1)s}{a_1 c_1 p_1} = \frac{yv(x+1)t}{a_2 c_2 p_2} \\ &= \frac{xyws + xyvt + cxy}{u} = (x+1)(y+1) + yv(x+1)(t+1) + xw(y+1)(s+1) \qquad (16) \end{aligned}$$

From these equations, if we assume

$$(x+1)(y+1) + yv(x+1)(t+1) + xw(y+1)(s+1) = \lambda,$$

λ being a subsidiary quantity introduced for convenience, we readily deduce

$$u = \frac{xyws + xyvt + cxy}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$a_1 c_1 p_1 = \frac{xws(y+1)}{\lambda}$$

$$a_2(1-c_2) = \frac{y(x+1) + xwy(s+1)}{\lambda}$$

$$1 - a_2 c_2 = \frac{(x+1)(y+1) + xw(y+1)(s+1)}{\lambda}$$

Hence

$$\frac{a_1 c_1 p_1 \times a_2(1-c_2)}{1 - a_2 c_2} = \frac{xyws}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad (18)$$

In like manner

$$\frac{a_2 c_2 p_2 \times a_1(1-c_1)}{1 - a_1 c_1} = \frac{xyvt}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Again we have

$$a_1(1-c_1) = \frac{x(y+1) + xyv(t+1)}{\lambda}$$

$$a_2(1-c_2) = \frac{y(x+1) + xwy(s+1)}{\lambda}$$

$$1-a_1c_1-a_2c_2=\frac{(x+1)(y+1)}{\lambda}$$

$$1-a_1c_1=\frac{(x+1)(y+1)+yv(x+1)(t+1)}{\lambda}$$

$$1-a_2c_2=\frac{(x+1)(y+1)+xw(y+1)(s+1)}{\lambda}$$

Whence we find

$$\frac{a_1(1-c_1)a_2(1-c_2)(1-a_1c_1-a_2c_2)}{(1-a_1c_1)(1-a_2c_2)}=\frac{xy}{\lambda} \quad . \quad . \quad . \quad (20)$$

By means of (18), (19), and (20), we reduce (17) to the form

$$u=\frac{a_2(1-c_2)}{1-a_2c_2}a_1c_1p_1+\frac{a_1(1-c_1)}{1-a_1c_1}a_2c_2p_2+c\frac{a_1(1-c_1)a_2(1-c_2)(1-a_1c_1-a_2c_2)}{(1-a_1c_1)(1-a_2c_2)}$$

therefore effecting a slight reduction

$$\text{Prob. } xyz=\frac{a_1(1-c_1)a_2(1-c_2)}{(1-a_1c_1)(1-a_2c_2)}\left\{\frac{1-a_1c_1}{1-c_1}c_1p_1+\frac{1-a_2c_2}{1-c_2}c_2p_2+c(1-a_1c_1-a_2c_2)\right\} \quad (21)$$

The arbitrary constant c, interpreted according to the rule, is the probability that if the event $xy\bar{w}\,\bar{v}\,\bar{s}\,\bar{t}$ take place, xyz will take place. Putting for $\bar{s}$ and $\bar{t}$ their values, and reducing as before, we find that c is the probability that if $xy\bar{w}\,\bar{v}$ take place, xyz will take place. In the end this amounts to the following statement.

c = probability that if both observations are incorrect, a pointer directed at random to the quadrant in which the star is situated will point below the star.

The value of Prob. $xy\bar{z}$ will be obtained from that of Prob. xyz by changing p_1 p_2 and c into $1-p_1$, $1-p_2$, and $1-c$. If we effect this change, and then substitute the expressions above found, in the formula,

$$\frac{\text{Prob. } xyz}{\text{Prob. } xyz+\text{Prob. } xy\bar{z}}$$

We shall find

$$\left.\begin{aligned}\frac{\text{Prob. } xyz}{\text{Prob. } xy}&=\frac{\frac{1-a_1c_1}{1-c_1}c_1p_1+\frac{1-a_2c_2}{1-c_2}c_2p_2+c(1-a_1c_1-a_2c_2)}{\frac{1-a_1c_1}{1-c_1}c_1+\frac{1+a_2c_2}{1-c_2}c_2+1-a_1c_1-a_2c_2}\\&=\frac{\frac{1-a_1c_1}{1-c_1}c_1p_1+\frac{1-a_2c_2}{1-c_2}c_2p_2+c(1-a_1c_1-a_2c_2)}{1+\frac{1-a_1}{1-c_1}c_1+\frac{1-a_2}{1-c_2}c_2}\end{aligned}\right\} \quad . \quad . \quad . \quad (22)$$

This expression involves an arbitrary constant c which we have no means of determining. This circumstance indicates that those principles of probability which relate to the combination of *events* do not *alone* suffice to enable us to combine into a definite result the conflicting measures of an astronomical observation.

The arbitrary character of the final solution might have been inferred from

the appearance of the symbol $\frac{0}{0}$ in (14). I have thought it better to complete the investigation, especially as it will serve as a model for the one which follows.

24. Before proceeding to the second solution of the problem, I will endeavour to explain the principle on which it will be founded. It is involved in the following definition.

Definition. The mean strength of any probabilities of an event which are founded upon different judgments or observations is to be measured by that supposed probability of the event *à priori* which those judgments or observations following thereupon would not tend to alter.

Thus, suppose we were considering the question of the suitableness of a newly discovered island for the growth of a particular plant, and that the probability of its suitableness, as dependent upon general impressions of the climate were r; but that added special observations,—such as analysis of the soil, determination of allied species growing in the locality, &c., had some of them the effect of raising, others that of depressing, the general expectation before entertained. Now we might suppose that expectation to have had such a measure, that the added observations should, when united, leave the mind in the same state as before. I call that measure the *mean value* of the testimonies—the value about which, to adopt (for illustration, not for argument) a mechanical analogy, they balance each other. I conceive that in thus doing, I am only giving a scientific meaning to a term which has been hitherto used in a vague sense. I shall show that the formula of the arithmetical mean is a special determination applicable only to particular problems, of the more general mean of which I here speak, and that other determinations of it exist, applicable to other problems, but possessing, in common, certain definite characteristics.

To apply this principle to the problem under consideration, we must add to the data a new element, viz., the *à priori* value of Prob. z, *i.e.*, the value which the mind is supposed to attach to it before the evidence furnished by the observations. We will suppose this value r. We must then seek, as before, the *a posteriori* value of Prob. z, *i.e.*, its value after the observations, and, equating the two expressions, determine thence the value of r.

I shall, in referring to the above principle, speak of it as the "principle of the mean."

Special solution of Problem I. founded upon the principle of the mean.

Assigning to z the *à priori* probability r, our data are the following:

$$\begin{array}{lll} \text{Prob. } x=a_1 & \text{Prob. } y=a_2 & \text{Prob. } z=r \\ \text{Prob. } w=a_1 c_1 & \text{Prob. } v=a_2 c_2 & \\ \text{Prob. } wz=a_1 c_1 p_1 & \text{Prob. } vz=a_2 c_2 p_2. & \end{array}$$

with the conditions $w\bar{x}=0$ $\quad v\bar{y}=0$ $\quad wv=0$

and hence we are to seek, as before, the value of

$$\frac{\text{Prob. } xyz}{\text{Prob. } xyz + \text{Prob. } x\bar{y}\bar{z}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Assuming then as before,

$$wz=s, \qquad vz=t, \qquad xyz=\phi$$

we find, by the calculus of Logic, the following expression for ϕ as a developed logical function of x, y, w, v, s, t, and z, viz.:

$$\begin{aligned}\phi = {} & x\,y\,w\,s\,z\,\bar{v}\,\bar{t} + x\,y\,v\,t\,z\,\bar{w}\,\bar{s} + x\,y\,z\,\bar{w}\,\bar{v}\,\bar{s}\,t \\ & + 0\{x\,y\,v\,\bar{w}\,\bar{s}\,\bar{t}\,\bar{z} + y\,v\,z\,t\,\bar{x}\bar{w}\bar{s} + y\,v\,\bar{x}\bar{w}\bar{s}\,\bar{z}\,\bar{t} + x\,y\,w\,s\,\bar{z}\,\bar{v}\,\bar{t} + x\,w\,z\,s\,\bar{y}\,\bar{v}\,\bar{t} \\ & + x\,w\,\bar{y}\,\bar{z}\,\bar{v}\,\bar{s}\,\bar{t} + x\,y\,\bar{z}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t} + x\,z\,\bar{y}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t} + y\,z\,\bar{x}\,\bar{s}\,\bar{v}\,\bar{w}\,\bar{t} + x\,\bar{y}\,\bar{z}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t} \\ & y\,\bar{x}\,\bar{z}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t} + z\,\bar{x}\,\bar{y}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t} + \bar{x}\,\bar{y}\,\bar{z}\,\bar{v}\,\bar{w}\,\bar{s}\,\bar{t}\} \\ & + \text{terms with coefficient } \frac{1}{0} \quad . \quad . \quad . \quad . \quad . \quad (2)\end{aligned}$$

Hence, adopting the simplification of Art. 21, we have

$$\begin{aligned}V = {} & xywsz + xyvtz + xyz + xyv + yvzt + yv + xyw + xwzs \\ & + xw + xy + xz + yz + x + y + z + 1 \\ = {} & xw(y+1)(zs+1) + yv(x+1)(zt+1) + (x+1)(y+1)(z+1) \quad . \quad (3)\end{aligned}$$

whence we form the algebraic system

$$\begin{aligned}& \frac{xw(y+1)(zs+1) + xyv(zt+1) + x(y+1)(z+1)}{a_1} \\ = {} & \frac{xwy(zs+1) + yv(x+1)(zt+1) + (x+1)y(z+1)}{a_2} \\ = {} & \frac{xw(y+1)(zs+1)}{a_1 c_1} = \frac{yv(x+1)(zt+1)}{a_2 c_2} \\ = {} & \frac{xw(y+1)zs}{a_1 c_1 p_1} = \frac{yv(x+1)zt}{a_2 c_2 p_2} \\ = {} & \frac{xw(y+1)zs + yv(x+1)zt + (x+1)(y+1)z}{r} \\ = {} & \frac{xyzws + xyzvt + xyz}{u} \\ = {} & \frac{xw(y+1)(zs+1) + yv(x+1)(zt+1) + (x+1)(y+1)(z+1)}{1} = \lambda \quad . \quad (4)\end{aligned}$$

λ being a subsidiary quantity introduced for convenience.

From the above we find

$$a_2 - a_2 c_2 = \frac{xyw(zs+1) + y(x+1)(z+1)}{\lambda}$$

$$a_1 c_1 p_1 = \frac{xwzs(y+1)}{\lambda}$$

$$1 - a_2 c_2 = \frac{xw(y+1)(zs+1) + (x+1)(y+1)(z+1)}{\lambda}$$

whence

$$\frac{a_2(1-c_2)}{1-a_2\,c_2}\,a_1\,c_1\,p_1 = \frac{xyzws}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

In like manner we find

$$\frac{a_1\,(1-c_1)}{1-a_1\,c_1}\,a_2\,c_2\,p_2 = \frac{xyzvt}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Again, since we have from the above

$$\frac{a_2-a_2\,c_2}{1-a_2\,c_2} = \frac{y}{y+1} \text{ and } \frac{a_1-a_1\,c_1}{1-a_1\,c_1} = \frac{x}{x+1}$$

we have

$$\frac{a_1\,(1-c_1)\,a_2\,(1-c_2)}{(1-a_1\,c_1)\,(1-a_2\,c_2)} = \frac{xy}{(x+1)\,(y+1)}$$

moreover,

$$r-a_1\,c_1\,p_1-a_2\,c_2\,p_2 = \frac{(x+1)\,(y+1)\,z}{\lambda}$$

Multiplying the two last equations together we find

$$\frac{a_1\,(1-c_1)\,a_2\,(1-c_2)}{(1-a_1\,c_1)\,(1-a_2\,c_2)}\,(r-a_1\,c_1\,p_1-a_2\,c_2\,p_2) = \frac{xyz}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Now,

$$u = \frac{xyzws+xyzvt+xyz}{\lambda}$$

Substituting in this expression the values found for its several terms in (5), (6), and (7), we have

$$u = \frac{a_2\,(1-c_2)}{1-a_2\,c_2}\,a_1\,c_1\,p_1 + \frac{a_1(1-c_1)}{1-a_1\,c_1}\,a_2\,c_2\,p_2 + \frac{a_1\,(1-c_1)\,a_2'(1-c_2)}{(1-a_1\,c_1)\,(1-a_2\,c_2)}\,(r-a_1\,c_1\,p_1-a_2\,c_2\,p_2)$$

This is the value of Prob. xyz. That of Prob. $xy\bar{z}$ will be found by simply changing in the above expression p_1, p_2, and r, into $1-p_1$, $1-p_2$, and $1-r$ respectively. These expressions admit of some reductions, and give

$$\text{Prob. } xyz = \frac{a_1\,(1-c_1)\,a_2\,(1-c_2)}{(1-a_1\,c_1)\,(1-a_2\,c_2)}\left\{\frac{1-a_1}{1-c_1}\,c_1\,p_1 + \frac{1-a_2}{1-c_2}\,c_2\,p_2 + r\right\} \quad . \quad . \quad . \quad (8)$$

$$\text{Prob. } xy\bar{z} = \frac{a_1\,(1-c_1)\,a_2\,(1-c_2)}{(1-a_1\,c_1)\,(1-a_2\,c_2)}\left\{\frac{1-a_1}{1-c_1}\,c_1\,(1-p_1) + \frac{1-a_2}{1-c_2}\,c_2\,(1-p_2) + 1 - r\right\} \quad . \quad (9)$$

whence we find for the *à posteriori* value of Prob. z,

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy} = \frac{\dfrac{1-a_1}{1-c_1}\,c_1\,p_1 + \dfrac{1-a_2}{1-c_2}\,c_2\,p_2 + r}{\dfrac{1-a_1}{1-c_1}\,c_1 + \dfrac{1-a_2}{1-c_2}\,c_2 + 1}$$

Equating this to r we have

$$\frac{1-a_1}{1-c_1}\,c_1\,p_1 + \frac{1-a_2}{1-c_2}\,c_2\,p_2 + r = \left(\frac{1-a_1}{1-c_1}\,c_1 + \frac{1-a_2}{1-c_2}\,c_2 + 1\right) r$$

Whence

$$r = \frac{\dfrac{1-a_1}{1-c_1}\,c_1\,p_1 + \dfrac{1-a_2}{1-c_2}\,c_2\,p_2}{\dfrac{1-a_1}{1-c_1}\,c_1 + \dfrac{1-a_2}{1-c_2}\,c_2} \quad . \quad . \quad . \quad . \quad (10)$$

26. Such is the final general expression for the probable altitude of the star. The following observations may throw light upon its real nature:—

1*st*, In the analysis by which this expression was obtained, p_1 and p_2 are the observed altitudes of the star, a quadrant of the celestial arc being taken as unity. Considered, however, as the expression, not of a *probability*, but of the most probable measure of a physical magnitude, the truth of the formula will of course be independent of the unit of magnitude.

2*dly*, The formula is independent of mechanical analogy. We may place it in the well-known form

$$r = W_1 p_1 + W_2 p_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which, as the subject is usually treated W_1 and W_2 are called the *weights* of the observations. Here, however, these quantities are determined as functions of the initial data—these data being probabilities. We have

$$W_1 = \frac{\frac{1-a_1}{1-c_1}c_1}{\frac{1-a_1}{1-c_1}c_1 + \frac{1-a_2}{1-c_2}c_2} \qquad W_2 = \frac{\frac{1-a_2}{1-c_2}c_2}{\frac{1-a_1}{1-c_1}c_1 + \frac{1-a_2}{1-c_2}c_2} \quad . \quad (2)$$

3*dly*, The initial probabilities, of which W_1 and W_2 are functions, are neither foreign nor imaginary elements. They may be difficult to determine, but theoretically their determination rests upon considerations which are entirely proper to the subject. When an observation has been made, the question whether it is correct or not is a question of probability. We can never predicate absolute correctness. We can seldom affirm absolutely that an observation is incorrect. Our knowledge of the circumstances of the observation, Art. 22, leads us to regard the probability in question as sometimes greater, sometimes less. To suppose it capable of a numerical value, as we have done, by the introduction of the constants $c_1\, c_2$, is then perfectly legitimate. It has been said that an estimate of the correctness of the observation rests upon the circumstances by which it was accompanied. These circumstances, taken in the aggregate, are themselves a subject of probability. This we express by the introduction of the constants $a_1\, a_2$. The probability after an observation is made that it is correct, and the probability before it is made that the state of things shall be such as to give to the result that particular probability of correctness, are quite different things.

4*thly*, In the same course of observations made by the same individual with consciously uniform regard to personal and instrumental accuracy the values of a_1 and a_2 would be sensibly equal. The formula (10) would thus reduce to the following, viz.:—

$$r = \frac{\frac{c_1}{1-c_1}p_1 + \frac{c_2}{1-c_2}p_2}{\frac{c_1}{1-c_1} + \frac{c_2}{1-c_1}} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Here $$W_1=\frac{\frac{c_1}{1-c_1}}{\frac{c_1}{1-c_1}+\frac{c_2}{1-c_2}} \qquad W_2=\frac{\frac{c_2}{1-c_2}}{\frac{c_1}{1-c_1}+\frac{c_2}{1-c_2}} \quad . \quad . \quad . \quad (4)$$

If $c_1=1$, we have

$$W_1=1 \qquad W_2=0$$

and $$r=p_1.$$

This accords with the condition that if either of the observations is believed to be correct, the value which it furnishes for the altitude of the star must be taken as the true one.

5thly, If $c_1=c_2$, *i.e.*, if we have no right to give preference to one observation over the other, we have

$$r=\frac{p_1+p_2}{2} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

the formula of the arithmetical mean.

6thly, From the form of W_1, W_2 in (4), it is evident that the weights, so to speak, of the observations vary in a higher ratio than that of the simple probabilities of correctness of the observations. The practical lesson to be drawn from this is, that we ought to attach a greater weight to good observations, and a smaller to bad ones, than, according to usual modes of consideration, we should be disposed to do.

The above are the most important observations suggested by the formula to which the last investigation has led. One or two remarks remain to be offered upon the analysis by which it was obtained.

Although the two forms of investigation which we have exhibited differ, there is nothing inconsistent in the results to which they lead. If we compare corresponding formulæ in the two, *e.g.*, the values of Prob. xyz, or those of Prob. $xy\bar{z}$, we shall find that the one investigation assigns a definite but consistent value to what the other left arbitrary. Either comparison gives

$$c=\frac{r-a_1c_1p_1-a_2c_2p_2}{1-a_1c_1-a_2c_2}$$

We may prove, either by the "conditions of possible experience," or independently, that this value is necessarily a proper positive fraction, and this accords with the interpretation of c as a probability. Art. 23.

27. But a much more important consideration is the following. It is a plain consequence of the logical theory of probabilities, that the state of expectation which accompanies entire ignorance of an event is properly represented, not by the fraction $\frac{1}{2}$, but by the indefinite form $\frac{0}{0}$. And this agrees with a conclusion at which Bishop TERROT, on independent, but as I think just grounds, has arrived.* Now this shows, why, if the consideration of the *à priori* probability of z is, from

* Transactions of the Royal Society of Edinburgh, vol. xxi. p. 375.

the insufficiency of the remaining data, necessary in order to give to the *à posteriori* probability of z a definite value, the solution obtained when that *à priori* value is neglected should involve the symbol $\frac{0}{0}$. The presence of this symbol in a solution always indicates insufficiency in the data. And herein, as it seems to me, consists the reason why the mind, impatient of incertitude even while dealing with the very science of uncertain knowledge, is led to seek escape from its doubts, by calling in the aid, in some form or another, of that adventitious principle which I have denominated the principle of the mean. I say in some form or another; for I can conceive of another form of the same principle connected more directly with the idea of a *limit* than with that of a mean. Thus as testimonies which are insufficient of themselves to produce a definite expectation may definitely modify a definite expectation previously formed, we have suggested to us the idea of that limiting state to which perpetual and independent repetition of the same series of testimonies would cause the mind, whatever its starting point of expectation might be, to tend. And as this limiting state would be one which a further repetition would not alter, we should thus arrive in effect at the same solution as is indicated by the principle of the mean, in its direct expression.

28. I have extended the preceding analysis to the case in which three observations are to be combined, a case which, in connection with the previous one, is sufficient to determine the general law. The result is what the preceding analysis suggests, and may be expressed in the following theorem:—

If n conflicting observations assign to the altitude of a star the respective values $p_1 p_2 \ldots p_n$; if, moreover, $a_1 a_2 \ldots a_n$ are the antecedent probabilities that the observations will be such as they prove to be with respect to those circumstances which determine their relative accuracy, and $c_1 c_2 \ldots c_n$ their respective probabilities of correctness to a mind acquainted with these circumstances, *i.e.*, to the mind of the observer after the observations have been made, then the most probable altitude of the star will be

$$\frac{\frac{1-a_1}{1-c_1}c_1 p_1 + \frac{1-a_2}{1-c_2}c_2 p_2 \ldots + \frac{1-a_n}{1-c_n}c_n p_n}{\frac{1-a_1}{1-c_1}c_1 + \frac{1-a_2}{1-c_2}c_2 \ldots + \frac{1-a_n}{1-c_n}c_n}$$

This expression admits of the same deductions as the one before obtained for the case in which the observations are two in number, and in particular it leads, when the circumstances of the observations are judged to be in all respects the same, to the principle of the arithmetical mean expressed by the formula

$$\frac{p_1 + p_2 \ldots + p_n}{n}$$

29. I have remarked that the principle of the arithmetical mean has some

claim to be regarded as axiomatic. In the preceding sections it presents itself as a special result of a very complex analysis founded upon the logical theory of probabilities. Now I wish to observe, that there is nothing in these circumstances which we have a right to regard as denoting inconsistency. Of the theory of probabilities it is eminently true that modes of investigation, which to our present conceptions must appear fundamentally different, habitually lead us to the same result. A profounder acquaintance with the laws of the human mind, and a deeper insight into the relations of things, might perhaps show us that principles which appear to us to have nothing in common may yet have a necessary connection with each other,—may possibly spring up from a common origin. I will endeavour to make my meaning clear by two illustrations, which will present this question in somewhat different lights.

30. An idea which seems naturally to suggest itself in connection with the theory of probabilities is that of mechanical analogy. Evidence of this we see in the language, already referred to, which attributes *weight* to observation. The complete and scientific development of the idea will be found in a memoir by Professor Donkin,* who, establishing a kind of metaphysical statics on proofs of the same nature as those which are employed in deducing *à priori* the laws of ordinary statics, has arrived, by legitimate deduction, at the remotest consequences of Gauss's theory of the combination of observations. The mind, in the developed analogy, is compared to a lever acted upon by different weights, or to a mechanical system subject to given forces, and seeking, under this action, a position of equilibrium. Now it is at least a very remarkable circumstance, that an analogy of this kind should not only admit of exact scientific expression, but should, through a long train of analytical consequences, present the same laws and results, and suggest the same methods, as the principle of the arithmetical mean already referred to. All the abstract terms by which mental states and emotions are expressed, derive, if philology be of any value, their origin from outward and material things. And hence, though it might be impossible to ascend historically to the first employment of those expressions which describe the mind under the action of *forces*, and speak of the *balancing* of opinions, we cannot doubt that a perceived analogy was their source. But it could hardly have been anticipated that this analogy should remain complete and unimpaired through so lengthened a range of scientific deductions.

To what I have said above I will only add, that it is as instruments of expression and communication, rather than of thought, that material symbols, and the analogies which they furnish, seem to me to possess importance. Even the analogy which we have been considering cannot of itself occupy the place of a first principle, but seems to be a particular manifestation of that deeper

* Sur la Theorie de la Combinaison des Observations. Liouville's Journal de Mathematiques, tom. xv., 1850.

truth of which LEIBNITZ had a glimpse when he spoke of the principle of fitness and congruity—"*principe de la convenance*,"*—the ground of rational mechanics. Of course, I do not contemplate this or any subjective principle whatever, as affording us the slightest ground for affirming that the constitution of nature must, *à priori*, possess such and such a character. But it does seem to be a fact that the material system has been constituted in a certain degree of accordance with our rational faculties. The study of this accordance, *à posteriori*, is a perfectly legitimate object; and I think it the more interesting, when it brings before our view the scientific form of any of those analogies which commended themselves to the minds of the fathers of our race, which are embodied in our common speech, and without which we could apparently never hold converse with our fellows, except upon material objects.

31. The second illustration which I have to offer is the following. Many of the most important applications of the theory of probabilities, the method of least squares, for example, rest upon what has been termed the law of facility of error. This consists in the position, that in seeking to determine by observation a physical magnitude, as the elevation of a star, the probability that any measure will deviate by a quantity x from the true value, will vary directly as the function $\epsilon^{-k^2x^2}$, where k is a constant quantity. The probability that our measure will fall between the limits x and $x+dx$ being expressed by the function

$$\frac{k}{\sqrt{\pi}}\epsilon^{-k^2x^2}dx \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

GAUSS has shown that this is the only "law of facility" consistent with the assumption that, in a series of observations of the same magnitude, the arithmetical mean of the several measures obtained is the most probable value. It may even be shown, that whatever the actual "law of facility," under given circumstances, may be, and it is plain that it must vary with circumstances, such as the constitution of the instrument and the character of the observer, &c., the probability that the arithmetical mean of a very large number of values determined by observation will deviate from some fixed value by a quantity x, will vary directly as $\epsilon^{-k^2x^2}$, k being a constant dependent upon the nature of the observations.† Such, at least, is the limiting form of the function to which the law of deviation approaches as the number of obervations is increased. Now it is remarkable that considerations of a totally different kind, and founded mainly upon our conceptions of space, lead to a similar result. The probability of linear deviation (measured in a given direction) of a ball from a mark at which it is aimed, seems to obey the same law; the principle upon which that law is deter-

* ERDMANN'S Edit., p. 716.

† For some very interesting illustrations of this doctrine, see the letters of M. BRAVAIS, published in the notes to QUETELET'S Letters on the Theory of Probabilities.

mined being, not that of the arithmetical mean, but rather a principle of geometrical consistency, intimately connected with our ideas of the composition of motion.

The principle was first stated in a popular and somewhat inexact form, by Sir JOHN HERSCHEL, I believe, in the Edinburgh Review.* It was afterwards made the subject of an adverse criticism in the Philosophical Magazine, by Mr LESLIE ELLIS.† There is no living mathematician for whose intellectual character I entertain a more sincere respect than I do for that of Mr ELLIS; and even while stating the grounds upon which I differ from him, with respect to the value of Sir JOHN HERSCHEL's principle, I avail myself of his labours, in giving to that principle a more scientific form and expression, and in developing its consequences. The language adopted in the following statement, will be, as far as possible, that of the author of the principle,—the analysis will be that of Mr ELLIS.

Suppose a ball dropped from a given height, with the intention that it shall fall on a given mark. Now, taking the mark as the origin of two rectangular axes, let it be assumed, that the actual deviation observed is a compound event, of which the two components are the corresponding deviations measured along the rectangular axes. Grant, also, that the latter deviations are independent events. Further, let us represent by $f(x^2)$, $f(y^2)$, the probabilities of the respective component deviations measured along the axes x and y,—we give to them this form, because, positive and negative deviations being equally probable, the function expressing probability must be an *even* one, *i.e.*, must not change sign with the error. Hence the probability of the actual deviations observed will be $f(x^2)$ $f(y^2)$. Let it be observed that this is not the probability of a deviation to the extent $\sqrt{x^2+y^2}$ from the mark, but of a deviation to that extent *in a particular line of direction.* Now, let the principle be assumed, that this expression is independent of the position of the axes, *i.e.*, that we may regard component deviations along any two rectangular axes as independent events, by the composition of which the actual deviation is produced. We have then x' and y' representing two new component deviations,

$$f(x^2)\,f(y^2)=f(x'^2)\,f(y'^2) \quad . \quad . \quad . \quad . \quad (2)$$

If $y'=\sqrt{x^2+y^2}$ then $x'=0$ and we have

$$f(x^2)\,f(y^2)=f(0)\,f(x^2+y^2) \quad . \quad . \quad . \quad . \quad (3)$$

An equation of which the complete solution is,

$$f(x^2)=\mathrm{A}\,\epsilon^{hx^2}$$

A and h being constants. The condition that the probability of the error must

* Vol. xcii. p. 17, Art. QUETELET on Probabilities.

† Vol. xxxvii. p. 321, "Letter addressed to J. D. FORBES, Esq., Professor of Natural Philosophy in the University of Edinburgh, on an alleged proof of the Method of Least Squares."

diminish as the amount of the error increases, requires that h should be negative. We may therefore write,—k^2, for h. Whence

$$f(x^2)=\mathrm{A}\,\epsilon^{-k^2x^2} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

To apply this result to the case in which the ball is supposed at some point on the plane which, projected on the axis of x, will fall between x and $x+\delta x$, we must give to A the form $\mathrm{C}\,\delta x$.

Thus we get the expression,

$$\mathrm{C}\,\epsilon^{-k^2x^2}\,\delta x$$

Lastly, the *certainty* that the ball must fall at some point for which the value of x lies between $-\infty$ and ∞ gives us the equation

$$\int_{-\infty}^{\infty} \mathrm{C}\,\epsilon^{-k^2x^2}\,\delta x=1$$

whence $\mathrm{C}\frac{\sqrt{\pi}}{k}=1$ and $\mathrm{C}=\frac{k}{\sqrt{\pi}}$. Thus, the probability of a deviation from the axis y to a distance lying between x and $x+\delta x$ will be given by the formula

$$\frac{k}{\sqrt{\pi}}\,\epsilon^{-k^2x^2}\,\delta x \quad . \quad . \quad . \quad . \quad (5)$$

an expression which agrees with (1).

In like manner, the probability that the ball will deviate to a distance greater then y and less then $y+\delta y$ from the axis x will be

$$\frac{k}{\sqrt{\pi}}\,\epsilon^{-k^2y^2}$$

whence the probability that it will actually fall upon the elementary area $\delta x\,\delta y$ will be

$$\frac{k^2}{\pi}\,\epsilon^{-k^2(x^2+y^2)}\,\delta x\,\delta y$$

Now, this result admits of a remarkable confirmation. For it is manifest that the probability that the ball will fall *somewhere* between the distances x and $x+\delta x$ from the axis y, ought to be equal to the above expression integrated with respect to y between the limits $-\infty$ and ∞. But that probability has been already determined to be $\frac{k}{\sqrt{\pi}}\,\epsilon^{-k^2x^2}\,\delta x$; we ought therefore to have

$$\int_{-\infty}^{\infty}\left(\frac{k^2}{\pi}\,\epsilon^{-k^2(x^2+y^2)}\,\delta x\right)\delta y=\frac{k}{\sqrt{\pi}}\,\epsilon^{-k^2x^2}\,\delta x \quad . \quad . \quad (6)$$

an equation which is actually true.

Mr Ellis considers this as showing, that the principle from which the demonstration sets out, viz., that the actual deviation of the ball from the mark may be regarded as a compound event, of which the two independent components are the deviations from the axes, involves either a mistake or a *petitio principii*. But consistency of results can never be a proof of mistake in the principles from which they are deduced; and *alone*, it offers no adequate ground for the suspicion of a

petitio principii. It is to be observed, that it is only the probability of deviation from a fixed axis which follows, according to the above investigation, the law expressed by Gauss's function. The probability of deviation in *any* direction to a distance between r and $r+\delta r$ from the mark, is expressed by a different function. This would be fatal to any hypothesis which should represent Gauss's function as determining, *à priori*, the actual law of deviation. There are indeed few cases in which it can be determined what the law is, and writers on probability have been far too anxious to interpret nature in accordance with their formulæ. No one has shown this more clearly than Mr Ellis. The precise value of Sir John Herschel's principle, as corrected by him, I conceive to be this,—that it establishes an identity between the law of facility of error expressed by Gauss's function and the law which in a special problem, involving the consideration of space and motion, seems to accord with our most elementary conceptions of these things; and this identity I apprehend to be, not an accidental thing, but a very distinct expression of that harmonious relation which binds together the different spheres of thought and existence.

33. We proceed next to the consideration of the second general problem,—that in which it is proposed to determine the combined force of two testimonies or judgments in support of a fact, the strength of each separate testimony being given.

The problem has a material as well as a formal aspect. Thus oral testimonies differ from the judgments which are furnished by the immediate personal observation of facts. And although no definite general laws have, so far as I am aware, been assigned concerning the mode in which the material character of the evidence affects expectation, it is not to be doubted that an influence does proceed from this source. As respects testimony alone, there are cases in which we feel that it is cumulative,—there are cases in which we feel that it is not so; and this difference we also feel depends upon the nature of the testimony itself. But in the majority of cases, we should probably feel that the elements upon which this difference of character depends are blended together, some decided preponderance being due to the one or to the other. Testimony will be chiefly or entirely cumulative which is given quite independently by different persons, and is at the same time based upon different grounds. In proportion as these conditions fail of being satisfied, the testimony partakes less and less of the cumulative character. Still this possession of cumulative character may be regarded as the standard by which the distinctive qualities of testimonies, as affecting belief or expectation, may be estimated. In judgments founded upon the personal observation of facts, though this character may be observed, the standard seems to be different. When different modes of considering a subject—different courses of experiment or inquiry—lead to different probabilities of a fact, some making it more probable, some less, we generally feel that a kind of mean ought to be taken among them. Perhaps the most succinct general statement would be,

that it belongs to testimony, in its normal character, to be cumulative,—to judgment, to require the application, in some form or other, of the principle of means or averages; but that all departures from these normal states involve the blending of the two elements together, in proportions determined by the degree of the deflection.

Now, although it does not belong to the theory of probabilities, in its formal and scientific character, to pronounce upon the material character of a problem, and to say whether its data are in their own nature cumulative or not, yet the results to which the theory leads are, in a very remarkable degree, accordant with the distinctions which have just been pointed out. I shall show that the solution of the problem of the combination of testimonies, when the data are presented in a purely formal character, and without any adventitious principle, involves arbitrary constants, and is therefore indefinite,—being capable, however, under certain circumstances, of assuming a definite form. I shall show that such a form is assumed when the circumstances are such as to give to the testimonies the highest degree of cumulative character. I shall then solve the problem a second time, introducing that adventitious principle which I have already exemplified in the problem of the reduction of astronomical observations, and which appears to me to contain the true theory of means or averages. The form of the solution thus obtained, which is also perfectly definite, will apply to the case, in which it is our object, not to combine testimonies, in the ordinary sense of the term, but to determine the mean of expectations founded upon the issues of conflicting judgments. To one point of importance I must again, before entering upon the analytical investigation, ask the attention of the reader. It is, that in the present subject, the question of the right application of a formula is quite distinct from that of the validity of the processes by which that formula is derived from its data. The latter is a question of formal science, the former involves considerations which belong rather to the philosophy of the human mind.

I will first express the problem which we have to consider in a general form, equally applicable to the combination of testimonies or of judgments. I shall consider the fact of a testimony having been borne, or an observation made, as a circumstance or event affecting our expectation of the event to which it has reference.

Problem II.

34. *Required the probability of an event* z, *when two circumstances* x *and* y *are known to be present,—the probability of the event* z, *when we only know of the existence of the circumstance* x *being* p,—*and its probability when we only know of the existence of* y *being* q.

Here we are concerned with three events, x, y, and z. For convenience and uniformity I shall, in the solution of the problem, speak of x and y as *events*, as

well as of z. A circumstance is an event—a state of things which comes to pass, or has come forth—*evenit*.

The data leave wholly arbitrary the probabilities of the event x and y. Thus p and q are *conditional* probabilities; p is the probability that if the event x occur, the event z will occur; q is the probability that if the event y occur, the event z will occur. Hence

$$p=\frac{\text{Prob. } xy}{\text{Prob. } x}, \qquad q=\frac{\text{Prob. } yz}{\text{Prob. } y} \qquad . \quad . \quad . \quad . \quad (1)$$

Our object is to determine the probability that if the events x and y both occur, the event z will occur. We have therefore to seek the value of the fraction

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy}$$

or, as for our present purpose it is more convenient to say, of

$$\frac{\text{Prob. } xyz}{\text{Prob. } xyz+\text{Prob } xy\bar{z}} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

In seeking the value of Prob. xyz, which we shall represent by u, the formal statement of our *data* and *quæsitum* will therefore be

$$\text{Given } \left\{ \begin{array}{ll} \text{Prob. } x=c, & \text{Prob. } y=c' \\ \text{Prob. } xz=cp, & \text{Prob. } yz=c'q \end{array} \right\} \qquad . \quad . \quad . \quad . \quad (3)$$

$$\text{Required Prob. } xyz.$$

c and c' being arbitrary constants expressing the unknown probabilities of the events x and y.

A misconception may here arise respecting the meaning of Prob. x, Prob. y, which it is worth while to anticipate. In the case of testimony, Prob. x would not mean the probability that a testimony would be borne, but the probability that the particular kind of testimony actually recorded considered with reference to its object, credibility, &c., would be borne. Testimonies differ, not merely as to their degree of credibility, but as to their *unexpectedness*—as to the *surprise* which they occasion. And it is, I think, matter of personal experience that this unexpectedness is in itself an element affecting the strength of that expectation which combined testimonies produce. So, too, if x and y are facts of observation, *e.g.*, observed symptoms of a disease z, the probability of that disease, when both symptoms present themselves, is not determined by the strength of the separate presumptions merely, but is consciously increased by our knowledge of the rarity of the symptoms themselves. And thus the elements Prob. x Prob. y, which have been introduced by a formal necessity of the statement of the problem are seen to belong to the very matter of its solution.

Making

$$xz=s, \qquad yz=t, \qquad xyz=\phi,$$

we find, by the calculus of logic,

$$\phi = xyst + 0\,(xs\bar{y}\,\bar{t} + yt\bar{x}\,\bar{s} + xy\,\bar{s}\,\bar{t} + x\bar{y}\,\bar{s}\,\bar{t} + y\bar{x}\,\bar{s}\,\bar{t} + \bar{x}\,\bar{y}\,\bar{s}\,\bar{t})$$

$$+ \text{ terms whose coeficients are } \frac{1}{0} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

a result which may be verified by the method applied to (14) in Art. 23.

Hence we find, adopting the simplification of Art. 21,

$$V = xyst + xs + yt + xy + x + y + 1,$$

and since we have

$$\left.\begin{array}{l}\text{Prob. } x = c, \quad \text{Prob. } y = c', \quad \text{Prob. } s = cp, \quad \text{Prob. } t = c'q \\ \text{Prob. } xyz = u,\end{array}\right\} \quad . \quad . \quad . \quad (5)$$

we find, as an algebraic system of equations,

$$\left.\begin{array}{rcl}\dfrac{xyst + xs + xy + x}{c} & = & \dfrac{xyst + yt + xy + y}{c'} \\ = \dfrac{xyst + xs}{cp} & = & \dfrac{xyst + yt}{c'q} \\ = \dfrac{xyst}{u} = xyst + xs + yt + xy + x + y + 1 & & \end{array}\right\} \quad . \quad . \quad . \quad (6)$$

This system is easily reduced to the form

$$\left.\begin{array}{rcl}\dfrac{xs}{cp - u} & = & \dfrac{yt}{c'q - u} = \dfrac{xy + x + y + 1}{1 + u - cp - c'q} \\ = \dfrac{x + 1}{1 + u - cp - c'} & = & \dfrac{y + 1}{1 + u - c - c'q} = \dfrac{xsyt}{u}\end{array}\right\} \quad . \quad . \quad . \quad (7)$$

And if we equate the respective products of the first three and of the last three members of the above, we find

$$(cp - u)\,(c'q - u)\,(1 + u - cp - c'q) = (1 + u - c' - cp)\,(1 + u - c - c'q)u \quad . \quad . \quad (8)$$

a quadratic equation by which the value of u must be determined.

If, in like manner, we assume

$$\text{Prob. } xy\bar{z} = t$$

we shall find

$$(c\overline{1-p} - t)\,(c'\overline{1-q} - t)\,(1 + t - c\overline{1-p} - c'\overline{1-q}) = (1 + t - c' - c\overline{1-p})\,(1 + t - c - c'\overline{1-q})t \quad (9)$$

From these equations the values of u and t being determined, we have finally

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy} = \frac{u}{u + t} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Before we can apply this solution, we must determine the conditions of possible experience, and the conditions limiting the values of u and t. For this purpose writing

$$\begin{array}{llll}\text{Prob. } xyz = u, & \text{Prob. } xy\bar{z} = t, & \text{Prob. } xz\bar{y} = \mu, & \text{Prob. } x\bar{y}\bar{z} = \nu, \\ \text{Prob. } yz\bar{x} = \varrho, & \text{Prob. } y\bar{x}\bar{z} = \sigma, & & \end{array}$$

we have, from the data, the equations,

$$\begin{aligned} u + t + \mu + \nu &= c \\ u + t + \varrho + \sigma &= c' \\ u + \mu &= cp \\ u + \varrho &= c'q, \end{aligned}$$

to which must be added the inequations

$$u \gtrless 0 \qquad t \gtrless 0 \qquad \mu \gtrless 0 \qquad \nu \gtrless 0 \qquad \varrho \gtrless 0 \qquad \sigma \gtrless 0$$
$$u+t+\mu+\nu+\varrho+\sigma \lessgtr 1$$

Proceeding as in Art. 14, we find ultimately as the conditions of possible experience

$$cp \lessgtr 1-c'(1-q) \qquad c'q \lessgtr 1-c(1-p) \quad . \quad . \quad . \quad . \quad (11)$$

together with the usual condition that c, c', p and q must be positive proper fractions, or at any rate must not transcend the values 0 and 1. We find, too, as the conditions limiting u and t,

$$\left.\begin{array}{lll} u \lessgtr cp & u \lessgtr c'q & \\ u \gtrless c+c'q-1 & u \gtrless c'+cp-1 & u \gtrless 0 \\ t \lessgtr c\overline{1-p} & t \lessgtr c'\overline{1-q} & \\ t \gtrless c+c'\overline{1-q}-1 & t \gtrless c'+\overline{c-p}-1 & t \gtrless 0 \end{array}\right\} \quad . \quad . \quad , \quad (12)$$

The solution of the problem assumes, therefore, the following form and character:—

1*st*, It involves two constants c and c', which are arbitrary, except in that they are subject to the conditions (11).

2*ndly*, The values of u and t, determined from (8) and (9), in subjection to the conditions (12), are to be substituted in the formula (10).

3*dly*, In the absence of any means of determining c and c', the value obtained will be indeterminate, except for particular values of p and q. Some general conclusions may nevertheless be deduced from its expression indicating the manner in which expectation is influenced by circumstances insufficient of themselves to give to it a definite amount of strength. This will appear from the following analysis.

Analysis of the Solution.

35. The solution is contained in the numbered results, from (8) to (12) inclusive, of the preceding Article. Of these, (11) expresses the conditions of possible experience, (12) the conditions limiting u and t. From (8) and (9), these quanties are to be determined in accordance with (12), and the resulting values substituted in (10).

By a proper reduction of (8) and (9), the solution may also be put in the following form:—

$$a'u^2 + (cc'm - aa')u - acc'pq = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$at^2 - (cc'm + aa')t - a'cc'(1-p)(1-q) = 0 \quad . \quad . \quad . \quad (2)$$

where $a = cp + c'q - 1 \qquad a' = c(1-p) + c'(1-q) - 1 \qquad m = p + q - 1.$

The values of u and t hence found, in accordance with the limitations expressed by (12), are to be substituted in the equation

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy} = \frac{u}{u+t} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The following special deductions may now be noted.

1*st*, If either of the quantities p and q is equal to 1, the probability sought is equal to 1, whatever the values of c and c' may be.

Thus let $p=1$. Then (2) gives $t=0$; the only value which satisfies the conditions (12), in connexion with (11). The equation (1) is not satisfied by $u=0$, whence

$$\frac{u}{u+t} = \frac{u}{u} = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This result is obviously correct. If, for example, of two symptoms which are present, and which furnish ground of inference respecting a particular disease, one be of such a nature as to make the existence of the disease a matter of certainty, the fact of that existence is established, however adverse to such a conclusion the presumption furnished by the other symptom, supposing it our only ground of inference, would be.

So, too, the verdict of an authority deemed infallible is consistently held to annul and make void all opposing testimony or argument, however powerful such testimony or argument, considered in itself, may be.

2*ndly*, If either of the quantities p and q is equal to 0, the probability sought reduces to 0, as it evidently ought to do.

3*dly*, If $p=\frac{1}{2}$ and $q=\frac{1}{2}$, the equations for determining u and t become identical. Hence $u=t$, and

$$\text{Probability sought} = \frac{u}{u+u} = \frac{1}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This result is quite independent of the values of c and c'. And it is obviously a correct result. If the causes in operation, or the testimonies borne, are, separately, such as to leave the mind in a state of equipoise as respects the event whose probability is sought, united they will but produce the same effect, whatever the *à priori* probability may be that such causes will come into operation, or that such testimonies will be borne.

4*thly*, If $c=1$, and at the same time c' is not equal to 0, we find, for the equations determining u and t,

$$(p-u)\,(c'q-u)\,(u-p-c'q+1)=u(u-c'-p+1)\,(u-c'q)$$

$$(1-p-t)\,(c'\overline{1-q}-t)\,(t-c'\overline{1-q}+p)=t(t-c'+p)\,(t-c'\overline{1-q})$$

These give

$$u=c'q \qquad t=c'(1-q)$$

as the only values which satisfy (12). Hence

$$\text{Probability sought} = \frac{c'q}{c'q+c'\overline{1-q}} = q \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This result is evidently correct. The probability that such an event will take place when two other events, x and y, are present, is the same as the probability that it will take place when the event y is present, if it is known that the other event x is never absent.

5thly, If $c=c'$ and $q=1-p$, we find in like manner $u=t$, whence

$$\text{Probability sought} = \frac{1}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This result is evidently correct. If the events or testimonies x and y are equally likely to happen, and if the first yields the same presumption *in favour* of that event whose probability is sought as the other yields *against* it, the chances are equally balanced, and the probability required is $\frac{1}{2}$.

6thly, But if $q=1-p$, while c and c' are *not* equal, then the value of the probability sought is no longer $\frac{1}{2}$. It may be shown, by a proper discussion of the formulæ, that the presumption afforded by the event x, whether favourable or unfavourable, is stronger than the *opposite* presumption afforded by the event y, whenever c is less than c', and *vice versa*. And hence it follows, that if there be two events which, by themselves, afford equal presumptions, the one for and the other against some third event, of whose probability nothing more is known, then, if the said two events present themselves *in combination*, that one will yield the stronger presumption, which is itself, of the more rare occurrence. This, too, is agreeable to reason. For in those statistical observations by which probability is determined, we can only take account of co-existences and successions. We do not attempt to pronounce whether the presence of the event z in conjunction with the event x is due to the efficient action of the event x, or whether it is a product of some other cause or causes. The more frequent the occurrence of x, the less entitled are we to assert that those things which accompany or follow it derive their being from it, or are dependent upon it. If, for instance, x were a standing event, or a state of things *always* present, the probability that any event z would occur when x and y were jointly present, would be the same as the simple probability of that event z when y was present, and it would be wholly uninfluenced by the presence of x. This is the limiting case of the general principle.

7thly, The case in which $c=c'$ and $p=q$, is a very interesting one. A careful analysis leads to the following results.

If there be two events z and y, which are in themselves equally probable, the probability of each being c, and if when the event x is known to be present, while

it is not known whether y is present or not, the probability of z is p, the same probability being assigned to z, when it is known that y is present, but not known whether x is present or not; then, considering p as a presumption for or against z, according as p is greater or less than $\frac{1}{2}$.

1. That presumption is strengthened if the events x and y are known to be jointly present, *i.e.*, the probability of z is greater than p, if p is greater than $\frac{1}{2}$, but less than p in the contrary case.

2. The strengthening of the presumption is greatest when c is least. In other words, the less likely the events x and y are to happen, the more does their actual concurrence strengthen the presumption, favourable or unfavourable, which either of them alone must afford.

8*thly*, If we suppose c and c' both to approximate to 0, the values of u and t also approximate to 0, and the ratio $\frac{u}{u+t}$ assumes at the limit the form $\frac{0}{0}$. It may, however, be shown that its actual value at the limit is

$$\frac{pq}{pq+(1-p)(1-q)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This is most readily obtained from (1) and (2), by rejecting the terms $\alpha' u^2$ and αt^2, which we may do when u and t are infinitesimal. We thus find that u and t tend to assume the values $cc'pq$ and $cc'(1-p)(1-q)$, whence

$$\frac{u}{u+t} = \frac{pq}{pq+(1-p)(1-q)}$$

It is interesting here to inquire whether the appearance of the limiting value $\frac{pq}{pq+\overline{1-p}\,\overline{1-q}}$ is due merely to the *smallness* of c and c'. In studying this question, it occurred to me that it is generally not the mere improbability of events, or the mere unexpectedness of testimonies considered in themselves, but the improbability of the *concurrence* of such events or testimonies which gives to their *union* the highest degree of force. I therefore anticipated, that, if I should introduce among the primary data of the problem, the probability of the concurrence of the events x and y, assigning to it a value m, it would appear that, whenever m approached to 0, the presumptions with reference to the event z, founded upon x and y, would receive strength, whatever the values of c and c' might be. And this expectation was verified. On taking for the data

Prob. $x=c$, Prob. $y=c'$, Prob. $xy=m$, Prob. $xz=cp$, Prob. $yz=c'q$

and representing the sought value of $\frac{\text{Prob. } xyz}{\text{Prob. } xy}$ by w, I found, for the determination of w, the equation

$$(cp-mw)(c'q-mw)(1-w)=w(c\overline{1-p}-m\overline{1-w})(c'\overline{1-q}-m\overline{1-w}) \quad . \quad . \quad (9)$$

the conditions of possible experience being that c, c', p, q, and m, should be positive proper fractions, subject to the relation

$$c+c' \overline{\leqslant} 1+m \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

that root of (9) being taken, which satisfies the conditions

$$w \overline{\leqslant} \frac{cp}{m}, \quad w \overline{\leqslant} \frac{c'q}{m}, \quad w \leqslant 1$$

$$1-w \overline{\leqslant} \frac{c\,\overline{1-p}}{m}, \quad 1-w \overline{\leqslant} \frac{c'\,\overline{1-q}}{m}.$$

Now if in (9) we suppose m to vanish, we find

$$cp\,c'q\,(1-w) = wc\,\overline{1-p}\,c'\,\overline{1-q}$$

$$\therefore pq\,\overline{1-w} = w\,\overline{1-p}\,\overline{1-q}$$

whence $$w = \frac{pq}{pq+(1-p)\,(1-q)} \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The condition (10) becomes simply $c+c' \overline{\leqslant} 1$. The remaining conditions are all satisfied by the value of w.

The formula (11), which in the present investigation appears as a kind of limiting value, applicable only to cases in which the presumption for or against the event z increases most by the combination of the testimonies given, is usually regarded as expressing the *general* solution. The reasoning by which it is supposed to be established is the following.

Let p be the general probability that A speaks truth, q the general probability that B speaks truth; it is required to find the probability, that if they agree in a statement they both speak truth. Now, agreement in the same statement implies that they either both speak truth, the probability of which beforehand is pq, or that they both speak falsehood, the probability of which beforehand is $(1-p)\,(1-q)$. Hence the probability beforehand that they will agree is $pq+(1-p)\,(1-q)$, and the probability that if they agree, they will agree in speaking the truth, is accordingly expressed by the formula (11).* In the case of n, testimonies whose separate probabilities are $p_1\,p_2 \ldots p_n$, the corresponding formula is

$$\frac{p_1 p_2 \ldots p_n}{p_1 p_2 \ldots p_n + (1-p_1)\,(1-p_2) \ldots (1-p_n)} \quad . \quad . \quad . \quad . \quad (12)$$

In applying which, it is usual to regard one of the testimonies as the initial testimony of the mind itself.† Substantially the same reasoning is applied to determine the probability of correctness of a decision pronounced unanimously by a jury, the probabilities of a correct decision by each member of the jury being given.

In this reasoning there is no recognition that it is to the *same fact* that the several testimonies are borne. Take the case of two testimonies, and the problem

* Cournot Exposition de la Theorie des Chances, p. 411. De Morgan, Formal Logic, p. 191.

† Formal Logic, p. 195.

which is substituted for the true one is the following. The probability that A speaks truth is p, that B speaks truth is q; what is the probability that, if they both make assertions, and these assertions are both true or both false, they are both true? Whether A and B make the same assertion or not is assumed to be a matter of indifference. But this assumption is, in point of fact, as erroneous as it is unwarranted. The problem which we have solved in the preceding sections, interpreted in relation to testimony, is the following. Two witnesses, A and B, assert a fact. The probability of that fact, if we only knew of A's statement, would be p, if we only knew of B's, would be q; what is its probability when we know of both? The formal expression of this problem will be seen in Art. 34. The most complete formal expression of the problem which has been substituted for it, taking into account all its elements, is as follows. Let x and y represent the testimonies of A and B, w and z the facts to which these testimonies respectively relate. Observe that no hypothesis is here made as to the connection, by sameness or difference, of w and z. And the simple absence of any such hypothesis is properly signified by expressing the events by different symbols, unaccompanied by any logical equation connecting these symbols.

If we wish to indicate that the events w and z are identical, we must write as a connecting logical equation,

$$w = z$$

though it must be simpler to express the identity by the employment of a single symbol as before. Any other definite relation may be expressed in a similar way.

The Problem now stands thus:—

$$\text{Given} \quad \left.\begin{array}{ll} \text{Prob. } x = c, & \text{Prob. } xw = cp, \\ \text{Prob. } y = c', & \text{Prob. } yz = c'q, \end{array}\right\} \quad . \quad . \quad . \quad (13)$$

$$\text{Required} \quad \frac{\text{Prob. } xywz}{\text{Prob. } xywz + \text{Prob. } xy\bar{w}\bar{z}} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

First, we will seek the value of Prob. $xywz$.

Let $\quad xw = s, \quad yz = t, \quad xywz = v$

From these logical equations we must now determine v as a developed logical function of x, y, s, and t. The result is

$$v = xyst + 0(xyst + xyt\bar{s} + xy\bar{s}\bar{t} + xs\bar{y}\bar{t} + x\bar{y}\bar{s}\bar{t} + yt\bar{x}\bar{s} + y\bar{x}\bar{s}\bar{t} + \bar{x}\bar{y}\bar{s}\bar{t})$$

$$+ \text{ terms whose coefficients are } \frac{1}{0}.$$

Let u be the value of Prob. v. Then, by the simplification of Art. 21, we have

$$\frac{xyst+xys+xyt+xy+xs+x}{c} = \frac{xyst+xys+xyt+xy+yt+y}{c'}$$

$$= \frac{xyst+xys+xs}{cp} = \frac{xyst+xyt+yt}{c'q}$$

$$= \frac{xyst}{u} = xyst+xys+xyt+xy+xs+x+yt+y+1$$

Equating the product of the third and fourth to that of the fifth and sixth members of the above system, we have

$$u = cc'pq = \text{Prob. } xywz$$

whence $$cc'(1-p)(1-p) = \text{Prob. } xy\bar{w}\bar{z}$$

And hence

$$\frac{\text{Prob. } xywz}{\text{Prob. } xywz + \text{Prob. } xy\bar{w}\bar{z}} = \frac{pq}{pq+(1-p)(1-q)} \quad . \quad . \quad (15)$$

Here it will be noted, that although the arbitrary constants c and c' were necessarily introduced into the expression of the data of problem, they have no place in its solution. The result, it will also be seen, agrees with (8); and it thus shows that that formula would express the true solution of the problem originally proposed, if it were permitted to neglect the circumstance that it is to the *same* fact that the testimonies have reference, and so to regard their agreement as merely an agreement in being true or in being false, but not in being true or in being false about the same thing.

Special Solution of Problem II. founded upon the principle of the limit.

36. In the present investigation we employ the principle stated in Art. 24, our object being to determine the mean between p and q, when they represent probabilities founded upon different judgments, just as in Art. 25 we have determined the mean between p and q, when they represent different observed values of a physical magnitude.

To the previous data, viz.,

$$\text{Prob. } x=c, \quad \text{Prob. } y=c', \quad \text{Prob. } xz=cp, \quad \text{Prob. } yz=c'q \quad . \quad . \quad (1)$$

we now add, as the supposed *à priori* value of Prob. z,

$$\text{Prob. } z=r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From these collective data we determine the fraction

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy} \text{ or } \frac{\text{Prob. } xyz}{\text{Prob. } xyz + \text{Prob. } xy\bar{z}} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

representing the *à posteriori* value of Prob. z, and, equating the *à priori* and *à posteriori* values, determine x. The principle upon which the investigation proceeds, is, that we attribute to the mean strength of the probabilities p and q such a value, that if the mind had previously to the evidence been in the state of expectation which that value is supposed to measure, the evidence would not have

tended to alter that state. By the evidence I mean, of course, that which forms the basis of the judgments.

Making, as before,

$$xz=s \qquad yz=t \qquad xyz=v$$

and determining v as a developed logical function of x, y, z, s, and t, we find

$$v=xyzst+0\,(xy\bar{z}\bar{s}\bar{t}+xzs\bar{y}\bar{t}+yzt\bar{x}\bar{s}+x\bar{y}\bar{z}\bar{s}\bar{t}+y\bar{x}\bar{z}\bar{s}\bar{t}$$
$$+z\bar{x}\bar{y}\bar{s}\bar{t}+\bar{x}\bar{y}\bar{z}\bar{s}\bar{t})$$
$$+\text{terms whose coefficients are } \frac{1}{0}.$$

Hence, availing ourselves of the simplification of Art. 21, we have

$$\frac{xyzst+xy+xzs+x}{c} = \frac{xystz+xy+ytz+y}{c'}$$
$$=\frac{xystz+xsz}{cp} = \frac{xystz+ytz}{c'q}$$
$$=\frac{xystz+xsz+ytz+z}{r} = \frac{xystz}{u}$$
$$=xystz+xy+xsz+ytz+x+y+z+1$$

If we equate the product of the third and fourth to that of the fifth and sixth members of the above system, we find

$$\text{Prob. } xyz = \frac{cc'pq}{r}$$

whence by symmetry,

$$\text{Prob. } xy\bar{z} = \frac{cc'(1-p)(1-q)}{1-r} \quad . \quad . \quad . \quad (4)$$

Substituting these values in (3), we have

$$\frac{\text{Prob. } xyz}{\text{Prob. } xy} = \frac{pq(1-r)}{pq(1-r)+(1-p)(1-q)r} \quad . \quad . \quad . \quad . \quad (5)$$

Before proceeding further, it will be well to note that in this formula p and q represent, not the general probabilities which the testimonies or evidences upon which our judgments are founded would give to the event z, but the probabilities which they would separately produce in a mind embued with a previous expectation of the event z, the strength of which is measured by r. And there are some curious confirmations of the truth of the theorem, two of which I shall notice.

If we represent the *à posteriori* value of Prob. z by R, and accordingly make

$$\frac{pq(1-r)}{pq(1-r)+(1-p)(1-q)r}=\mathrm{R} \quad . \quad . \quad . \quad . \quad (6)$$

we find, on solving the equation relatively to r,

$$\frac{pq(1-\mathrm{R})}{pq(1-\mathrm{R})+(1-p)(1-q)\mathrm{R}}=r \quad . \quad . \quad . \quad . \quad (7)$$

from which it appears, that if r is the *à priori* expectation of an event z, and if evidences are presented which severally would change r to p and q, and unitedly would change it to R; then, reciprocally, if R measured the *à priori* expectation of the event, and evidences were received which would severally change it to p and q, unitedly they would reduce it to r. Now this is evidently what ought to be the case, since testimonies simply countervailing those by which r was changed to R, would simply undo what was done, and again reduce R to r.

We see that p and q being the same, R is greater when r is less, and less when r is greater; and this, though it is contrary to what we might at first expect, is agreeable to reason. For the effect of evidence is to be measured, not by the state of expectation which exists after it has been offered, but by the degree in which the previous state of expectation has been changed by it. Suppose p and q much greater than r, which we will conceive to be a small quantity, then the separate evidences greatly increase the probability of an event which was before very improbable; and unitedly they do this in a much higher degree than if the separate evidences had merely been such as to raise to the measures p and q, an expectation which was before not much below these measures.

Now, introducing the principle of the mean already explained, Art. 24, let us in (6) make $\mathrm{R}=r$, we have

$$\frac{pq\,(1-r)}{pq\,(1-r)+(1-p)\,(1-q)\,r}=r$$

and solving this equation relatively to r, we find

$$r=\frac{\sqrt{pq}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}} \quad . \quad . \quad . \quad . \quad (8)$$

the formula required.

37. Upon this result, the following observations may be made:—

In the first place it may be shown, from the formula itself, that it always expresses a value intermediate between the values p and q. Thus we have

$$\begin{aligned} r-p&=\frac{\sqrt{pq}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}}-p \\ &=\frac{\sqrt{q\,(1-p)}-\sqrt{q\,(1-q)}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}}\times\sqrt{p\,(1-p)} \quad . \quad (9) \end{aligned}$$

on reduction. In like manner we have

$$\begin{aligned} r-q&=\frac{\sqrt{p\,(1-q)}-\sqrt{q\,(1-p)}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}}\times\sqrt{q\,(1-q)} \\ &=-\frac{\sqrt{q(1-p)}-\sqrt{p\,(1-q)}}{\sqrt{pq}+\sqrt{1-p\;1-q}}\times\sqrt{q\,(1-q)} \quad . \quad (10) \end{aligned}$$

As p and q are positive fractions, the values of $r-p$ and $r-q$, given in (9) and (10), are clearly of opposite signs, whence r must lie between p and q.

In the second place, it may be shown, that r approaches more nearly to that one of the two values p and q, which most nearly approaches either of the limits 0 and 1. To show this, let us suppose q greater than p, and let us first inquire, under what circumstances r approaches more nearly to q than to p.

We must then assume

$$q-r<r-p$$

Substituting the values of these members from (9) and (10) we have

$$\frac{\sqrt{q\,(1-p)}-\sqrt{p\,(1-q)}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}}\times\sqrt{q\,(1-q)}<\frac{\sqrt{q\,(1-p)}-\sqrt{p\,(1-q)}}{\sqrt{pq}+\sqrt{(1-p)\,(1-q)}}\times\sqrt{p\,(1-p)}$$

Now, q being by hypothesis greater than p, it is evident that $\sqrt{q\,(1-p)}-\sqrt{p\,(1-q)}$ will be positive. Rejecting, then, the common positive factor on both sides of the inequation we have

$$\sqrt{q\,(1-q)}<\sqrt{p\,(1-p)}$$
$$q-q^2<p-p^2$$
$$q-p<q^2-p^2$$

and dividing both sides by the positive factor $q-p$

$$1<p+q$$
$$\therefore 1-q<p$$

a condition which shows that q must be nearer to 1 than p is to 0.

On the other hand, as would appear from the very same analysis, changing only the signs $<$ into $>$, the condition that r may approach more nearly to p than to q, is that p may be nearer to 0 than q is to 1.

Now, 1 and 0, as limiting the measures of probability of the event z, indicate, the one that it certainly will, the other that it certainly will not occur. And the approach of any measure of probability to these limits indicates the approach of the probability to certainty. We see, then, that when p and q are measures of the probability of an event founded on different judgments, the *mean* between these measures, as determined by (8), will not be the usual arithmetical mean, but will always fall nearer to that one of the two values p and q which expresses a probability the most nearly approaching to certainty.

Now, this seems to be in accordance with reason. Evidence of any kind which enables us to pronounce a judgment with certainty, entirely preponderates over that which only enables us to affirm a probable judgment, Art. 35. And the more of the character of certainty that is possessed, the greater is the weight which is due to the evidence to which it belongs.

38. By an analysis similar to that which is applied in the previous sections, I have determined the general value of r, when the number of judgments is n, and

the values which they respectively give to the probability of the event z are p_1 p_2 . . p_n. The result is

$$r = \frac{\left(p_1 p_2 \cdot \cdot p_n\right)^{\frac{1}{n}}}{\left(p_1 p_2 \cdot \cdot p_n\right)^{\frac{1}{n}} + \left((1-p_1)(1-p_2) \cdot \cdot (1-p_n)\right)^{\frac{1}{n}}} \quad . \quad . \quad (1)$$

This is the general formula of the mean in reference to judgments, and much as it differs from the formula of the mean, in reference to the observations of a physical magnitude, some remarkable points of analogy exist. I will notice but one. The arithmetical mean is not altered if to the quantities among which it is taken we add another equal to the previous mean. Thus we have

$$\frac{p_1+p_2 \cdot \cdot +p_{n+1}}{n+1} = \frac{p_1+p_2 \cdot \cdot +p_n}{n}$$

provided that $p_{n+1} = \frac{p_1+p_2 \cdot \cdot +p_n}{n}$. Or representing $\frac{p_1+p_2 \cdot \cdot +p_n}{n}$ by P_n we have

$$\mathrm{P}_{n+1} = \mathrm{P}_n$$

provided that

$$p_{n+1} = \mathrm{P}_n \quad . \quad . \quad . \quad . \quad (2)$$

The same relation may readily be shown to hold also, if P_n represent the mean of judgment, as expressed in (1).

39. The following is a brief summary of the conclusions established in this paper.

1*st*, The solution of the problem of astronomical observations by the logical theory of probabilities is, in its general form, indefinite.

2*ndly*, It becomes definite, if we introduce the general principle of means. The result is in accordance with the usual formulæ, but expresses the so-called *weights* of the observations as determinate functions of certain probabilities relating to the correctness of the observations, and the character of the observers.

3*dly*, When, as respects the two last elements, the observations are considered equal, the formula is reduced to the expression of the arithmetical mean.

4*thly*, The complete solution of the problem of the combination of two probabilities of an event founded upon different testimonies or judgments is indefinite, but admits, in various cases, of being reduced to a definite form.

5*thly*, This indefiniteness is due to the circumstance indicated by the formula, that the strength of the probabilities in combination is due, not to the strength of the separate probabilities alone, but also to the degree of unexpectedness of the testimonies or judgments themselves.

6*thly*, Combined presumptions, whether for or against an event, are generally strengthened by the unexpectedness of the combination.

7*thly*, When probabilities as $p_1, p_2, \ldots p_n$ are in a high degree cumulative, owing

to the exceeding improbability *à priori* of their combination, the expression for their united force tends to assume the form

$$\frac{p_1 p_2 \cdot \cdot p_n}{p_1 p_2 \cdot \cdot p_n + (1-p_1)(1-p_2) \cdot \cdot (1-p_n)}$$

commonly assumed to express the *general* solution.

8*thly*, When the probabilities are so far from being cumulative that we feel that we ought to take a mean between them, the above formula is replaced by the following, viz.:—

$$\frac{\left\{p_1 p_2 \cdot \cdot p_n\right\}^{\frac{1}{n}}}{\left\{p_1 p_2 \cdot \cdot p_n\right\}^{\frac{1}{n}} + \left\{(1-p_1)(1-p_2) \cdot \cdot (1-p_n)\right\}^{\frac{1}{n}}}$$

9*thly*, This formula takes, in reference to ordinary judgments, the place of the arithmetical mean, with relation to the problem of astronomical observations, both being expressions of a more general principle.

40. It will probably appear to some of the readers of this paper, that I have dwelt more upon questions of philosophy and of language, than it is usual to do in mathematical treatises, and that I have also, in various parts, assumed the office of a critic, rather than that of an expositor of original views. Respecting the first of these points, I will only express a hope, that I have nowhere in this paper entered into discussions that are not strictly relevant to the subject. Upon the second, I have to observe, that the theory of probabilities is one in which as it seems to me, the critical office is especially needed. I do not think that it is likely to gain much advance from mere analysis. As respects the original portions of this paper, it is my strongest wish, that they should be regarded chiefly as materials for future judgment. Thus it is possible that the theory which I have developed with reference to problems of which the elements are logical, may be found to involve inconsistencies as a scientific theory, though I do not think this likely to be the case. But whether that theory shall finally be accepted or not, it is, I conceive, of some present importance, to establish the necessary dependence of *any* theory, professing to deal with the same class of problems, upon what I have termed the conditions of possible experience,—to show how those conditions may be determined, and how they are to be applied. As respects the so-called principle of the mean, applied in certain portions of this paper, it is open to inquiry whether it in all cases leads to results possessing the characteristic property, noted in Art. 38, and the decision of this question would materially affect our estimate of its value. Lastly, it is, I think, highly probable, that conditions which we do not yet know of may be discovered, affecting, not the *possibility* of the data of a problem as discussed in this paper, but their *adequacy*, and the principles which, in statistical research especially, ought to guide us in their selection. I am so conscious how limited, imperfect, and in some cases fluc-

tuating my own views upon important questions connected with this subject are, that I should regret having engaged in inquiries so lengthened and laborious as those of which I now take leave, if I did not think that as materials for future judgment, they may possess value and importance. And although the interest attaching at present to these inquiries is chiefly speculative, it may be that they will yet be found to possess a practical utility. The vast collections of modern statistics seem to demand some kind of reduction. I am sure that all who read this paper will feel that even towards this end I regard the labours of the mathematician as contributing only in a secondary degree.

APPENDIX A.

The following proposition in Algebra is of extreme importance in connection with the theory of probabilities. It was originally published by me in the *Philosophical Magazine* for March 1855; but the present paper would be incomplete without some notice of it.

Proposition.

If V be a rational and integral function of n variables x, y, z . ., involving no power of these variables higher than the first, and having all its coefficients positive, and being complete in all its terms, then if V_x represent that part of V which contains x, V_y that part which contains y, and so on; the system of equations

$$\frac{V_x}{p}=\frac{V_y}{q}\ldots=V \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

p, q, &c. being positive fractions, admits of one solution, and of only one solution, in positive values of x, y, z . .

To exemplify this proposition, let us suppose

$$V=axy+bx+cy+d$$

a, b, c, and d being all greater than 0; then it is affirmed that the system of equations

$$\frac{axy+bx}{p}=\frac{axy+cy}{q}=axy+bx+cy+d \qquad . \quad . \quad . \quad (2)$$

p and q being positive, admits of one, and only one solution, in positive values of x and y.

The proposition is true when $n=1$. For then $V=ax+b$ and the system (1) is reduced to the single equation

$$\frac{ax}{p}=ax+b$$

Whence we have

$$x=\frac{bp}{a(1-p)}$$

and this value is positive if a and b are positive, and p a positive fraction.

The general proof consists in showing, that if the proposition is true for a particular value of n, it is true for the next greater. Whence, being true for the case of $n=1$, it is true universally. I will exemplify the method by showing how the truth of the proposition, when $n=2$ is dependent upon its truth when $n=1$.

Let $n=2$, then we have to consider the system (2), which may be reduced to the form

$$\frac{axy+bx}{axy+bx+cy+d}=p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{axy+cy}{axy+bx+cy+d}=q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Let us represent by Y the variable value of the first member of (4), when x and y are supposed to vary in subjection to the single condition (3). We have then

$$Y=\frac{axy+cy}{axy+bx+cy+d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now differentiating (3) and (5) relatively to x and y, we find, after slight reductions,

$$(ay+b)\,(cy+d)\,dx+(ad-bc)\,xdy=0 \quad . \quad . \quad . \quad . \quad (6)$$

$$dY=\frac{(ad-bc)y}{V^2}dx \quad +\frac{(ax+c)\,(bx+d)}{V^2}dy \quad . \quad . \quad . \quad (7)$$

where, as before, $V=axy+bx+cy+d$. Substituting in (7) the value of dx found from (6), we have

$$dY=\frac{(ax+c)\,(bx+d)\,(ay+b)\,(cy+d)-(ad-bc)^2xy}{(ay+b)\,(cy+d)\,V^2}dy$$

The numerator of this expression may be reduced to the form

$$V\,(abcxy+abdx+acdy+bcd)$$

whence

$$\frac{dY}{dy}=\frac{abcxy+abdx+acdy+bcd}{(ay+b)\,(cy+d)\,V} \quad . \quad . \quad . \quad . \quad (8)$$

This represents the differential coefficient of Y taken with respect to y as independent variable, x being regarded as a function of y determined by (3). The expression is always positive, if x and y are positive.

Now let y vary from 0 to ∞ through the whole range of positive magnitude. Writing (3) in the form

$$\frac{Ax}{Ax+B}=p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where $A=ay+b$, $B=cy+d$, the quantity x must, by reference to the case of $n=1$, have a positive value, since A and B are positive and p fractional. Whence, as y varies from 0 to ∞, the value of $\frac{dY}{dy}$ is always positive.

Now when $y=0$, $Y=0$, and when $y=\infty$, $Y=1$, as is evident from (5). Therefore, as y increases from 0 to ∞, Y continuously increases from 0 to 1. In this variation

it must once, and only once, become equal to q. Wherefore the system (3) (4) admits of one, and only one, solution in positive values of x and y.

The reasoning might also be presented in the following form. The condition of Y having a maximum or minimum value is expressed by the equation

$$abcxy + abdx + acdy + bcd = 0 \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It is obvious that this, as all the terms in the first member are positive, can never be satisfied by positive values of x and y Hence Y has no maximum or minimum, consistently with (3) being satisfied, and thus it never resumes a former value, and is only once, in the course of its variation, equal to q.

In the case of $n=3$, we have

$$V = axyz + byz + cxz + dxy + ex + fy + gz + h$$

and the system to be considered is

$$\frac{axyz + cxz + dxy + ex}{V} = p \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$\frac{axyz + byz + dxy + fy}{V} = q \quad . \quad . \quad . \quad . \quad . \quad (12)$$

$$\frac{axyz + byz + cxz + gz}{V} = r \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Let the first number of the last equation, considered as a variable function of x, y, z be represented by Z, and suppose x, y, and z to vary in subjection to the conditions (11) (12). Just as before, it may be shown that Z increases continuously with z. The condition of Z having a maximum or minimum value, will be expressed by the following equation:

$$\begin{aligned}
&(D+H+E+F)\,(ABC+ACG+ABG+BCG)\\
&+(A+B+C+G)\,(DHE+DHF+DEF+HEF)\\
&+(AC+BG)\,(DF+DH+EF+EH)\\
&+(AG+BC)\,(DF+EH+DE+FH)\\
&+(AB+CG)\,(DE+DH+FE+FH)\\
&+4\,AGFE+4\,BCDH = 0 \quad . \quad . \quad . \quad . \quad . \quad (14)
\end{aligned}$$

Wherein

$$\begin{array}{llll} A=axyz & B=byz & C=czx & D=dxy \\ E=ex & F=fy & G=gz & H=h \end{array}$$

And as this equation has positive values only in its first member, it cannot be satisfied by positive values of x, y, z; whence, by the same reasoning as before, the system (11), (12), (13) cannot have more than one solution in positive values of x, y, z.

To show that it will have one such solution, let z vary from 0 to ∞, then Z continuously increases from 0 to 1, and once becomes equal to r. At every stage of its variation we may give to (11) and (12) the form

$$\frac{Axy+Bx}{V}=p$$

$$\frac{Axy+Cy}{V}=q$$

which corresponds with the form of the general system (3) (4) in the case of $n=2$. Whence, for each positive value of z, one positive set of values of x and y will be found. The system (11), (12), (13) admits, therefore, of one solution in positive values of x, y, z, and of only one.

To prove the proposition generally, it ought to be shown that the function exemplified in the first members of (10) and (14), for the cases of $n=2$ and $n=3$ possesses universally the same property of consisting only of positive terms. I have proved that it does for the case of $n=4$, and the analysis was such as to leave no doubt whatever of its general truth.

I will now offer a few remarks on the application of the above proposition.

The system of equations for determining s and t . ., Art, 21, is of the form

$$\frac{V_s}{p}=\frac{V_t}{q}\ldots=V \qquad (15)$$

V being a function of the same general character as the one discussed in the foregoing proposition, but with this difference, that its coefficients, if we regard it as a complete function, are all equal either to 1 or to 0.

Thus in Art. 18, we have

$$V=stv+s+t+v+1$$

Here the terms st, tv, and vs, must be considered as present, but with the coefficient 0.

This limitation does not affect the essentially positive character of the determining function exemplified in (10) and (14). Whence the system (15) cannot have more than one solution in positive values of s, t, &c. *This shows that the solution of the system of equations furnished by the general method can never be ambiguous.*

The vanishing of some of the coefficients of V does, however, affect the reasoning by which it has been shown, that for the general form of V discussed in the last proposition, one solution of the algebraic system in positive values will exist. Thus Y in (5) does not vanish with y, if both b and d vanish. And generally this vanishing of coefficients in V entails conditions among the quantities p, q, r . ., in addition to that of their being fractional, in order that the derived algebraic system may admit of a solution in positive values.

Thus if we take, as in (7) Art. 18,

$$V=stv+s+t+v+1$$

with the derived algebraic system

$$\frac{stv+s}{V}=p \qquad \frac{stv+t}{V}=q \qquad \frac{stv+v}{V}=r$$

it is evident that if s, t, and v are positive quantities, and if we write

$$\frac{stv}{\mathrm{V}}=u, \qquad \frac{s}{\mathrm{V}}=\lambda, \qquad \frac{t}{\mathrm{V}}=\mu, \qquad \frac{v}{\mathrm{V}}=\nu$$

u, λ, μ, and ν must be positive fractions, whence, in addition to the equations

$$u+\lambda=p$$
$$u+\mu=q$$
$$u+\nu=r$$

we shall have the inequations

$$u \gtreqless 0 \qquad \lambda \gtreqless 0 \qquad \mu \gtreqless 0 \qquad \nu \gtreqless 0$$
$$u+\lambda+\mu+\nu \lesseqgtr 1$$

This system is identical with the one obtained in 10, Art. 13, for the determination of the conditions of possible experience in the particular question of Probabilities, in which the above function V presents itself. And a very little attention will show, that if in any case we express as above the relations which must obviously be fulfilled in order that s, t, &c., may be positive quantities, we shall form a system of equations and inequations precisely agreeing with those which we should have to form in order to obtain the conditions of possible experience, if we sought those conditions, not from the data in their original expression, but from the translated data, as employed in Art. 13.

Hence, in order that s, t . . *in the system of Art.* 21, *may be positive, or in the prior system, positive fractions, the problem of which these systems of equations involve the solution must represent a possible experience.*

Conversely if that problem represent a possible experience, the quantities s, t . . *will admit of being determined in the system of Art.* 21, *in positive values, or in the prior system, in positive fractional values.*

I have not succeeded in obtaining a perfectly rigorous proof of the latter, or converse proposition in its general form, but I have not met with any individual cases in which it was not trne. I will here only exemplify it in Problem II., Art. 34.

Here the value of V is

$$\mathrm{V}=xyst+xs+yt+xy+x+y+1$$

and the algebraic system employed in the determination of Prob. xyz is

$$\frac{xyst+xs+xy+x}{c}=\frac{xyst+yt+xy+y}{c'}$$
$$=\frac{xyst+xs}{cp}=\frac{xyst+yt}{c'q}=\frac{xyst}{u}$$
$$=xyst+xs+yt+xy+x+y+1 \quad . \quad . \quad . \quad (16)$$

For the determination of u we hence find the following equation—

$$u\,(u-\overline{c'+cp-1})\,(u-\overline{c+c'q-1})-(cp-u)\,(c'q-u)\,(u-\overline{cp+c'q-1})=0 \quad (17)$$

the conditions of limitation being

$$u>0 \qquad u>c'+cp-1 \qquad u>c+c'q-1$$
$$u<cp \qquad u<c'q \quad . \quad . \quad . \quad . \qquad (18)$$

Now, since u is greater then $c+c'q-1$ it is *à fortiori* greater then $cp+c'q-1$. Thus within the limits assigned to u, all the factors of each term of (17) will be positive.

If then we give to u the value which belongs to the highest of its inferior limits, the first member of (17) will be reduced to its second term, and will be negative. If we give to u the value which belongs to the lowest of its superior limits, the first member of (17) will be reduced to its first term, and will be positive. Moreover, that member is a quadratic function of u. Hence there is one root, and only one, within the limits specified.

We must now express x, y, s, and t, in terms of u. Their values determined from the system (16) are as follows, viz. :—

$$x=\frac{c(1-p)}{u-(+c'q-1)} \qquad y= \qquad \frac{c'(1-q)}{u-(c'+cp-1)}$$
$$s=\frac{u-(c+c'q-1)}{c(1-p)}\times\frac{u}{c'q-u}$$
$$t=\frac{u-(c'+cp-1)}{c'(1-q)}\times\frac{u}{cp-u}$$

All these expressions become positive when u is determined in accordance with the conditions (18).

It would seem from the above, as well as from reasonings analogous to those of Proposition I., that when the algebraic system belonging to a problem in the theory of probabilities is placed in the form

$$\frac{V_x}{V}=p, \frac{V_y}{V}=q \ . \ .$$

the limits of variation of the first member of any equation subject to the condition, that the variables shall all be positive, and shall vary in subjection to all the other equations of the system, will not in general be 0 and 1, as in the case contemplated in Prop. I., but will correspond with the limits of value of the second member of the same equation as determined by the conditions of possible experience.

This conclusion I have in various cases independently verified. The analytical theory still, however, demands a more thorough investigation.

APPENDIX B.

A note to Archbishop WHATELY's Logic, Book III., sec. 14, contains a rule for computing the joint force of two probabilities in favour of a conclusion which, as actually applied, is at variance with the preceding results. For this reason,

and also because the validity of the rule in question has been made the subject of recent controversy, I design to offer a few remarks upon the subject here. The rule is contained in the following extract. "As, in the case of two probable premises, the conclusion is not established except on the supposition of their being *both* true, so, in the case of two (and the like holds good with any number) distinct and independent indications of the truth of some proposition, unless *both* of them *fail*, the proposition must be true: we therefore multiply together the fractions indicating the probability of *failure* of each,—the chances *against* it;—and the result being the total chances against the establishment of the conclusion by these arguments, this fraction being deducted from unity, the remainder gives the probability *for* it. *E. g.*, A certain book is conjectured to be by such and such an author, partly, 1*st*, from its resemblance in style to his known works, partly (2*dly*), from its being attributed to him by some one likely to be pretty well informed: let the probability of the Conclusion, as deduced from one of these arguments by itself, be supposed $\frac{2}{5}$, and, in the other case $\frac{3}{7}$; then the *opposite* probabilities will be, respectively, $\frac{3}{5}$ and $\frac{4}{7}$; which multiplied together give $\frac{12}{35}$, as the probability *against* the Conclusion; *i.e.*, the chance that the work may *not* be his, notwithstanding those reasons for believing that it is: and consequently the probability in *favour* of that Conclusion will be $\frac{23}{35}$, or nearly $\frac{2}{3}$."

A confusion may here be noted between the probability that a conclusion is proved, and the probability in favour of a conclusion furnished by evidence which does not prove it. In the proof and statement of his rule, Archbishop WHATELY adopts the former view of the nature of the probabilities concerned in the data. In the exemplification of it, he adopts the latter. He thus applies the rule to a case for which it was not intended, and to which it is in fact inapplicable.

The rule is given, and the conditions of its just application are assigned in Professor DE MORGAN'S *Formal Logic*, p. 201. Its origin may be thus explained. Let there be two independent causes, A and B, either of which, when present, necessarily produces an effect E. Let a be the probability that A is present, b the probability that B is present; then $1-a$ is the probability that A is absent, $1-b$ the probability that B is absent, $(1-a)(1-b)$ the probability that they are both absent; finally, $1-(1-a)(1-b)$ the probability that they are not both absent. This, then, is the probability that one at least of the causes is present; and therefore it is the probability that the event E, so far as it is dependent upon these causes, will occur. In its special application to arguments viewed as causes of belief or expectation, it would lead to the following theorem. If there are two independent arguments in favour of a conclusion which the premises of either, if granted, are sufficient to establish, the doubt only existing as to the truth of the premises, and if the probability that the premises of the first argument are true is a, the probability that the premises of the second argument are true b, then the probability that the conclusion is established is $1-(1-a)(1-b)$. Interpreted, this formula gives

Archbishop WHATELY's rule, but the conditions of its valid application are evidently not fulfilled in the example which he has given. To satisfy these conditions, this problem ought to be changed into the following: "There exists a certain quality of style, the possession of which would *prove* the work to be by the author supposed. The probability that the work possesses that quality is $\frac{2}{5}$. There is a person so well informed that his attributing the work to the supposed author would be *conclusive.* The probability that he does attribute it to the author in question is $\frac{3}{7}$. Required the probability, on these grounds, that the supposed author is the real one." But this is evidently not the sense in which the problem was meant to be understood. Thus to take one point, it is not the quality of the style that is a matter of probability, but the mode in which a known and observed quality affects the question of authorship.

Taking the problem in its intended meaning, each of the fractions $\frac{2}{5}$, $\frac{3}{7}$, measuring, not the probability of the truth of certain premises, but the probability drawn from these premises, as conditions, in favour of a certain supposition (I use this word in preference to conclusion), we are no longer permitted to apply the formula above determined. And we are not permitted to do so, because the probabilities with which we are concerned are conditional, and their possession of this character greatly increases the difficulty of the problem. Its rigorous formal solution is given in Art. 34, and shows that the probability sought is, generally speaking, indefinite,—a result which agrees with the conclusions of Bishop TERROT, by whom the error to which attention has been directed was first pointed out, *Transactions of the Royal Society of Edinburgh,* vol. xxi., p. 369.

I trust that I have not in any way misrepresented Archbishop WHATELY's reasoning; and I am the more encouraged to believe that I have not, as a defence of it which appeared in the *United Church Journal* expressly proceeds upon the assumption that the probabilities with which we are concerned are probabilities that the authorship is *proved.* To this view Bishop TERROT justly demurred. Nor was its inconsistency materially diminished by assigning to proof a meaning less absolute than belongs to demonstration. For whatever degree of cogency,—of power to produce conviction,—we suppose to characterize proof, the thing itself belongs to consciousness, and the question whether given evidence is sufficient to convey proof to our minds or not, is a matter of knowledge, not of probability.

NATURAL HISTORY

PROCEEDINGS

OF THE

STATUTORY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE NOVEMBER 22, 1852;

WITH

LIST OF DONATIONS TO THE LIBRARY,

FROM DEC. 5, 1853, TILL APRIL 20, 1857.

PROCEEDINGS, &c.

Monday, November 28, 1853.

At a Statutory General Meeting, Dr CHRISTISON, V.P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,
Very Rev. Principal LEE,
Right Rev. Bishop TERROT,
Dr CHRISTISON,
Dr ALISON,
Hon. Lord MURRAY,
} Vice-Presidents.

Professor FORBES, General Secretary,

Dr GREGORY,
Professor SMYTH,
} Secretaries to the Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library and Instruments.

JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

ROBERT CHAMBERS, Esq.	ANDREW COVENTRY, Esq.
J. T. GIBSON-CRAIG, Esq.	Rev. Dr JAMES GRANT.
WILLIAM SWAN, Esq.	Rev. Professor KELLAND.
Prof. WILLIAM THOMSON.	Dr GEORGE WILSON.
Dr J. H. BENNETT.	CHARLES MACLAREN, Esq.
Dr J. H. BALFOUR.	Rev. Dr ROBERT LEE.

The following Committee was appointed to audit the Treasurer's accounts:—

J. T. GIBSON-CRAIG, Esq. ANDREW COVENTRY, Esq. JAMES CUNNINGHAM, Esq.

The Meeting then adjourned.

(Signed) JOHN LEE, *V.P.*

Memorandum.—November 28, 1853.—At a Statutory General Meeting of this date, Dr CHRISTISON, V.P., made a preliminary statement on the part of the Council to the following effect:—

That, as it appeared from a recent correspondence, that Professor FORBES considered his former resignation of the office of General Secretary to stand good without any formal renewal of it, the Council had taken into consideration the most advisable course of proce-

dure to be followed in consequence of Professor FORBES's continued indisposition; and having been apprised that such an improvement had taken place in Professor FORBES's health as to warrant strong hopes of his being able to return to Edinburgh, and to business, in the course of next year; and being also well satisfied with the manner in which the duties of the Secretaryship have been discharged in his absence, the Council had resolved to recommend to the Society that the arrangement of the past Session should be continued for another year.

That the Council having been further informed that Professor FORBES had intimated that he could not accept the usual salary of office for last year, they resolved also to recommend that Mr WILSON be requested to accept the said salary for his services during the preceding Session, and to grant the continuance of these services for the present Session upon the same terms.

The above recommendations were then unanimously adopted and confirmed by the General Meeting of the Society.

Memorandum.—December 19, 1853.—At the Ordinary Meeting of this date, the Acting General Secretary, on the part of the Council, stated that Dr GEORGE BUIST of Bombay had been duly elected a Fellow of the Society in the Session of 1845–6, but had not been enrolled in consequence of his absence in India, where he had, until very recently, resided; that anticipating only a short residence in India after his intended return thither, he was desirous to become a resident Fellow of the Society, and that the Council, having taken into consideration the circumstances of the case, had agreed to recommend that Dr BUIST be enrolled on payment of the usual admission-money, and the annual subscription for the current Session, without the exaction of arrears.

Whereupon it was moved by Dr GREVILLE, seconded by JAMES CUNNINGHAM, Esq., and unanimously agreed to, "That Dr BUIST having been duly elected a Fellow of this Society in the Session of 1845–6, during his absence in India, be now received as an Ordinary Resident Member on payment of his entrance money, and annual contribution for the current Session."

Monday, November 27, 1854.

At a Statutory General Meeting, Principal LEE, V.P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,
Very Rev. Principal LEE,
Right Rev. Bishop TERROT,
Dr CHRISTISON,
Dr ALISON,
Hon. Lord MURRAY,
} Vice-Presidents.

Professor FORBES, General Secretary.

Dr GREGORY,
Dr BALFOUR,
} Secretaries to the Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.
Dr TRAILL, Curator of Library and Instruments.
JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

Dr J. H. BENNETT.	Rev. Dr ROBERT LEE.
ANDREW COVENTRY, Esq.	Professor C. PIAZZI SMYTH.
Rev. Dr JAMES GRANT.	Hon. B. F. PRIMROSE.
Rev. Professor KELLAND.	Sir WILLIAM GIBSON-CRAIG, Bart.
Dr GEORGE WILSON.	Major EDWARD MADDEN.
CHARLES MACLAREN, Esq.	JAMES CUNNINGHAM, Esq.

The following Committee was appointed to audit the Treasurer's accounts:—

ANDREW COVENTRY, Esq. JAMES WALKER, Esq., W.S. WILLIAM THOS. THOMSON, Esq.

The Meeting then adjourned.

(Signed) R. CHRISTISON, *V.P.*

Monday, November 26, 1855.

At a Statutory General Meeting, Dr CHRISTISON, V.P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.
Sir D. BREWSTER. K.H.,
Very Rev. Principal LEE,
Right Rev. Bishop TERROT,
Dr CHRISTISON,
Dr ALISON,
Hon. Lord MURRAY,
} Vice-Presidents.
Professor FORBES, General Secretary.
Dr GREGORY,
Dr BALFOUR,
} Secretaries to the Ordinary Meetings.
JOHN RUSSELL, Esq., Treasurer.
Dr TRAILL, Curator of Library and Instruments.
JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

Dr GEORGE WILSON.	Sir WILLIAM GIBSON-CRAIG, Bart.
CHARLES MACLAREN, Esq.	JAMES CUNNINGHAM, Esq.
Rev. Dr ROBERT LEE.	Dr GREVILLE.
Professor C. PIAZZI SMYTH.	A. KEITH JOHNSTON, Esq.
Hon. B. F. PRIMROSE.	Dr MACLAGAN.
Colonel MADDEN.	WILLIAM SWAN, Esq.

The following Committee was appointed to audit the Treasurer's accounts:—

J. T. Gibson-Craig, Esq. David Smith, Esq. James Cunningham, Esq.

The Meeting then adjourned.

(Signed) John Lee, *V.P.*

Memorandum.—November 26, 1855.—At a Statutory General Meeting of this date, Professor Christison, V.P., in the Chair, the following Resolution was moved by the Treasurer, and unanimously adopted:—

"1. That it having been reported to the Royal Society by the Council, that Sir Thomas M. Brisbane, the venerable and respected President of the Society, has instituted a Prize, to be called "The Makdougall Brisbane Prize," to be awarded by the Council of the Society, in such manner and for such purposes as shall be deemed most expedient for the promotion and advancement of science, the Society takes the opportunity of the first meeting of the session to record the great satisfaction with which it receives this proof of Sir T. M. Brisbane's continued zeal to promote the interests of science, and its grateful sense of the confidence reposed by him in the Society, by conferring on the Council such large and discretionary powers as to the mode of awarding the prize.

"The Society has always viewed with pleasure the connection which has so long subsisted between it and Sir T. M. Brisbane as its President, from which this Society has derived so much benefit and so much honour; and it will henceforth consider the "Makdougall Brisbane Prize" as a permanent and pleasing memorial of that connection.

"2. That a copy of the above resolution be sent to Sir Thomas M. Brisbane."

Monday, November 24, 1856.

At a Statutory General Meeting, Principal Lee, V.P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. Makdougall Brisbane, Bart., G.C.B., G.C.H., President.

Sir D. Brewster, K.H.,
Very Rev. Principal Lee,
Right Rev. Bishop Terrot,
Dr Christison,
Dr Alison,
Hon. Lord Murray,
} Vice-Presidents.

Professor Forbes, General Secretary.

Dr Gregory,
Dr Balfour,
} Secretaries to the Ordinary Meetings.

John Russell, Esq., Treasurer.

Dr Douglas Maclagan, Curator of Library and Museum.

COUNSELLORS.

Hon. B. F. Primrose.	Dr Traill.
James Cunningham, Esq.	Hon. Lord Neaves.
Dr Greville.	Dr Thos. Anderson, Glasgow.
A. Keith Johnston, Esq.	Rev. Dr Hodson.
Dr Maclagan.	Robert Chambers, Esq.
Wm. Swan, Esq.	J. T. Gibson-Craig, Esq.

The following Committee was appointed to audit the Treasurer's Accounts:—

J. T. Gibson-Craig, Esq. John Mackenzie, Esq. James Cunningham, Esq.

The Meeting then adjourned.

Memorandum.—January 5, 1857.—At the ordinary Meeting of this date, Dr Christison, in the absence of Mr Russell, the Treasurer, proposed that the following motion, tabled on the 1st December, be carried:—

That the Meeting do resolve that Laws II., III., and IV., and Law XVI., be altered and stand as follows:—[The alterations are printed within brackets.]

Law II.

Every Ordinary Fellow, within three months after his election, shall pay [*Two*] Guineas as the fee of admission, and Three Guineas as his contribution for the Session in which he has been elected—and annually, at the commencement of every Session, the like sum of Three Guineas; [that this annual contribution shall continue for *ten* years after his admission, and thereafter it shall be limited to *Two* Guineas for fifteen years thereafter.]

Law III.

All Fellows who shall have paid *twenty-five* years' annual contribution shall be exempt from farther payment.

Law IV.

The fees of admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual annual contribution of £3, 3s. payable by each Resident Fellow, but after payment of such annual contribution for eight years he shall be exempt from any further payment.

[In the case of any Resident Fellow ceasing to reside in Scotland, and wishing to continue a Fellow of the Society, it shall be in the power of the Council to determine on what terms, in the circumstances of each case, the privilege of remaining a Fellow of the Society shall be continued to such Fellow while out of Scotland.]

Law XVI.

That the words "and an Assistant Curator," in this Law, shall be omitted.

The motion was seconded by Professor More, and was carried, it being recommended to the Council to take means of ensuring that all existing Members shall benefit by the change in the entrance fee and annual contribution after ten years, according to their period of Membership.

Memorandum.—February 2, 1857.—At the Ordinary Meeting of this date, it was announced from the chair that Mr Russell desired to be relieved from the office of Treasurer.

The Meeting received Mr Russell's resignation with the greatest regret; and resolved that a cordial acknowledgment of his long continued and zealous services to the Society be entered on the Minutes.

Memorandum.—February 16, 1857.—At the Ordinary Meeting of this date, Dr Christison, V.P., the Chairman, stated that the Council had, according to the recommendation made at last Meeting, considered the subject of Mr Russell's retirement, and had resolved to propose to the Society the following motions:—

"1. That Mr Russell's letter of resignation be recorded in the Minutes."

This motion was unanimously agreed to, and the letter follows :—

Sir,—I have now held the office of Treasurer of the Royal Society for eighteen years, and in the course of next month I shall enter on the 78th year of my age. It seems to me, therefore, full time that the duties of that office should be devolved on some younger and more active Fellow of the Society. I therefore now beg to resign that office, and request you will lay this letter of resignation before the next Meeting of Council.—I am, &c. (Signed) John Russell.

"2. That the following resolution, conveying the thanks of the Society to Mr Russell, be recorded in the Minutes."

This was carried unanimously, and the resolution follows :—

Resolved, That this Meeting do record their deep sense of the zealous and important services rendered to the Royal Society by its late Treasurer, Mr Russell. During the long period of eighteen years for which he has held that office, the funds of the Society have been in a steadily prosperous condition. The benefit of Mr Russell's cordial and effective assistance has been felt not only in this, but in every department of the Society's affairs, and that assistance the Meeting hope may still long continue, notwithstanding Mr Russell's withdrawal from the responsibility of his official position.

"3. That Mr James Gibson-Craig be elected Treasurer, in room of Mr Russell resigned."

This motion was carried unanimously.

Memorandum.—March 2, 1857.—At the Ordinary Meeting of this date, Mr D. Smith made the following motion, which was seconded by Mr Nasmyth, and unanimously adopted :—

"That the Council be requested to consider the propriety of recording in a fitting and permanent manner the feelings of the Society towards their late Treasurer, Mr Russell, for the benefit which the Society has received from his attention and exertions during the time he has acted as Treasurer, and to report their opinion and the manner in which they think such a record should be most appropriately made."

Memorandum.—April 20, 1857.—At the Ordinary Meeting of this date, Dr Fleming moved, and Mr Syme seconded, the following resolutions, of which notice had been given at the previous Ordinary Meeting :—

"1. That the Council has exceeded its powers in reference to the Neill Prize, by determining and announcing the conditions of appropriation before submitting the case for the approval of the Society.

"2. That the conditions announced by the Council are framed with the view of enhancing the importance and promoting the interest of the Society, rather than the recognition of merit, contemplated in a liberal spirit, and so unequivocally indicated by the terms of the bequest.

"3. That the subject be remitted to the Council for reconsideration, with an injunction that the results be laid before the Society for approval, previous to publication."

The Rev Dr Grant moved, and Mr D. Smith seconded, the following amendment :—

"That the Society sees no ground for disturbing the arrangements for the appropriation of the Neill Bequest, as announced by the Council, and expresses its approval of said arrangements."

On the vote being taken, 32 voted for the amendment, and 18 for the motion.

The Chairman declared the amendment to be carried.

Memorandum.—April 20, 1857.—At the Ordinary Meeting of this date, it was proposed on the part of the Council (in consequence of a remit to them from the Society on the 2d March), that a sum not exceeding Fifty Guineas, should be set apart from the funds of the Society for the purchase of a piece of Plate to be presented to Mr Russell, the late Treasurer, in the name of the Society, and in acknowledgment of his long-continued and valuable services ; a notice to this effect having been made at last Ordinary Meeting.

This motion was carried by acclamation.

MEMBERS ELECTED.

April 18, 1853.

Hugh Scott, Esq. of Gala.

December 5, 1853.

Græme Reid Mercer, Esq.

December 19, 1853.

Dr George Buist, Bombay.

January 3, 1854.

Sir John Maxwell, Bart.

January 16, 1854.

William Murray, Esq. of Monkland.

February 20, 1854.

Dr John Addington Symonds, Clifton, Bristol.

April 3, 1854.

Henry Dunlop, Esq. of Craigton.

April 17, 1854.

Dr William Bird Herapath, Bristol. Professor Robert Harkness, Queen's College, Cork.

December 4, 1854.

Dr Thomas A. Wise. Dr James Coxe.

December 18, 1854.

Ernest Bonar, Esq.

January 2, 1855.

James P. Fraser, Esq.

February 5, 1855.

Dr Stevenson Macadam.

February 19, 1855.

Robert Etheridge, Esq., Clifton, Bristol. John Inglis, Esq., Dean of Faculty.
Rev. James S. Hodson, M.A.

March 19, 1855.

Dr Wyville T. C. Thomson, Professor of Geology, Belfast.

April 2, 1855.

Sir Robert K. Arbuthnot, Bart.

April 30, 1855.

Dr Wright, Cheltenham.

December 3, 1855.

James Hay, Esq. R. M. Smith, Esq.

January 7, 1856.

David Bryce, Esq.

January 21, 1856.

William Mitchell Ellis, Esq. Dr George J. Allman.

February 4, 1856.

Hon. Lord Neaves. Dr Frederick Penny.

February 18, 1856.

Dr Thomas Laycock, Professor of Medicine.

March 17, 1856.

Thomas Cleghorn, Esq., Advocate.

April 21, 1856.

James Clerk Maxwell, Esq., Professor of Natural Philosophy, Marischal College, Aberdeen.

January 5, 1857.

Horatio Ross, Esq. Dr James Black.

February 2, 1857.

Dr John Ivor Murray.

February 16, 1857.

John Melville, Esq., W.S. John Blackwood, Esq.
Brinsley De Courcy Nixon, Esq.

March 2, 1857.

Andrew Murray, Esq. of Conland. Rev. Dr James Macfarlane, Duddingston.
Dr W. M. Buchanan.

April 6, 1857.

Thomas Login, Esq., C.E., Pegu.

LIST OF THE PRESENT ORDINARY MEMBERS,

IN THE ORDER OF THEIR ELECTION.

General Sir THOMAS M. BRISBANE, Bart., G.C.B., &c., F.R.S. Lond.,

PRESIDENT.

Date of Election.

1798 Alexander Monro, M.D.

1808 James Wardrop, Esq., London.

Sir David Brewster, K.H., LL.D., F.R.S., Lond., *St Andrews.*

1811 General Sir Thomas Makdougall Brisbane, Bart., G.C.B., G.C.H., F.R.S. Lond.

James Jardine, Esq., *Civil Engineer.*

Alexander Gillespie, Esq., *Surgeon.*

1812 James Pillans, Esq., *Professor of Humanity.*

Sir George Clerk, Bart., F.R.S. Lond.

1813 William Somerville, M.D., F.R.S. Lond.

1814 Right Honourable Viscount Arbuthnot.

John Fleming, D.D., *Professor of Natural Science, New College.*

1815 Henry Home Drummond, Esq., *of Blair-Drummond.*

William Thomas Brande, Esq., F.R.S. Lond., *Professor of Chemistry in the Royal Institution.*

1816 Leonard Horner, Esq., F.R.S. Lond.

1817 Alexander Maconochie Wellwood, Esq., *of Meadowbank.*

William P. Alison, M.D., *Emeritus Professor of the Practice of Physic.*

Robert Bald, Esq., *Civil Engineer.*

1818 Patrick Miller, M.D., *Exeter.*

John Watson, M.D.

Right Honourable John Hope, *Lord Justice-Clerk.*

1819 Patrick Murray, Esq., *of Simprim.*

Thomas Stewart Traill, M.D., *Professor of Medical Jurisprudence.*

Alexander Adie, Esq.

Date of Election.

1819 George Forbes, Esq.

1820 James Keith, M.D., *Surgeon.*
Charles Babbage, Esq., F.R.S. Lond.
Sir John F. W. Herschel, Bart., F.R.S. Lond.
John Shank More, Esq., *Professor of Scots Law.*
Dr William Macdonald, *Professor of Natural History, St Andrews.*
Sir John Hall, Bart., *of Dunglass.*

1821 Sir James M. Riddell, Bart., *Strontian.*
John Lizars, Esq., *Surgeon.*
John Cay, Esq., *Advocate.*
Robert Kaye Greville, LL.D.
Robert Hamilton, M.D.

1822 James Smith, Esq. *of Jordanhill*, F.R.S. Lond.
William Bonar, Esq.
George A. Walker-Arnott, LL.D., *Professor of Botany, Glasgow.*
Very Rev. John Lee, D.D., *Principal of the University of Edinburgh.*
Sir James South, F.R.S. Lond.
Sir W. C. Trevelyan, Bart., *Wallington, Northumberland.*
John Russell, Esq., P.C.S.

1823 Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Andrew Fyfe, M.D., *Professor of Medicine and Chemistry, King's College, Aberdeen.*
Robert Bell, Esq., *Advocate.*
Admiral Norwich Duff.
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq., *of Banchory.*
Liscombe John Curtis, Esq., *Ingsdon House, Devonshire.*
Robert Christison, M.D., *Professor of Materia Medica.*
John Gordon, Esq., *of Cairnbulg.*

1824 Robert E. Grant, M.D., *Professor of Comparative Anatomy, University College, London.*
Rev. Dr William Muir, *one of the Ministers of Edinburgh.*
James Pillans, Esq.
James Walker, Esq., *Civil Engineer.*
William Wood, Esq., *Surgeon.*

1825 Honourable Lord Wood.

1826 Sir David Hunter Blair, Bart., *Blairquhan, Ayrshire.*

1827 John Gardiner Kinnear, Esq.
James Russell, M.D.
Very Rev. Edward Bannerman Ramsay, A.M., Camb.

1828 Erskine Douglas Sandford, Esq., *Advocate.*
David Maclagan, M.D.
Sir William A. Maxwell, *of Calderwood*, Bart.
John Forster, Esq., *Architect, Liverpool.*
Thomas Graham, A.M., *Professor of Chemistry, London University.*
David Milne Home, Esq., *Advocate.*

Date of Election.

1828 Dr Manson, *Nottingham.*

1829 A. Colyar, Esq.

Sir William Gibson-Craig, Bart., *of Riccarton.*

Right Honourable Duncan M'Neill, *Lord Justice-General.*

Venerable Archdeacon Sinclair, *Kensington.*

Arthur Connell, Esq., *Professor of Chemistry, St Andrews.*

James Walker, Esq., W.S.

1830 J. T. Gibson-Craig, Esq., W.S.

Sir Archibald Alison, Bart., *Sheriff of Lanarkshire.*

Honourable Mountstuart Elphinstone.

James Syme, Esq., *Professor of Clinical Surgery.*

Thomas Barnes, M.D., *Carlisle.*

1831 James D. Forbes, D.C.L., F.R.S. Lond., *Professor of Natural Philosophy.*

Right Honourable Lord Dunfermline.

David Boswell Reid, M.D., *London.*

1832 John Sligo, Esq., *of Carmyle.*

William Gregory, M.D., *Professor of Chemistry.*

Robert Allan, Esq., *Advocate.*

Robert Morrieson, Esq., *Hon. E.I.C. Civil Service.*

Montgomery Robertson, M.D.

1833 Captain Milne, R.N.

His Grace the Duke of Buccleuch, K.G., *Dalkeith Palace.*

David Craigie, M.D.

Sir John Stuart Forbes, Bart., *of Pitsligo.*

Alexander Hamilton, LL.B., W.S.

Right Honourable Earl Cathcart.

1834 Mungo Ponton, Esq., W.S., *Clifton, Bristol.*

Isaac Wilson, M.D., F.R.S. Lond.

Professor Low.

Patrick Boyle Mure Macredie, Esq., *Advocate, of Piercetown.*

John Davies Morries Stirling, Esq.

Thomas Jameson Torrie, Esq.

John Haldane, Esq., *Haddington.*

William Sharpey, M.D., *Professor of Anatomy, University College, London.*

1835 John Hutton Balfour, M.D., A.M., F.R.S. Lond., *Professor of Botany.*

Right Honourable Lord Campbell.

William Brown, Esq., F.R.C.S.

R. Mayne, Esq.

1836 David Rhind, Esq., *Architect.*

Archibald Robertson, M.D., F.R.S. Lond.

1837 John Archibald Campbell, Esq., W.S.

John Scott Russell, Esq., A.M , *London.*

Charles Maclaren, Esq.

Archibald Smith, Esq., M.A., Camb., *Lincoln's Inn, London.*

Date of Election.

1837 Richard Parnell, M.D.
Peter D. Handyside, M.D., F.R.C.S.

1838 Thomas Mansfield, Esq., *Accountant.*
Alan Stevenson, Esq., *Civil Engineer.*

1839 David Smith, Esq., W.S.
Adam Hunter, M.D.
Rev. Philip Kelland, A.M., *Professor of Mathematics.*
William Alexander, Esq., W.S.
F. Brown Douglas, Esq., *Advocate.*
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